SCIENTIFIC AMERICAN SUPPLEMENT No. 607

Scientific American Supplement, Vol. XXIV., No. 607.
Scientific American, established 1845.

NEW YORK, AUGUST 20, 1887.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.

FOUNDATIONS OF THE GREAT TOWER AT PARIS.

SOME very interesting details upon the construction of the 984 foot tower have recently been communicated to the Society of Civil Engineers by Mr. Eiffel. We herewith reproduce the latter's description, along with a few complementary details collected on a visit to the works, made on the kind invitation of the great engineer.

The Subsoil.—It results from numerous borings made in the Champ de Mars that the lower stratum of the subsoil consists of a bed of plastic clay, about fifty feet in thickness, resting upon chalk. This clay is dry and quite compact, and capable of supporting a weight of over fifty pounds to the square inch. The stratum slopes slightly from the Military School to the Seine, and is surmounted by a bank of sand and compact gravel, well adapted for supporting foundations. As far as to the vicinity of the fence that separated the Champ de Mars properly so called, belonging to the state, from the square belonging to the city, that is to say, nearly to the height of Grenelle street, this stratum of sand and gravel has a nearly constant thickness of from 20 to 23 feet. Beyond, we seem to enter the old bed of the Seine, and the action of water has reduced the thickness of the stratum, which continually diminishes to almost nothing when we reach the actual bed.

The solid bed of sand and gravel is itself surmounted by a variable thickness of fine sand, muddy sand, and filling-in of all kinds, not fit to support foundations.

As certain administrative considerations forbade the erection of the tower in the states portion of the Champ de Mars, where the foundations presented no difficulty, it was afterward decided to locate it on the Seine quay, in order that it should be as far as possible from the exhibition buildings; but a knowledge of the subsoil demonstrated that this would be impossible, since the direct founding of so large a structure on clay could not be thought of.

At my instance, it was therefore decided to place it at the extreme limit of the square, where it is now building. The foundations of each of its four columns are thus separated from the clay by a sufficient thickness of gravel.

Foundation.—The two back piers, which are numbered 2 and 3, are placed on each side of the line of the old fence. The natural soil at this point is at the level + 34, the filling-in of all kinds is 23 feet deep, and we meet at the level + 27, which is the normal level of the Seine, the stratum of sand and gravel, the thickness of which is here about nineteen feet. It has thus been possible to obtain very easily a perfect foundation for these two piers, the lower portion of which consists of a stratum of beton 6½ feet in thickness.

The two front piers, which are numbered 1 and 4, were differently constructed. The stratum of sand and gravel was met only at the level + 22, that is to say, 16 feet under water, and, in order to reach it, it was necessary to pass through muddy and marly earth derived from the recent alluviums of the Seine. In order to obtain precise data, free from the uncertainties that accompany the ordinary processes of boring, we made a bore hole in the center of each of the pier places by means of compressed air and an iron plate cylinder 5 feet in diameter, surmounted by a hoist. We cannot too greatly recommend this process, which, on the whole, is not very expensive when compressed air apparatus is at one's disposal, and which gives absolutely sure information as to the consistency and actual composition of the ground. In this way, we ascertained that as far as to the clay we would meet under the sand and gravel with nothing but pure sand, ferruginous loam, and a bank of chloritic limestone, that had formed at the bottom of the depression made by the water in the plastic clay. We had thus an incompressible stratum of a thickness of more than 10 feet at pier 4 (Grenelle side) and of nearly 20 feet at pier 1 (Paris side). We therefore had every security, and the more so in that the foundations had been so calculated that the maximum pressure upon the earth, even including the effect of the wind, would not exceed about fifty-seven pounds to the square inch.

In the construction of these two piers, iron caissons had to be used. These are four in number for each pier, and are 49¼ feet long by 19¾ feet wide, and are sunk to a depth of 16 feet below water. It would have been possible, perhaps, to use a different method, and to have filled in an excavated space with beton; but, on the one hand, despite all the previous borings, we were not sure enough of the ground in this much disturbed part of the Champ de Mars at all points of the surface embraced by the columns, and which is nearly 11,000 square feet per column. Under such circumstances, it was therefore necessary to adopt a solution that responded to every contingency. Moreover, the use of compressed air is accompanied with such security, either as regards work or the certainty of the result obtained, that, by reason of the great interest that we had in proceeding as quickly as possible and in establishing foundations for which there should be absolutely no fear in the future, we did not hesitate to employ this costly but sure and quick compressed air process. We have to congratulate ourselves for it so much the more in that we are now meeting in the caissons of pier 1 large quantities of masonry that probably date from some preceding exhibition, and that would have proved a serious obstacle to the prompt construction of our foundations had not the resources of compressed air been at our disposal. This material we break up with chisels and remove through the air chamber in the same manner that we do the ordinary rubbish.

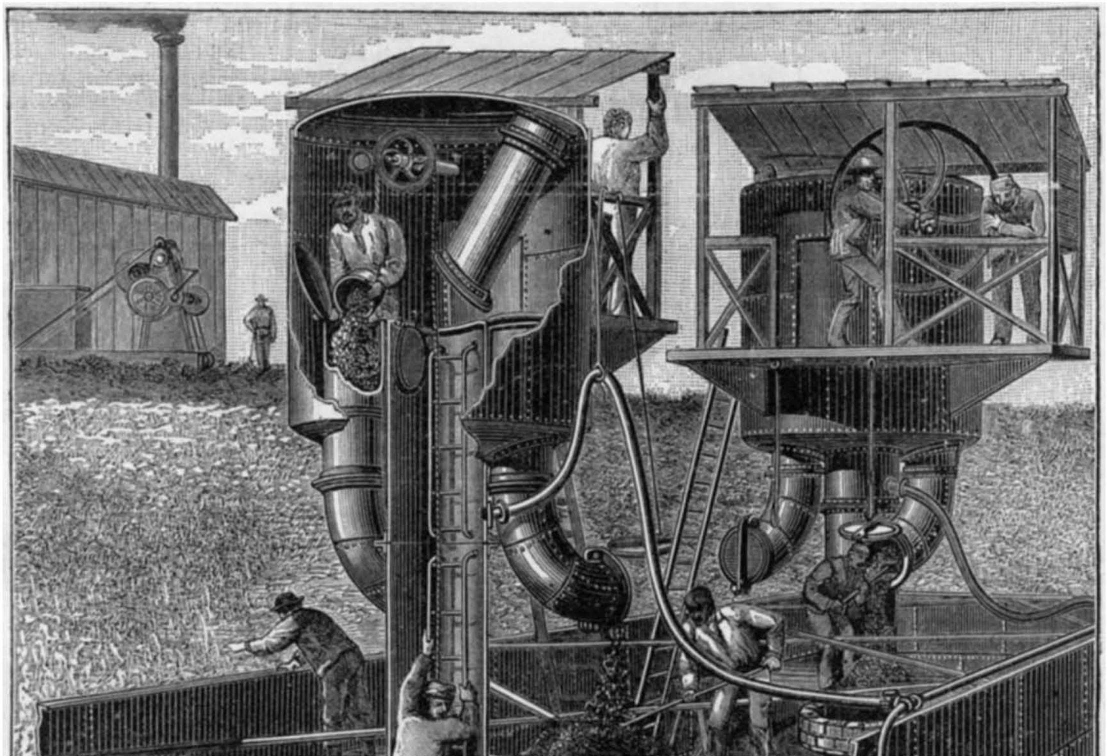


FIG. 2.—THE GREAT TOWER AT PARIS—SECTION OF ONE OF THE CAISSONS.

Foundation Masonry and External Walls.—Each of the four columns will, as well known, consist of a large framework of 49 × 49 feet section, whose corners will transmit the pressures to the earth through the intermedium of masonry piers placed under them. There are therefore four masonry piers for each column. The upper part of these, which receives the bed plate, is at right angles with the direction of the corners, and the pier itself has the form of a pyramid with a vertical face in front and a sloping one behind, the dimensions of which are such that they will bring the oblique resultant of the pressures to a point very near the center of the foundation.

This oblique reaction of the pressures amounts on its entrance into the masonry, at the level + 36, to 565 tons without the wind and to 875 tons with it. Upon the foundation soil of the two piers near the Seine, which is at a depth of 46 feet, the vertical pressure upon the earth is 3,320 tons with the wind, and this distributed over a surface of 967·5 square feet gives a load of about fifty-seven pounds to the square inch.

Upon the two piers toward Paris, the pressure upon the earth at a depth of 29·5 feet is 1,970 tons, which distributed over a surface of 645 square feet gives a pressure of about fifty-seven pounds to the square inch.

The masses of beton that afford such surface are 32·8 feet in length by 19 6 in width. All the beton work is arranged according to the horizontal projection of the corners, that is to say, at 45° with respect to the axis of the Champ de Mars. The masonry is of Souppes stone laid in cement. The use of this latter was necessitated by the desire to obtain a very rapid setting and to prevent any settling. Into the center of each pier are inserted two large anchorage bolts, 22 feet in length and 4 inches in diameter, which, through the intermedium of iron bed plates and I-irons, will connect the superstructure with the masonry. Such anchorage, which is not necessary for the stability of the tower (which is secured by its own weight), nevertheless affords an excess of security against overturning and, moreover, will be utilized when the erection of the superstructure takes place. The masonry will be capped with two courses of Chateau-Landon dressed stone, of which the resistance to crushing is, according to experiments, about fifteen thousand pounds to the square inch. The pressure under the bed plates will be but 462 pounds to the square inch, and the stone will be taxed to but a fortieth of its resistance.

It results from all the figures and data that precede that these foundations are established under conditions of very great security, and that, either as regards selection of materials or dimensions, they have been treated very broadly, so as to insure their solidity.

Yet, in order to be entirely sure that, whatever happens, the columns of the tower shall remain in a perfectly horizontal plane, we have formed a space in the bed plates for the reception of an 800 ton hydraulic press, by means of which the column could be lifted to the necessary height and wedged up with steel. These presses will, then, at any moment (if it ever becomes necessary) effect an accurate leveling of all the bearing points, after the manner of an adjusting screw.

Besides the piers, we have projected a masonry wall which will bear no load, but is designed to receive the finials of the metallic mouldings that are to go on the base of the columns.

These walls are supported by pillars and arches arranged with their faces parallel or at right angles with the axis of the Champ de Mars, and form around each base a structure 26 feet square.

All this infrastructure will be buried up to the level of the soil, except pier 3, where a cellar will be left for the reception of the engines and generators for running the elevators. The engines will be of 500 horse-power. Atmospheric electricity will be carried off through the earth by two cast-iron pipes, 18 inches in diameter, for each pier, sunk beneath the level of the aquiferous stratum, and having a length of 59 feet. At their extremity these pipes are bent vertically and run to the level of the earth, where they will be put in direct communication with the metallic part of the tower.

State of the Work.—The excavating was begun with a ditch 11½ feet square for each of the piers. These were dug to a depth of 19½ feet for piers 2 and 3, and to 23 feet for piers 1 and 4. About 4,236,000 cubic feet of material has been carried to the public dump heap, while the rest has been spread upon the square and will be used for filling in the excavations after the masonry is all finished. The first stroke of the pick was given on the 28th of January. Since then we have finished the masonry of piers 2 and 3. Nothing remains but to give a few finishing touches, which will not be possible until after the filling in, which is now under way. Piers 1 and 4 are in the following state of advancement: At pier 4 three of the caissons have been sunk to the level +22, and the foundation masonry that they support is nearly finished; the fourth caisson is still sinking. In eight days this pier will be finished. At pier 1 two of the caissons are sinking, and the other two have been sunk to 20 inches beneath the surface of the water. In about fifteen or twenty days the sinking will have been finished, and the construction of the masonry will have been begun.

In our programme we reckon that the foundations, as a whole, and the putting of the earth in order will be completed on the 4th of next June, so as to permit us to begin at that time the mounting of the metallic parts.

Note on the Above.—On the 5th of May, 1887, Mr. Eiffel, whose report has just been read, did us the honor to receive us at his field of operations. We first visited the four foundations of the tower, and Mr. Albert Londe, our collaborator, who accompanied us, took the photograph that we reproduce in Fig. 1. Mr. Eiffel afterward took us down into one of the iron caissons. Fig. 2 very clearly shows the arrangement adopted. A descent into one of these caissons is a very curious expedition for an outsider. We enter the upper metallic chamber, which is isolated from the caisson by an air valve, and then, after the door is closed, the air is compressed, and the visitor feels a peculiar sensation in the tympanum. But a simple deglutition suffices to remove this. Then the trap leading to the caisson is opened, and we descend by an iron ladder fixed to one of the vertical cylinders that serves likewise for the passage of the buckets of rubbish. Once in the caisson, we see the workmen picking away the earth by the light of their electric lamps, and putting it into buckets that are raised and emptied outside in measure as they are filled. The caisson, loaded with beton, thus gradually sinks until it reaches firm earth. After this it is entirely filled with beton, and forms an enormous bed of immovable solidity.—*La Nature.*

THE NINE HUNDRED AND EIGHTY-FOUR FOOT TOWER AT PARIS.

The famous Eiffel tower is now in course of construction. Of all the structures that are to cover the Champ de Mars, this is the one that is most actively pushing forward, and the one whose state of advancement is the most curious to study. So Messrs. Alphaud and Eiffel recently invited a number of engineers and builders to examine the foundations of the structure. We were present on the occasion, and are in a position to give the readers of this journal some accurate data as to what has thus far been accomplished.

As well known, the tower is to be supported by four metallic arches that will rest upon four masonry piers.

The soil of the Champ de Mars, which has been so many times disturbed for the last hundred years, consists of a superficial stratum, about twenty-four feet in thickness, composed of a filling-in of diversified material. Underneath lies a stratum of sand and gravel of the same thickness, and, finally, a stratum of clay that constitutes the first layer of the geological formation.

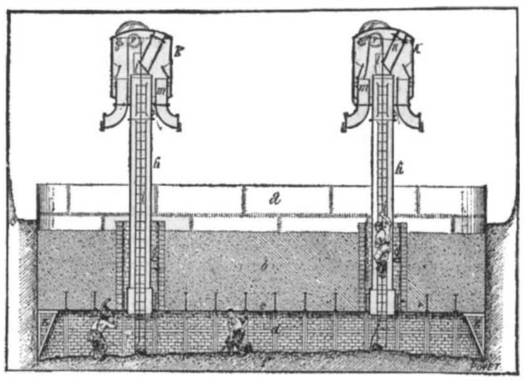


Fig. 3.—SECTION OF THE TOWER CAISSON.

The stratum of sand possesses the proper features of resistance for the foundations, and it is upon this that will rest all the masonry work of the tower. But, as piers 1 and 4 are very near the Seine, and are even located on an old arm of that river that has been filled in for about a century, a solid bed for them was not reached until an excavation had been made to a great depth, and after traversing aquiferous sand that rendered the use of compressed air necessary.

The four piers form a square with sides 328 feet in length. When one stands in the center of the field of operations, he is truly astonished at the area occupied by the base of the tower, and begins to get an idea of the colossal proportions that the arches will possess. As a point of comparison we have the Trocadero, whose towers rise about 328 feet above the surface of the Champ de Mars. Mr. Eiffel's structure will rise three times higher. To tell the truth, it is not so much the total height that strikes one, as the immense span of the metallic arches, and their gigantic proportions. The tower will be neither beautiful nor useful, as every one knows, but it will be interesting from the standpoint of the builder's art, and, since the construction of it is irrevocable, we shall take care to follow all the phases of it and to study the solution of all the difficulties which are to be overcome, and which are to present themselves for the first time to engineers.

Each pier is 91·8 feet square in section, but is not, as might be supposed, composed of a single mass of masonry. This, in fact, was not necessary. It was only necessary to offer a point of support to each of the four corners of the columns of the arches.

As for the pillars, they are about twenty-five feet in height, and their base is such that the earth nowhere has to undergo a pressure of more than fifty-seven pounds to the square inch.

The foundations of piers presented no difficulty. The excavating was done in dry ground, and the pillars of pier 2 are now entirely finished. It will be seen that their surface is not horizontal, but oblique, so as to oppose a normal resistance to the pressure of the metallic arches. They are coursed like a vault, so that the curve of the pressures shall always meet the plane of the joints normally. These pillars are built of Souppes rubble laid in Portland cement, thus permitting of the setting taking place in less than a month—a very important consideration for a work that is to be finished in two years.

In each pillar are embedded two iron bolts six inches in diameter that project more than six feet from the masonry. These are situated in the same vertical plane, and slant in the direction of the arch butments of the tower, for the erection of which they are to serve. They are designed solely for the mounting of the arches, and when once this operation has been effected they will no longer play any part in the resistance of the structure, which will be stable of itself and will need no anchoring.

Pier 3 will support the elevator. Consequently, its central pillar is hollowed out in such a way as to make room for the base of the elevating apparatus. It is in this pier, too, that will be located all the apparatus necessary for operating the elevator.

The construction of piers 1 and 4 was accompanied with more difficulty. As we have stated, it became necessary to have recourse to the use of compressed air. The stratum of gravel was not met with, in fact, until a depth of 35 feet was reached, and aquiferous earth had been traversed. In each trench, therefore, there were placed four caissons upon which the pillars were to be mounted. Each caisson (Fig. 3) is 49×9·5 feet, and is more than 13 feet in height. Toward its center, a horizontal partition, held by a 27½ inch wide girder, supports a thick layer of beton upon which the masonry will rest. Beneath, is the working chamber, which, inclusive of the cutting edge, is about 6½ feet in height. The edge, which is always buried to a depth of 20 inches in the earth, leaves but a slight height for the workmen, who are unable to stand upright. The sides of this chamber are provided with consoles, between which are established oblique linings of masonry. Each caisson is provided with two air chambers. During our visit, twenty-four of us were enabled to enter the caisson at the very moment that a sinking was about being effected.

Although the pressure was scarcely half an atmosphere, the passage through the lock seemed to be quite distressing to most of the visitors. It must be stated that there were twelve of us in the chamber all squeezed up together, at a temperature of 40°, and this did not contribute to render the passage agreeable. But, when once in the working chamber, we became cool again, and the pressure, to which we had got accustomed, was no longer felt.

While we were present, the caisson was sunk eight inches, which was above the average in the present foundations of the tower. Two or three sinkings per day may be effected.

It is needless to say that the interior of the caissons is lighted with electricity.

Much debris has been met with in the excavations. We saw a quantity of oyster shells, bones, and the entire skull of a horse taken out. But, from a mineralogical or geological point of view, nothing of interest has as yet been found in these relatively contemporaneous grounds.

It remains for us to say a few words concerning the precautions to be taken against atmospheric electricity, which might put the future visitors to the tower in jeopardy. In each pier are placed two cast iron pipes, TT, twenty inches in diameter, which are laid horizontally in the aquiferous strata of the earth, and which join one another in front of one of the pillars, and then run vertically to the level of the ground. At this point, they will be connected with the metallic framework of the tower. It is to be hoped that these precautions will suffice to ward off all danger. It is reckoned, in fact, that, should the communication with the earth be well established, the tower may be struck by lightning with impunity, and without any of the persons who chance to be in it feeling the least shock. This metallic structure will operate, it is thought, like Faraday's cage, which receives the heaviest static discharges without affecting an electrometer placed within it. Let us hope that such will be the case, and that, in event of new discoveries in electric phenomena, no experiments will be made that shall prove fatal to visitors, even though the latter be Cook's tourists.—*La Construction Moderne.*

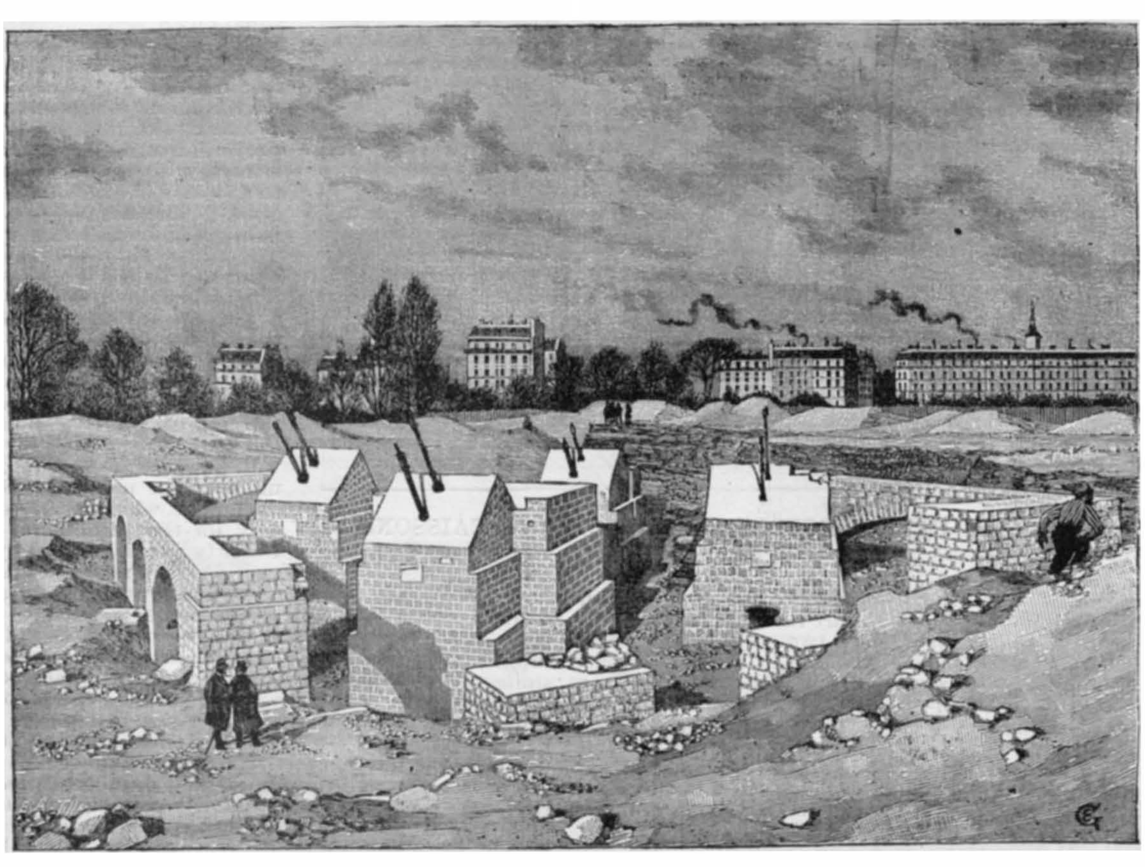


Fig. 1.—MASONRY FOUNDATION FOR ONE OF THE COLUMNS OF THE EIFFEL TOWER.

ENGLISH SALOON RAILWAY CARRIAGE.

The Lancashire and Yorkshire Railway Company show at the Manchester Exhibition an invalid saloon railway carriage, which we illustrate on opposite page. The body of the carriage is 32 ft. long, and consists of a saloon for the use of an invalid, a servant's compartment, with lavatory and luggage compartments. The saloon is highly finished, the cabinet work being of walnut and sycamore, which with the aid of gilt lines produces a very pleasing effect. The upholstering is of claret-colored morocco, and is very tastefully arranged. Double doors are provided at each side, with a view to the easy entry of an invalid on a chair or otherwise; care having been taken that when the doors are closed no draught will be felt.

A bed is introduced, and is so hung (by springs being inserted in each pillar, and attached to the frame of the bed) that when the carriage is in motion the amount of vibration is reduced to a minimum. The bed is provided with a spring mattress, etc., and over the bed is placed a table which is movable logitudinally; also a book rest, which can be placed in any convenient position. Immediately over the bed, and recessed into the partition, is a bracket, which holds a water bottle and tumbler; a bed pull is suspended from the roof to enable the invalid to alter his position if desired. By the side of the bed is a small chair for the use of the attendant. A curtain is hung from a bracket fixed to the partition, to prevent any draught from the sliding door leading into the servant's compartment.

Four chairs are also provided, also a table with falling leaf for the use of any relatives or friends traveling with the invalid. The floor is covered with a rich velvet pile carpet, bordered, of special design; also two Daghestan rugs, one by the side of the bed, and the other between the two double doors.

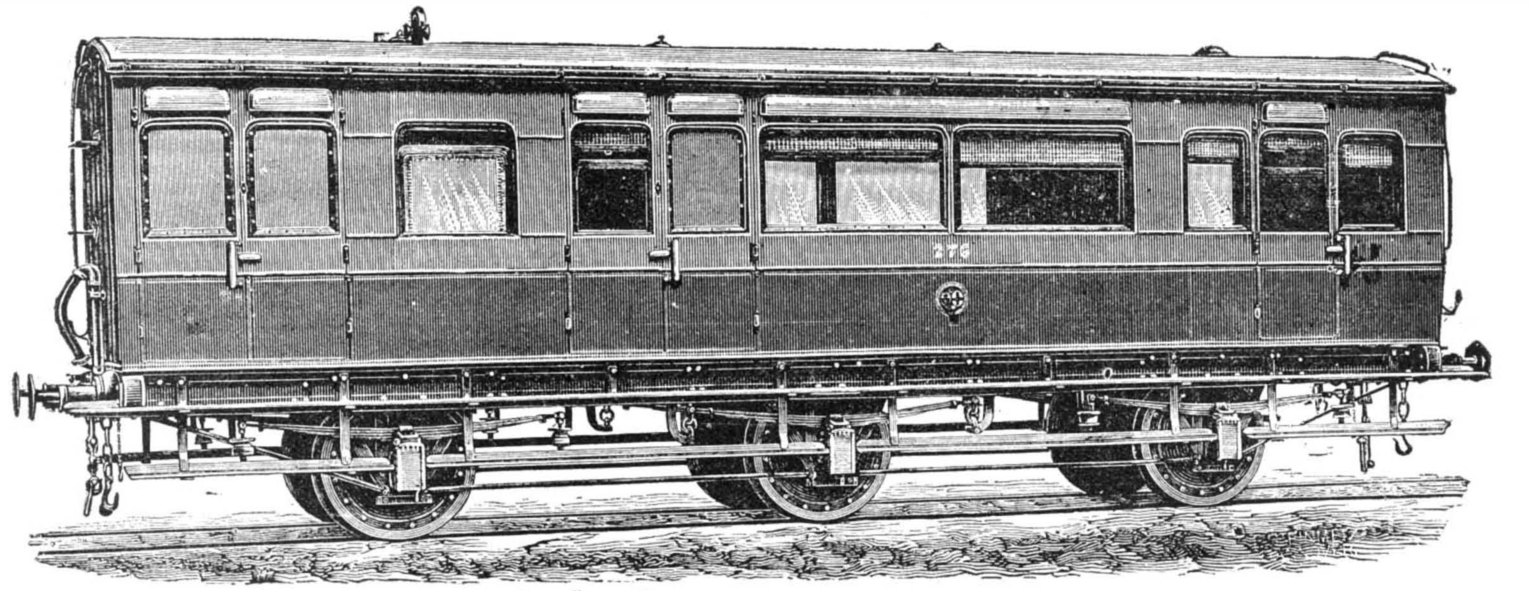
A sliding door in the partition nearest the bed leads to the servant's compartment, the cabinet work in which is of sycamore and walnut. This compartment contains two couches, upholstered in plum-colored cloth, the shorter one being covered, when not in use, by a loose table, which serves as a dumb waiter in the

saloon when required. The compartment is paneled with papier mache, painted brown, and relieved with dark brown and gold. An umbrella stand is also provided. The floor is covered with a similar carpet to the saloon.

At the other end of the saloon, and in the center of the partition, is a sliding door, leading to the closet and lavatory compartment. This compartment is finished with lincrusta-walton of a tint of light green, and the cabinet work is composed of sycamore and walnut. Immediately opposite the door is a large mirror, with a bracket underneath to hold a vase. At one side of the lavatory is a cupboard, which contains a "slipper" commode. The floor is covered with linoleum of a mosaic pattern. Next to the lavatory is a compartment for passenger's luggage.

The coach is lighted by means of Pope's system of gas lighting, a controlling valve being placed in the servant's compartment to control the lights.

The lower exterior of the carriage is painted a rich lake, while the upper part is brown, relieved with gold and black lines. The carriage is supported on three pairs of wheels, which have a total wheel base of 20 ft. It is also fitted with the automatic vacuum brake.—*Engineering.*



Double doors to enable Invalid to be carried in on chairs Fig. 2.

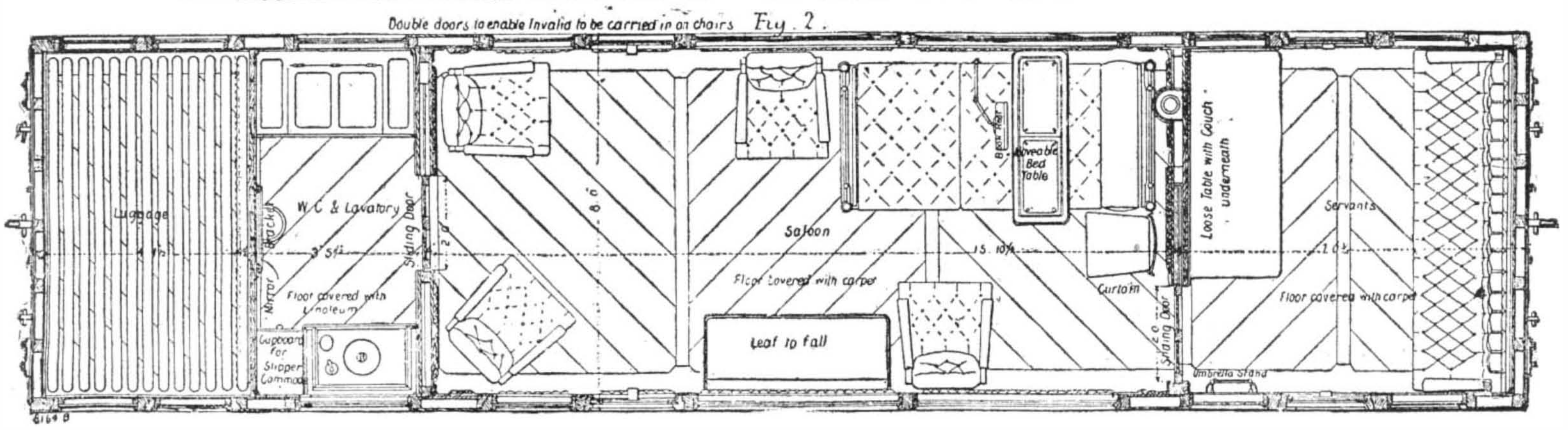
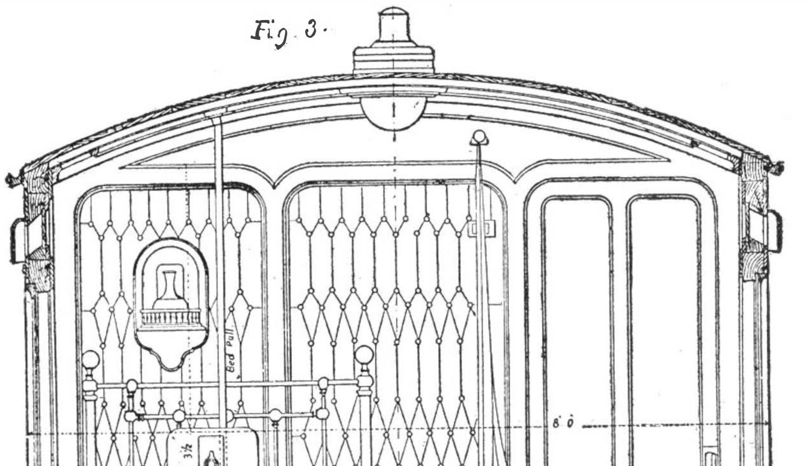


Fig. 3.



ENGLISH SALOON RAILWAY CARRIAGE AT THE MANCHESTER EXHIBITION.

THE PRESERVATION OF RAILWAY TIES AND TIMBER BY THE USE OF ANTISEPTICS.

By JOSEPH P. CARD, Member of the Western Society of Engineers.

THE antiseptics that have been used up to the present time to any considerable extent in the preservation of railway ties and timber are: Corrosive sublimate, kyanizing; sulphate of copper, Boucherie; chloride of zinc, burnettizing; and dead oil, creosoting.

Many others, however, have been tried in the past fifty or more years, and abandoned for one cause and another, which I will not attempt to explain, but will confine my remarks to those now in use.

Corrosive sublimate is the most powerful poison of them all, and its antiseptic properties are some fifty or more times greater than sulphate of copper or chloride of zinc; that is, a solution of one part corrosive sublimate in 10,000 parts of water would, according to the best authorities, be more than an equivalent to sulphate of copper diluted one part in 400, or chloride of zinc one part in 200 of water, which is about the minimum at which they will preserve.

In treating timber with corrosive sublimate, it is generally placed in large wooden vats for one day for each inch in thickness, not counting the day it is put in or taken out, or say ten days for an 8 in. × 8 in. square stick.

The handling of the timber after treatment has to be done with care, or serious consequences may follow. The solution used has generally been one part in 100 of water.

The treatment with sulphate of copper has generally been done by the Boucherie process, or in copper cylinders, on account of its corrosive properties, while the treatment with chloride of zinc is done in iron cylinders, which cost say ten times less than copper. All three of these salts being more or less liable to be chemically changed or washed out of the wood, and as the chloride of zinc has, under most conditions, when injected in proper quantities, answered equally as well, and being cheaper and more economically handled, has come more generally into use than either of the others. In fact, comparatively speaking, corrosive sublimate and sulphate of copper have practically gone out of use.

Chloride of zinc has served a good purpose in the preservation of railway ties in Germany, while in England the treatment has not been satisfactory; in fact, has been abandoned. Now, why this great difference in results? The roadbeds are, so I understand it, alike (rock-ballasted), consequently the drainage is the same. It must be on account of the impurities absorbed into the ties from England's moist climate, which changes gradually the chloride into a non-antiseptic; for rainfall, as a rule, will not, so far as my observations go, wash it out. It takes more than rain; in fact, it means submerging it in water; and this would hardly occur on a rock-ballasted roadbed. If a tie was reasonably dry and in rock ballast, and it should rain, it would absorb moisture slowly as it rained; the flow would be inward, taking more or less of the salt with it, for any of the soluble salts mentioned will, to a certain extent, move around through the wood in whatever direction the moisture goes. When it rains, it goes inward or toward the center; when the moisture evaporates, to the point of evaporation. I have had this tested by analysis to my entire satisfaction.

If ties were submerged, or partially so, in water for any considerable time, the chloride, being of greater specific gravity than water, the tendency would be to go out of the ties rather than inward, or to equalize with the water surrounding them. Again, if the ties were in sand (like the Rock Island ties, which I will mention later on), the result would be, when your sand was moist or wet they would absorb moisture where they come in contact with it, and as it gradually moved to the point of evaporation, which would be the top or exposed portion of the ties, it would carry with it more or less of the chloride. This constant, or at certain seasons of the year long-continued evaporation, weakens, in my opinion, the strength of the chloride at point of contact with the ground or moisture below the minimum of its preserving properties; and in the case of the Rock Island ties, which were in clean sand, they gradually decayed where they came in contact with the ground, but remained sound on top, as a general thing.

Again, should there be impurities in the ground or water surrounding the ties, or in rainfall, that would combine with the chloride or any salt and transform it into a non-antiseptic, as oxide of zinc, the change would be more or less rapid, and it is in this way that I account for the bad results with chloride of zinc in England, and from rainfall, if ties are on rock ballast.

I know of burnettized gumwood ties that were placed in cinder and slack coal ballast in 1880 (the cinders and slack came from a coal mine dump that had been burned over), that were worthless in twelve months after being placed in the track, while ties treated at same time that were placed in sand are sound to-day, or were when I examined them last year.

I don't mean to say that all cinders and slack will produce this result, but these did; neither do I wish to convey the idea that the changes mentioned heretofore occur in a day. Some of them may in a month, or, as in the case of the Rock Island ties, their average life was over fifteen years.

There is a section of some twenty miles of the Union Pacific Railroad where the ties have been preserved since the road was first built, by the soil in which they lie.

With reference to *creosoting, or the use of dead oil in wood preserving*, if you inject a sufficient quantity of oil (of proper quality after steaming and vacuum) into ties or timber, they will remain sound so long as the oil remains undisturbed, if it enters the wood but one-half inch, or even less, on the sides of say a 10 in. × 10 in. stick of timber, notwithstanding the oil remains practically where it is placed at time of treatment, and does not diffuse through the wood, like chloride of zinc, and for the following reasons:

Dead oil contains carbolic and other acids, which

are more or less soluble in water, and enough of these acids combine with the moisture in the wood at time of treatment to destroy the fermentable or other matter then in the wood, that tends to decay, and any impurities or germs of decay thereafter coming from the outside will have to pass through the dead oil, and in doing so are destroyed or rendered inert.

The trouble with creosoting is to get the dead oil where you want it (it will stay where you put it) and the cost. The trouble with a mineral salt, or chloride of zinc, is to keep it where you put it, or where it places itself shortly after treatment, if the work on your part is properly done.

Having given you my experience as well as ideas as to the benefits to be expected from the proper use of mineral salts and dead oil when used by themselves, I will now submit for your consideration and discussion before this society the process known as the "zinc creosote" process, which consists in the use of both dead oil and chloride of zinc in combination, for the preservation of railway ties and timber from decay, as well as protection against the attacks of the teredo where timber is placed in the sea.

For railway ties, bridge timber, and the like, or where timber is subjected to no considerable moisture, as when placed on or in the ground, the process is as follows:

After preparing the timber in the usual way by steaming and vacuum, the dead oil is run into the cylinder, and such quantity as may be desired is forced into the wood.

For railway ties or timber I would recommend say one-half gallon to the cubic foot, or 1½ gallons to the tie. A less amount may be found to answer. After the timber has been treated with oil, the oil is removed and cylinder charged with chloride of zinc, when by pressure it can be made to enter the wood, pass through and beyond the oil, and impregnate by diffusion that portion of the wood that the oil will not penetrate, especially where timber is not well seasoned or dense like oak. The aim of this process is to get the benefit of the dead oil treatment, where ties or timber come in contact with the ground or moisture, with one-half or less oil, besides having those portions of the wood not penetrated by the oil impregnated with the zinc chloride. The zinc chloride, surrounded as it is by oil, should be protected for a long time in railway ties or bridge timber against moisture. I find that less than one-half the quantity of oil used in ordinary creosoting can be distributed by this process through every portion of the wood penetrated by the greater quantity injected in the usual way.

Creosoting, as practiced abroad, unless a much larger quantity of oil is used on railway ties than is used in England (6 to 10 pounds to the cubic foot), is of little value in my opinion, unless a chair is used under the rail to take the wear, for the following reasons:

Where dense woods are used—in fact it is the case with many of those considered porous—the heartwood will take the oil but skin deep, consequently the oil is in time worn off by the rail, decay commences and at the worst possible place, the spike becomes loose and the tie valueless.

This is probably the reason there were so few American creosoted ties exhibited at the Exhibition of Railway Appliances in 1883, and the few there were had been treated to at least two gallons of oil to the cubic foot. If I am not correct, I would ask what has become of the thousands that have been treated in the past thirty years in this country, where we use no chair?

I have here one of a lot of ties treated by a Mr. Pelton some years since for the Chicago, Rock Island & Pacific Railway Company, and placed in their tracks near Englewood. It was taken up in May, 1883, for exhibit at Chicago exposition of that year. These ties were treated by what is known as the Seely process, in 1872, and notwithstanding they contained but little oil (less than four pounds to the cubic foot), were sound, so far as examined by me, where they came in contact with the ground, but commenced to decay (so I was told) under the rail as soon as the oil wore off, and not before.

If a sufficient quantity of oil is used to impregnate the ties to a considerable depth at point of contact with the rail (which means for oil say fifty cents or six gallons to the tie, and this applies only to soft woods and not to oak), a good result would be obtained; otherwise, a chair must be used.

If you will show me one tie that has served a good purpose, I will convince you that it was treated to at least six gallons of oil, or a chair had been used. Not but what a much less quantity would preserve it from decay if placed in the ground as a post and undisturbed; but should you remove the oil at the ground line, it matters not to what extent, so the untreated timber is exposed, you will find your creosoting of little value, and this is the experience of all.

Mr. J. W. Putnam, of New Orleans, in a letter of June 20, 1885, to the Chairman of the Committee on Preservation of Timber of the American Society of Civil Engineers (I presume many of you know him), says: "With reference to creosoting, wherever the coating is broken and the air with its dust allowed to come in contact with the untreated wood, decay follows, and extends in each direction from the opening;" and he is but one of the many who make this or similar statements.

The burnettized ties on the Chicago, Rock Island & Pacific Railway, near Englewood, and so far as I have examined those from other roads (where work was well done), were sound under the rail, but decayed where they came in contact with the ground.

Mr. Alexander, in his report of March 23, 1882, to Mr. Hugh Riddle, then president of the Rock Island road, says: "I made a careful examination of the burnettized hemlock ties we laid in main track just west of Englewood in November, 1866, last summer, and found at least seventy-five percent. of them still in the track, and in my opinion in such a state of preservation that they will be serviceable for two or three years longer. Some five or six of these ties were taken out of track and found to be sound and solid in the center and only decayed to the depth of one-half to three-quarters of an inch on the surface and sides. The rail has not worn into these hemlock ties to any greater extent than would have occurred with oak, and they hold a spike fully as well as the oak tie. The pine and cedar ties that were burnettized at the same time have worn out in the fifteen years' service, and have disappeared. The tamarack have held out about the same as the hemlock." Continuing, he says: "My experience is that untreated hemlock ties decay first in the center or heart, when the spike becomes loose and the tie crumbles; but these treated ties are sound in the center, which shows that where the chloride of zinc is not washed out, the wood is in a perfect state of preservation."

I saw these ties a short time after they were taken up, and examined those remaining in the track in June, 1883 (they had then been down over seventeen years), and found them to be sound under the rail with hardly an exception. I also had the sound wood from several of these ties analyzed, and found them to contain from 0·05 to 0·14 of one per cent. of chloride of zinc to weight of the wooden when dry.

Again, in the same report (March 23, 1882), Mr. Alexander says: "In 1872 we laid in second track east of Washington Heights about 5,000 hemlock ties that were subjected to the creosoting process. These ties I do not believe were thoroughly treated. They seemed to be tolerably sound at the bottom, but are badly decayed on the surface, and the rail wears into them to a much greater extent than it does into those that were treated with chloride of zinc. There is probably not more than from 30 to 50 per cent. of these creosoted ties now in track, and these will no doubt all be taken out this summer."

I examined these ties, or what there was left of them, in June, 1883, finding few then in the track, but was fortunate, however, in finding several hundred that had just been taken up and piled along the track. Nearly all of them showed results of which the creosoted tie exhibited here, which was one of them, is a fair sample.

If I am correct, what can be expected of creosoted ties with but 6 to 10 pounds of oil to the cubic foot if used as they are in this country in direct contact with the rail, and what must we do to get best results in the preservation of our ties? Use a chair as in England, or open porous woods and inject 50 to 75 cents' worth of oil into each tie, or will a double treatment first with dead oil and then with chloride of zinc answer the purpose?—the dead oil to preserve the outer or exposed parts, which it will do, and the zinc chloride the central portions, which the oil does not penetrate to any considerable extent in our most desirable woods. So far as my observations and experiments go, I am satisfied that time will demonstrate that dead oil and chloride of zinc injected into ties and timber as proposed will give the best results for money invested, and where dense woods are used, especially for ties, the best result, without regard to cost.

You may say that the old way of creosoting closed the pores, thereby keeping out moisture. Dead oil will not keep moisture in or out of wood like paint, tar, or pitch for any considerable time. Moisture will not enter a creosoted tie above the surface of the water surrounding it, or without pressure; neither will it enter except under same conditions where the fiber is oiled. This being the case, your ties, under most conditions, will remain dry, and the zinc chloride should be protected.

Again, you seldom see decay in wood the fiber of which has once been covered with dead oil, to such an extent as to be seen by the eye. The zinc creosote process will, as I said before, distribute one-half or less oil in every part penetrated by the greater quantity when injected in the old way, and in such quantities as can be readily seen.

You may say, Would it not be better to first inject the chloride and then the dead oil? If the treatment were reversed, you would have to remove a portion of the moisture before the oil could be injected. Wood being one of our best non-conductors of heat, the process would be tedious, and timber or ties would be more or less injured by the long-continued application of the heat required to evaporate sufficient moisture. In fact, the only cheap and practical way would be to air, dry, or stack the timber until sufficient moisture has evaporated, and then apply the oil. I do not believe there would be anything gained by so doing, and it would add greatly to the cost.

With reference to the treatment of piling with dead oil as protection against the teredo: The old way is to inject all the oil the timber will take (which depends on the piles being more or less dry and the kind of wood operated on), from one to three gallons to the cubic foot. The object of the zinc creosote process is to economize in the quantity of oil used and nothing more, and consists in first injecting say two-thirds (one-half may be found to answer) of the quantity used in the old way, and then by substituting some other fluid for the oil, as chloride of zinc, air or water by pressure, compress or force the two-thirds previously injected solidly to the center, which leaves the two ends, the one in the mud or ground, the other above water, with their fibers virtually painted with the dead oil, while the center of the pile or that portion in the water is as well treated and contains as much oil as would be the case if the whole quantity used had been oil. I would prefer an antiseptic for the second injection, as it would help to preserve that portion above water from decay.

Thanking you, gentlemen, for your kind attention, I would, in concluding, say that we have at our works, Fifteenth and Clark Streets, a small experimental cylinder, which is at the service of this society, or any of its members, should they desire at any time to make experiments in this line.—*Jour. Assn. of Eng. Societies.*

APPARATUS FOR TESTING BEER AND OTHER LIQUIDS.

THERE are many devices for obtaining the specific gravity of fluids, but one of the best of these is Westphal's specific gravity balance, which is illustrated in Fig. 1, and which works on the old principle that when a body is introduced into a liquid, said body suffers a loss of weight equal to the weight of the liquid displaced. In the use of this apparatus counterweights are hung on the scale beam, and a thermometer is the body sunk in the liquid. The temperature of the liquid experimented with must be about 60°. When liquids are tested which have a greater specific gravity than water, the weight, A_2, is hung on the forward end of the beam, which is divided by notches into ten equal parts, as shown. The beam is in equilibrium when the body to be inserted in the liquid is suspended in the air, and, therefore, when said body is introduced into the liquid, an additional weight must be used. For instance, with a certain liquid which is heavier than water, the weight, A or A_1, must be placed in the notch 6, which shows that the load at *m* equals 0·6 time the respective weight, A, A_1, or A_2, and consequently the weight of the liquid displaced equals 1·6A or 1·6 times the weight of an equal volume of water at 60° In order that the specific gravity may be obtained to within a hundredth or thousandth, the rider, B, is made to equal $\frac{1}{10}$, and the rider, C, $\frac{1}{100}$ of A or A_1 and A_2. When testing liquids the specific gravity of which is less than that of water, the weight, A_2, is removed and the count begins with "0." As it may be necessary to use two or three riders in one notch, said riders are provided with hooks, so that one can be hung on another.

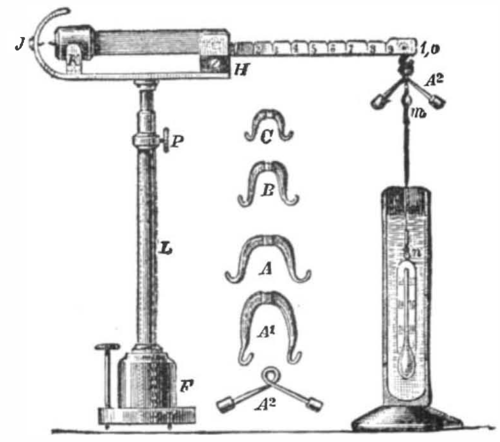


FIG. 1.—THE WESTPHAL BALANCE.

In Fig. 2 we show the Geissler vaporimeter used in determining the amount of alcohol in a liquid. This apparatus consists of the brass receptacle, A, which is half filled with water, and the bent tube, B, fastened to a brass plate provided with a scale. A cylindrical vessel, O, is filled partly with the liquid to be tested and partly with quicksilver, and a thermometer is introduced into the casing, D. The vessel, O, is first filled to the mark, *a*, with quicksilver, the remaining space to the mark, *b*, serving for the reception of the alcoholic liquid; then the tube, B, is inserted in the vessel, O, and the whole apparatus is turned over and secured to the vessel, A, and finally the casing, D, is placed over it. The steam from the vessel, A, warms the vessel, O, and its contents, and a portion of the latter is converted into vapor, which presses the column of liquid in the tube, B, up the scale on the brass plate a distance corresponding to the amount of alcohol in the liquid under consideration. The steam in the vessel, A, passes through openings in the casing, D, is condensed, and then drops down again to the receptacle, A.

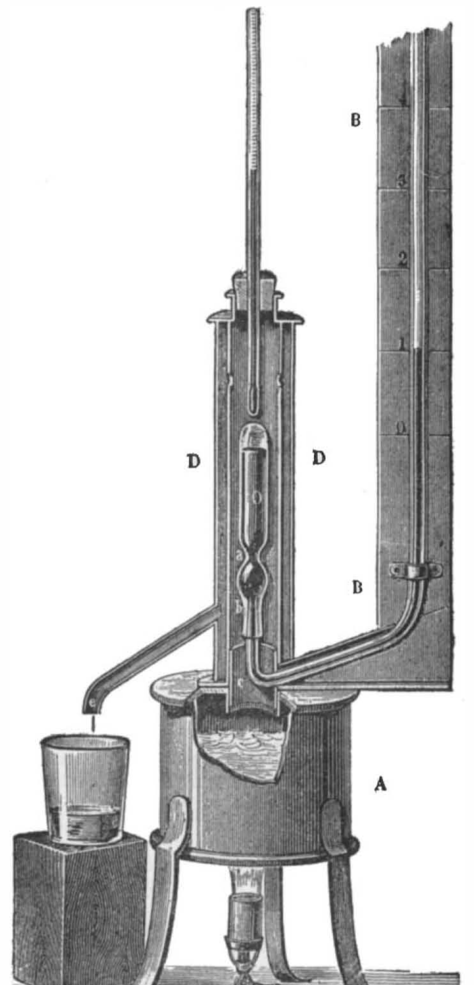


FIG. 2.—GEISSLER'S VAPORIMETER.

The apparatus shown in Fig. 3 is designed for the determination of the amount of carbonic acid in a liquid, and is used as follows: A certain amount of the liquid to be tested is weighed and then heated in the vessel, A. The carbonic acid escapes through the tube, *b*, the condenser, B, the chloride of calcium tube, C, the absorption bulbs, D, which contain concentrated sulphuric acid, the potash bulbs, E, the little tube, F, containing stick potash, and the guard tube, G. The vessel, A, is connected with an absorption bottle, T, containing soda lime, which bottle can be cut off from

the vessel, A, by a cock. When there is no more pressure of carbonic acid, the liquid is heated and boiled until bubbles are no longer formed in the potash apparatus. Then the tube, G, is connected with the aspirator. H, by means of the pipe, *g*, and the air which has been freed from its carbonic acid in the soda lime bottle is drawn into the aspirator. Finally the potash apparatus is weighed again—it was weighed before the operation was begun—and the increase in its weight equals the amount of carbonic acid contained in the liquid being tested.

If, in running in one direction, it is the wheel, D, that moves the drawing forward, it will suffice to throw the wheel, E, into gear in returning, in order to move the drawing in the contrary direction.

Owing to the facility with which both wheels may be freed from contact with the endless screw, it is possible, through a screw, to regulate the position of the drawing so as to make the starting point exactly coincide with the index.

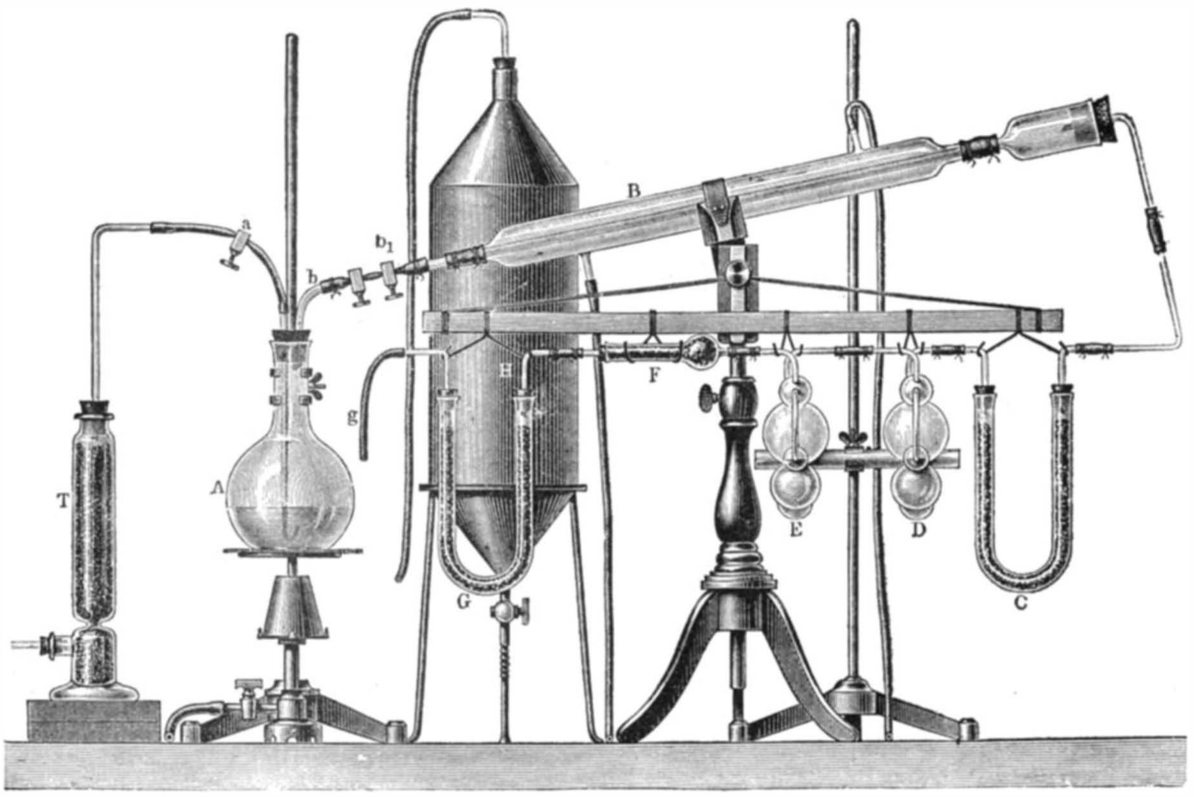


FIG. 3.—APPARATUS FOR THE DETERMINATION OF CARBONIC ACID.

We are indebted for the accompanying cuts to the *Illustrirte Zeitung*.

THE LOCOGRAPH.

THE object of the apparatus called the "locograph," which we represent herewith, is to unwind before the eyes of a locomotive engineman a drawing which represents, on a properly selected scale, the direction line and profile of the road on which his train is running.

The apparatus consists of a toothed wheel, C, which

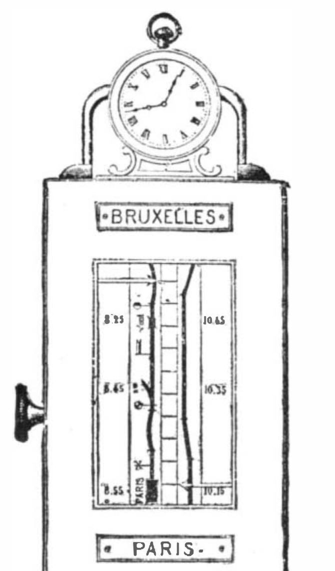
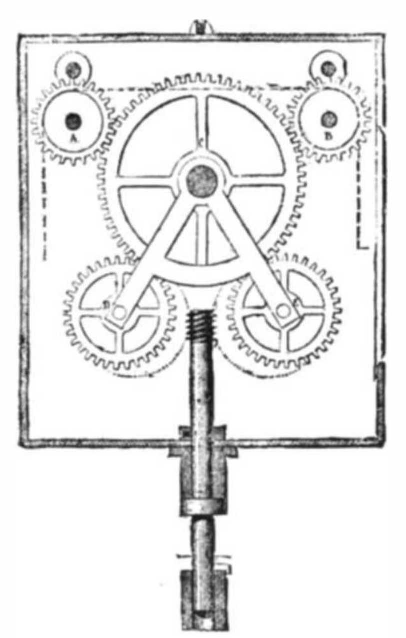


FIG. 1.—THE LOCOGRAPH.

actuates two cylinders, A and B, by means of toothed wheels placed at their extremity. Two other toothed wheels, D and E, engage with the wheel, C, and are connected with its axle by an invariable piece that permits of putting either of these two wheels in contact with an endless screw or of severing contact therewith.

This endless screw is set in motion through a gearing by means of a free wheel of the locomotive. This wheel, in revolving, actuates the screw, and according as it acts upon the wheel, D, or the wheel, E, causes the drawing of the roadway to move in one direction or the other. The drawing, which is first wound round the cylinder, F, passes over a second free cylinder, then over the cylinders, A and B, which carry it along, and finally winds round the cylinder, G, which the cylinder, F, acts upon through friction. A simple calculation permits of ascertaining the relation between the road passed over on the rail and the unwinding of the drawing. An index constantly shows the engineman just exactly where he is.

Mr. Clepkens, the inventor, belives that this apparatus is capable of furnishing useful indications to the engineman in all cases where it is difficult to see objects in front of or around the locomotive. It especially presents the advantage of allowing of the employment of any engineman whatever, in all countries, and on any lines whatever of a system of railroads; and such



FIGS. 2 AND 3.—DETAILS OF MECHANISM.

an advantage may prove of importance in cases of the quick transfer or mobilization of troops.—*Le Genie Civil.*

FORMULA FOR THE DEVELOPMENT OF PAPER NEGATIVES.

BUCHANAN WOLLASTON, in a paper read before the British photographers' convention, gives the following useful hints for the development of paper negatives: In my own practice I have found films, *cæteris paribus*, more rapid than glass plates, but upon this point I have met with the most conflicting evidence, some experimenters positively asserting that they are slow, others that they are *much* more rapid than any other form of gelatino-bromide. Now, my impression is that with a given emulsion rapidity is favored by spreading it upon paper, for three reasons: (1) That in drying emulsion upon paper no crushing of particles takes place on the lower strata; (2) that no blurring of light into shadow takes place; and (3) that the developer, attacking the light-affected parts *on both sides*, brings about a more complete reduction of the haloids. But a great secret lies in development, and I shall endeavor to show you how this can be accomplished with success.

Two golden rules: Do not overexpose. Push the development further than *seems* necessary.

Without troubling you with experimental formulæ, I will give you at once the developer I now use and swear by, but if any member will show me a better, I will abandon my pet at once upon conviction. My formulæ stand thus:

No. 1.

Sodic sulphite, *pure*............8 ounces.
Hot distilled water.......... ....40 " (fluid).

Let cool to 60° Fahr., and render *just* acid with citric. Test with litmus. Pour on to 1 ounce (437½ grains) pyro.

No. 2.

Sodic carbonate, *pure*........... 4 ounces.
Potassic " *pure*............. 1 ounce.
Distilled water.................... 40 ounces (fluid).

Mix equal parts No. 1 and No. 2 for normal exposures.

Always have a ten per cent. solution of bromide at hand for emergencies, and use if great opacity is desired, but as a rule no restrainer seems necessary.

It is sometimes desired to proceed with caution, and add a portion only of the No. 2 solution at the outset, and when time is not of much importance I prefer further to dilute the mixed developer with one-third to one-half its bulk of water.

With the above developer and plenty of time—always given a reasonable amount of brains, as Colonel Stuart Wortley once remarked—uniformly excellent negatives can be produced with almost absolute certainty, and upon any of the leading films now in the market; but I must frankly admit that I have seen negatives produced by one of our much respected members with an ammonia developer fully equal in quality and pluck to the best by my method, but then it is the man, not the developer. In my hands ammonia falls far short of the fixed alkalies in uniformity of results.

It is not within the scope of this paper to give detailed instruction in finishing either paper or film negatives, nor is it my intention to recommend any particular brand, or special roller slide, but I will tell any member who wishes to know—quite privately of course—where lies my love. And if my remarks may induce any gentleman to try filmography and he gets into difficulties, let him write to me and I will try and help him over the stile.

NEW PROCESS OF METAL WORKING OF MESSRS. N. DE BENARDOS AND S. OLSZEWSKI.

THE pieces of metal to be treated are connected with one pole, generally the negative one, of an electric generator. A rod of carbon is connected to the other one by a flexible conductor. If, for an instant, the carbon is pressed against the metal and then immediately drawn back, a voltaic arc is formed between the metal and carbon. The evolution of heat that follows is utilized at once for the work that is to be done. Thin sheets of metal are thus perforated; holes are sunk in larger pieces of metal; by means of soldering, pieces of different forms are united.

The apparatus shown in Fig. 1 is designed to be

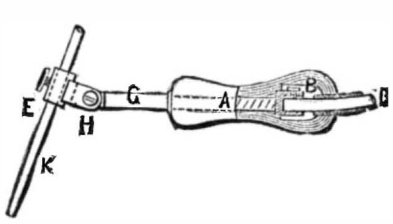


FIG. 1.

held in the hand. It consists of a carbon holder, G, supplied with a handle, A. The set screw, E, admits of longitudinal adjustment of the carbon, K. The arrangement shown in Fig. 2 is somewhat more complicated. It is adjustable by means of the plates, *r r'*; *a* and *b* are connected by suitable conductors with the electric generator. The carbon is brought in contact with the piece to be treated by pressure upon the lever, J. A spring, H, acts in opposition to the lever, J, and raises up the carbon when the lever, J, is relieved from

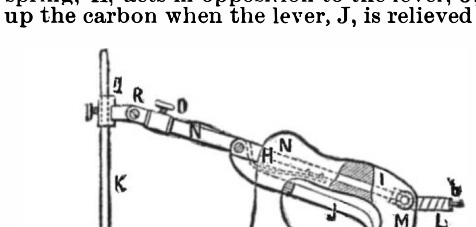


FIG. 2.

pressure. The apparatus shown in Fig. 2 is specially designed to execute long rectilinear soldering.

Other similar dispositions have been devised by Messrs. Benardos and Olszewski, for moving the apparatus along the metal so that the arc will only act at intervals previously determined. So that the workman's eyes will not suffer from the reiterated appearance and extinction of the voltaic arc, the inventors supply the apparatus with a screen of colored glass. The arrangements of the parts as described do not seem altogether satisfactory. It is enough to note that in the apparatus of Fig. 2 no arrangement is pro-

vided for the automatic feeding of the carbon as it burns. The idea, however, upon which these apparatus are based is well worthy of consideration. In Germany experiments are being conducted in many large metal-working establishments to determine its practical value. The French patent has been acquired by Baron Rothschild.—*L'Industria.*

FIFTY YEARS' PROGRESS IN TELEGRAPHY.*

By W. H. PREECE, F.R.S., Vice-Prest. of the Society.

THIS is the jubilee year of the electric telegraph in England. On July the 25th, 1837, the first practical trial of telegraphy was made between Euston and Camden, on the London and Northwestern Railway, by Cooke and Wheatstone. Nine tedious years were spent in endeavoring to create enthusiasm among financial circles for this young and novel means of communication. The first company formed to develop the business of transmitting and delivering written telegrams was incorporated in 1846, and was called the Electric Telegraph Company. Wires were speedily extended to every great center of trade along the great railways that were then being developed, and about 1851 the business was fairly established. Telegraphy and railways have grown up side by side. In 1851 submarine telegraphy was proved to be practical. Cables were laid in 1852 and 1853 to France, Belgium, Holland, and Ireland, and from that year domestic and international telegraphy grew and prospered. The Electric Telegraph Company suffered the fate of all prosperous initiators of that which is novel, useful, and successful. The validity of their patents was contested, the strength of their business was assailed all along the line by enterprising competitors. Their monopoly was broken; but the public gained by this active rivalry greater development of communication and considerable reduction of rates. We see the same process going on at the present day with respect to telephones, but with less success.

The rates for messages, which were as high as 12s. 6d. for twenty words, were based on a sliding scale. The rates for a message of twenty words were fixed at 1d. per mile for distances under 50 miles, then ½d. per mile up to 100 miles, and ¼d. per mile for a distance of more than 100 miles; addresses forming part of the twenty words. A message to Glasgow cost 8⅝d. per word, to Manchester $5\frac{1}{16}$d.

Various reductions took place from time to time. In 1864, that is in less than twenty years, three great companies competed in every principal town for this profitable business of telegraphy. The United Kingdom Telegraph Company was formed in 1861 to introduce the universal shilling rate; but the severity of the competition of their opponents was too great for this spirited company, and they had, in 1865, to submit to the following general tariff: Within London and other towns, 6d.; within 100 miles, 1s.; within 200 miles, 1s. 6d.; over 200 miles, 2s., addresses being sent free.

The public now began to suffer. Only the larger towns, where profitable business could be tapped, were served. The smaller towns were neglected, and great villages had to suffer the aggravation of seeing posts and wires pass through their main thoroughfares without being able to avail themselves of this speedy mode of communication.

Telegraphy became a necessity of the age for the due and proper transaction of business. It became so closely allied with other modes of communication that public opinion, in 1868–69, forced the government to purchase and absorb all the telegraph companies, and to transfer their business to the care of the post office, which had shown itself so capable in dealing with letters and newspapers, and in establishing the penny post.

The telegraphs of this country again became a great monopoly; but there is a vast difference between a monopoly in the hands of a private speculative corporation, subject to no control but that of its own close ring, and whose sole object is to earn dividends, and a monopoly in the hands of the government, whose sole object is to serve their masters the public faithfully and well, and whose actions are incessantly supervised by the jealous and watchful eyes of those they serve, who have an equally watchful and much less tolerant press in which to air their wrath and grievances, and an active House of Parliament ever ready to counteract and reform real abuses when fairly and properly laid before it.

It is impossible to conceive supervision more complete than that to which the post office is subjected. The receiver of every letter, the transmitter of every message, the editor of every newspaper, the reader of every news dispatch, every householder or business man, can see for himself how his own business is conducted; he can growl and grumble to his heart's content at any error or delay that may occur, with a certain knowledge that his complaint has been heard, and, if just, remedied, although he may not always be satisfied with the attention he receives. The telegraphic business of this country has reached its present dimensions because the work has been done well, and it has been done well because the mode of doing the business has been so well and so thoroughly supervised by the public.

The transfer of the telegraphs to the state took place on February 5, 1870. The tariff established by Parliament was a uniform tariff of 1s. for twenty words, addresses being sent free, and this tariff remained in force until October 1, 1885, when the present simple word tariff of ½d. per word, irrespective of distance, and including addresses, with a minimum of 6d., was introduced.

The average cost for the transmission of a telegram immediately before the transfer was 2s. 1½d. After the transfer it was reduced to 1s. 1d. It is now 8d.

The number of offices open to the public prior to the transfer was 2,932, but owing to the existence of three large companies in the same town, the number of towns in telegraphic communication was probably not more than 2,500. There are now 6,514 offices open to the public.

But the amount of business done is a better criterion of the benefits that the public have derived from the transfer of the telegraphs to the post office. At the close of the year 1870, the gross receipts of the telegraph department were £612,301; at the close of 1886 they were £1,787,264; and at the close of this year they will probably reach £1,950,000. The number of messages transmitted in 1869 was 6,000,000; in 1870, there were 9,850,177; in 1880, there were 26,547,137; in 1886, there were 39,235,813; and at the end of this year (1887) they will probably exceed 52,000,000.

The number of messages dealt with each day ranges from 70,000 to 100,000, nearly half of which are transmitted messages which have to be both received and forwarded, and therefore become, practically, two messages, although they count as only one in the total.

The local traffic in London—that is, messages emanating from one part of London for delivery in some other part, and passing through the central station—is very large. It ranges from 12,000 to 18,000 messages per day. In 1868 there were 60 offices open in the metropolis, dealing with 300 messages per day; there are now 480 offices, dealing with an average of 15,000 messages a day.

The total number of local metropolitan messages for four weeks was: February, 1870, 138,534; January, 1880, 726,199; January, 1887, 1,277,838. A tenfold growth nearly.

PRESS ARRANGEMENTS.

One of the great objections raised against the absorption of the telegraphs by the state was the difficulty which the government would have in transmitting news. In no country is there now such a complete system of telegraphy for news purposes as there is in the United Kingdom. Two complete news circuits serve always (day and night) the following important centers from London:

1. Newcastle, Edinburgh, Glasgow, Dundee, Aberdeen.
2. Nottingham, Sheffield, Leeds, Bradford.
3. Birmingham, Manchester, Liverpool.
4. Bristol, Gloucester, Newport, Cardiff, Exeter, Plymouth.
5. Brighton, Portsmouth.
6. Dublin, Cork, Belfast.
7. Northampton, Leicester, Derby.

There are several other groups in the provinces served from these centers, and every evening after 6 P.M. a third, and frequently (especially in the parliamentary session) a fourth circuit is made up to all these stations from London. Each office on each of these groups receives simultaneously the news sent from London. Thirty newspapers obtain the use of special wires, with clerks' services, from 6 P.M. to 6 A.M., many, like the *Scotsman*, the *Newcastle Chronicle*, and the *Manchester Guardian*, having two circuits, and whenever any great political event arises, such as the delivery of a great speech, all the important towns throughout the kingdom receive simultaneously a *verbatim* report of the speech. There is not a town in the country where a daily paper is printed which is not placed after 6 P.M. in direct communication with London, and where there is not deposited on every subscriber's breakfast table a nearly *verbatim* report of the previous night's debate in Parliament.

The press rates are very low. The average price paid is a little in excess of 2d. per 100 words. This entails a loss to the department roughly estimated at £200,000 a year, which is the amount the public is taxed for the support of the press. It is doubtful whether Parliament knew when it passed this low rate that it virtually meant a subsidy to the press. The loss might be very materially diminished, if there were less competition and more union among newspapers and news agencies. As it is, the same matter has frequently to be sent twice over the same route, and the amount of unnecessary news sent, and therefore unnecessary expense, is enormous, much of it finding its way into the waste paper basket. The supply of news, before the transfer, was very meager. Reuter's telegrams, parliamentary reports, general news, markets, races, were supplied for £200 a year. The companies did this jointly, and news was collected as well as distributed; but the post office is simply a carrier. It is not allowed to collect, and it is thus saved much of the obloquy that attached to the irresponsible monopoly of the telegraph companies. The average number of words supplied to each newspaper was said to be 4,000 a day. It is now roughly estimated at about an average of 12,000 in the recess, and 15,000 or 20,000 in the session.

APPARATUS.

It is, perhaps, in the character and form of the apparatus used to transmit messages that the greatest progress has been made. It is difficult to say whether increased business has led to better apparatus, or whether improved apparatus has led to large business. One thing is certain, that the better the work is done the more is business encouraged, and work cannot possibly be well done if the apparatus used for discharging that business is inefficient or backward. When the post office assumed the control of the telegraphs, it amalgamated into one department an incongruous combination of various systems, worked by differently trained staffs, due to various companies having been formed at different periods to work different patents. The Electric Telegraph Company established the needle system of Cooke & Wheatstone, the printing system of Bain, which merged into that of Morse, and the various improvements patented by Varley; the Magnetic Company fathered the magnetic system of Henley and the bell system of Bright; the British Company introduced the system of Highton; the United Kingdom Company promoted the beautiful type-writer of Professor Hughes; and the Universal Private Company was established to introduce the simple A B C system of Wheatstone.

We had telegraphs that appealed to the eye like the needle, those that appealed to the ear like the bell, some recorded signals in ink like the Morse, others printed their characters in bold type like the Hughes. Some were slow but simple, like the A B C, others were fast but complicated, like the automatic. Time and patience were needed to consolidate into homogeneity this heterogeneous collection of telegraphs and telegraphists. Some years elapsed before the doctrine of the survival of the fittest was established.

Now, in 1887, the predominant telegraph instruments are the simple sounder and the fast speed automatic recorder. Reading by sound is confined almost exclusively to the United Kingdom and to the United States. In Europe there is scarcely a sounder outside our islands. This is very remarkable, for the sounder is simpler, more expeditious, and more accurate than any other key system. Those who have been educated to regard a record as an element of accuracy can only be convinced by actual experience that it is an element of error, and this experience they will not seek. The argument they use against the adoption of the sounder, viz., its liability to error, is devoid of any foundation in fact.

Comparative Return, showing the Number and Descriptions of Telegraph Instruments in the United Kingdom.

Year.	Automatic.	Sounders.	Printers.	Needles.	Bells.	A B Cs.	Miscellaneous.	Total.
1877......	164	1,294	1,692	3,680	210	4,572	129	11,741
1878......	167	1,380	1,560	3,495	277	4,641	859	12,376
1882......	224	2,000	1,330	3,791	313	4,398	2,035	14 091
1886......	384	3,181	1,368	4,003	388	3,883	5,179	18,386

During the past ten years a complete revolution has been effected in the quality and manufacture of our instruments. Exact measurement and scientific principles have supplanted rough and ready methods. Complete specification and rigid inspection have replaced cheap and nasty competition. The workmanship of a good telegraph instrument is to be rivaled only by that of a chronometer. Technical training has converted the workshop into a scientific laboratory.

The rapid increase of business that resulted from the uniform shilling tariff soon led to the erection of more wires, and the multiplication of wires soon attracted attention to methods of duplexing and quadruplexing the circuits. The duplex system means a mode of sending two messages in opposite directions at the same time. This was shown to be possible by Gintl, in Vienna, in 1853, but the necessity for such a system did not arise until 1872; and as at the moment a want is felt something is sure to turn up to supply this want, so when duplexing was needed Mr. Stearns arrived from America with a well worked out practical system, that was at once adopted, improved, and perfected. Still further congestion arising, quadruplex working, or the art of sending four messages on one wire at the same time, became desirable, and a practical quadruplex system, due to Mr. Edison, was imported in 877 from the same inventive and practical region, the United States. The work of the Austrian Meyer ought also to be referred to. Later on, in 1885, a still further development was matured in America, viz., the multiplex system of Delany, by which six messages can be simultaneously sent on the same wire, which we have adopted, and the main features of which I now show you in action. The chief reason why these systems have been matured in America is that the want has been experienced there before it was felt here. Neither system was invented in America—each was invented in Europe. There are other wants that have been experienced here first, and those who have visited the States have found that English inventions are equally appreciated and adopted there. It is in automatic telegraphy that we have made the greatest advances. The following table illustrates the progress made:

	Words per minute.	Speed to Ireland.
1870..............................	80	59·3
1875..............................	100	70
1880..............................	200	150
1885..............................	350	250
1887..............................	450	450

This increase has been due not only to improvement in the design of the apparatus, but to the steady examination of every defect and its removal, in the instrument and on the line. It would require a paper of itself to narrate the ten years' conflict with electro-magnetic inertia, static induction, climatic influences, and battery defects. There still exists in our system a potentiality of expansion. We are now attacking the wires. Copper is replacing iron on our poles, with very advantageous results. Its better conductivity, and its entire freedom from electro-magnetic inertia, give it an immense superiority over iron. Its greater price per ton is compensated by its lesser weight per unit resistance.

We are also examining and testing various modes of laying wires underground, with a view to attaining greater speed of working, hitherto the great difficulty which has checked the establishment of underground work.

One consequence of the introduction of these advanced systems of working has been the necessity of educating the operating staff in the scientific and technical details of the business. The absence of technical knowledge in all branches has hitherto been a great difficulty to surmount. The technological examinations inaugurated by this society, and continued by the City and Guilds Institute, have been most beneficial, but the most successful incentive has been the selection and promotion of those who have given their attention to their own scientific education. This evident necessity for technical knowledge is reacting on the higher post-office officials, and one finds all over the country a healthful spirit of inquiry arising—a striving after something better than the mere perfunctory discharge of official duties. There is something so captivating in the development of the practical applications of electricity, that those who make a study of it, especially experimentally, find in it more real enjoyment than any puzzling over the vagaries of the modern poet or poring over the meaning of ancient cynics can afford. A successful experiment is a distinct revelation—an admission into courts where, according to Bacon, are found "secrets not dangerous to know, sides and parties not factious to hold, precepts not penal to disobey."

SNOWSTORMS.

We have been subject, at long intervals of ten years, to serious and destructive snow storms, which have seriously damaged our overground wires. The wires constructed and maintained by the post office did not suffer very seriously, but those maintained along the railways out of London, and those erected by the telephone company overhead, were severely handled by the elements. In fact, in London, many unrepaired telephone wires are still seen dangling over the housetops. No accident to person has been recorded, but the very serious interruption to communication has directed earnest attention to the necessity of putting

* Abstract from a paper read before the Society of Arts, London, May, 1887.

more wires underground. It has always been the practice of the post office to do this in London and large cities, but not only the excessive cost, but the diminished speed of working, has hitherto prevented its being done to any large extent in the country. The postmaster-general has now under his consideration a scheme for using underground wires more extensively, and there is nothing whatever to prevent this being done by the telephone companies. In fact, in many cases, telephones work better underground than overhead. The laws that govern the transmission of speech are now thoroughly known, and the fancied difficulties in using underground wires have vanished into thin air.

In London alone we have 255 miles of pipes, containing 10,212 miles of wire. In fact, all our great trunk lines are out of danger from stoppage from storms. We have 868 miles of open wire included within the metropolitan area, but these are chiefly in the suburbs, and include long outlying sections, used either for police or fire brigade wires, or for private persons.

There are 213 offices in London now served wholly by buried wires. It will be seen, therefore, that the post office has been fully alive to the drawbacks attending the existence of overhead lines in crowded centers. It is steadily pursuing the same policy, and although some open wires must exist if telegraphs are to exist at all in certain localities, still the overhead proportion, as compared with the underground, steadily diminishes.

Comparative Return, showing the Mileage of Line and Wire in the United Kingdom.

Year.	Overhead.		Underground.	
	Line.	Wire.	Line.	Wire.
	Miles.	Miles.	Miles.	Miles.
1877...	23,766¼	101,627¼	394¾	8,013¾
1878...	24,438¾	102,074	445¾	9,023
1882...	25,001½	111,811¾	478¾	10,993¼
1886...	26,425	150,590	677¾	19,605

To provide a scheme extending throughout the kingdom to only connect the more important towns, and uniting those towns by less than half the existing number of overhead wires, would cost something like £2,500,000, and in these days of attenuated exchequers such an outlay is very serious to incur, although it may effectually guard against stoppages ranging from three days to three weeks, once in five years or so.

TELEPHONES.

In 1877, a most striking departure in telegraphy was made by Professor Graham Bell, in America, who showed that it was possible to reproduce for commercial purposes the human voice at great distances. The telephone sprang into existence almost perfect in its action, and with the improvements immediately afterward introduced by Mr. Edison and Professor Hughes, it has continued to progress with giant bounds. In America there were on December 31, 1886, 353,518 telephone receivers and microphone transmitters in use and under rental. The total number of subscribers to exchanges is 147,068. In Europe the number is very great. The number of exchanges in England is 184; there are 19,784 subscribers, but there are 101,000 telephones in use as royalty-paid receivers and transmitters.

Stockholm has shown a remarkable development. With a population of 210,000—about the size of Edinburgh—there are over 5,000 subscribers to the exchange, while in Edinburgh there are only 312

PNEUMATIC TELEGRAPHS.

The Electric Telegraph Company introduced, in 1854, a pneumatic tube of lead protected in iron pipes, between their central station in Lothbury and the Stock Exchange, through which the messages themselves, in small leather carriers, were driven. The system proved so economical and rapid that it was extended in the City of London, Glasgow, Liverpool, and in Manchester.

At the transfer there were altogether 2 miles 1,625 yards of pipes laid down, but now there are in :

London	32 miles,	1,209 yards.
Birmingham	0 "	917 "
Liverpool	5 "	1,593 "
Newcastle	0 "	1,204 "
Glasgow	1 "	768 "
Dublin	1 "	1,013 "
Manchester	2 "	233 "
Total	44 miles,	1,737 yards.

The longest tube is that between the central station, general post office, and the House of Commons (3,859 yards), through which 700 messages are sometimes sent, each carrier taking about six minutes per journey, but intermediate signalers being employed, carriers can be dispatched every two minutes. In most cases there are two tubes, one for sending and the other for receiving—through one the messages are blown, through the other they are sucked—the engines and pumps being in all cases in the central station. At the time of the transfer the engine power employed was as follows :

	Engines.	h. p.
London telegraph station	1	20
Birmingham	1	6
Liverpool	1	8
Manchester	1	10
Glasgow	1	6
Total nominal horse power		50

The engine power employed at present is as follows :

	Engines.		h. p.
London—St. Martin's-le-Grand—condensing engines. (Three are working and one spare)	4	each	50
Liverpool—condensing engines (two are working and one spare)	3	"	30
Manchester—(one is working and one spare)	2	"	10
Birmingham	1		6
Glasgow	1		20
Newcastle-on-Tyne	1		8
Dublin—(one is working and one spare)	2	each	10
	14		134

This gives a total of 14 engines, and a total of 364 nominal horse power for working the 112 pneumatic tubes now in use.

SUBMARINE CABLES.

GOVERNMENT ADMINISTRATIONS.	No. of Cables.	Length in nautical miles.
Germany	35	461
Austria	31	96
Denmark	36	123
Spain	3	127
France	46	3,197
Great Britain and Ireland	104	876
Greece	45	457
Italy	22	613
Norway	236	228
Netherlands	20	59
Russia in Europe and the Caucasus	5	201
Sweden	9	61
Turkey in Europe and in Asia	8	830
Cochin China	3	810
British India, Indo-European Telegraph Department	5	1,718
British India, Indian administration	67	155
Japan	11	55
Russia in Asia	1	70
South Australia	5	49
New Caledonia	1	1
Dutch East Indies	1	31
New Zealand	3	196
British America	3	200
Brazil	19	19
Total	719	10,142

SUBMARINE CABLES.

COMPANIES.	No. of Cables.	Length in nautical miles.
Submarine Telegraph Company	10	803
German Union Telegraph Company	2	1,119
Hamburg - Heligoland Telegraph Company	2	40
Direct Spanish Telegraph Company	2	699
Spanish National Submarine Telegraph	5	1,172
India Rubber, Gutta Percha, and Telegraph Works Company	2	122
West African Telegraph Company	11	2,825
Black Sea Telegraph Company	1	351
Indo-European Telegraph Company	2	14
Great Northern Telegraph Company	20	6,108
Eastern Telegraph Company	53	18,838
Eastern and South African Telegraph Company	5	4,554
Eastern Extension, Australasia and China Telegraph Company	21	12,035
Anglo-American Telegraph Company	15	10,437
Direct United States Cable Company	2	2,983
Compagnie Francaise du Telegraphe de Paris a New York	4	3,409
Western Union Telegraph Company	4	5,537
Commercial Cable Company	6	6,937
Brazilian Submarine Telegraph Company	6	7,326
African Direct Telegraph Company	7	2,739
Cuba Submarine Telegraph Company	3	940
West India and Panama Telegraph Company	20	4,119
Western and Brazilian Telegraph Company	9	3,801
River Plate Telegraph Company	1	32
Mexican Telegraph Company	2	709
Central and South American Telegraph Company	9	3,178
West Coast of America Telegraph Company	7	1,698
Total	231	102,531

SUMMARY.

OFFICES.	No. of Cables.	Length in nautical miles.
Government administrations	719	10,142
Private companies	231	102,531
Total	950	112,673

CABLES.

The exclusion of private enterprise from telegraphic undertaking in these islands does not apply to our colonies and to the ocean. The growth of submarine telegraphy has been enormous. We read in our newspaper every morning the previous day's doings in every quarter of the world. The *Times* of Monday morning last had two and a half columns from Philadelphia, one column and a half from India, a dispatch from Natal, another from Melbourne, and news from every capital in Europe.

In 1851, two or three far-seeing individuals, prominent among whom was our own vice-president, Mr. T. Crampton, risked their capital in laying a cable between Dover and Calais. Now there are 112,673 miles of submarine cable resting on the bottom of the ocean, which have absorbed a capital of £37,000,000

No less than nine cables cross the Atlantic. All our important colonies are in connection with London—the heart of the world. Laying and repairing has become a simple and a certain matter in any depth and in every sea. A whole fleet of ships—over thirty—are maintained for the purpose. Submarine telegraphy has become a solid property—the main result of British skill and British enterprise—unaided by government support. The apparatus used is principally Sir William Thomson's recorder.

RAILWAYS.

The monopoly which the postmaster-general possesses regarding telegraphy only applies to message carrying for profit—it does not apply to those numerous wires that are required for the protection of life on railways. Each of our large railway companies has a distinct telegraph system of its own employing a very large staff, and used for the purpose of regulating its own traffic. There are about 80,000 miles of wire erected for the purpose, and probably 20,000 instruments in use of various kinds. The apparatus used for telegraphy is invariably the needle, though generally on the wires connecting the post offices sounders are employed. The apparatus used for block working is very various. Every great railway out of London has a different system. The survival of the fittest has not yet asserted itself. But they all work well, and without electric signaling the working of our railway system would be absolutely impossible.

FINANCE.

The financial position of the telegraph business is sound. The amount of capital debited against telegraphs is £10,140,000; of this £7,000,000 was the purchase money, the rest has been expended in extensions and in furthering the business. If the trust which the post office undertook on behalf of the public were handed back to the care of private enterprise, we should hand over a going business drawing a gross revenue of nearly £2,000,000 per annum, and assets which may be estimated at £4,000,000. If financiers were willing to pay several millions for a single brewing business in Dublin, what would they pay for a grand imperial monopoly, serving every town in the United Kingdom, and of which every person in these islands is a customer?

It is very much the fashion to decry the terms of purchase. Doubtless, the terms paid were very high, and the post office authorities who negotiated the purchase were unnecessarily hurried, and perhaps overmatched, by the railway companies, for they had to pay twice over for certain privileges. Even Mr. Fawcett proposed to wipe out as wasted capital the excess paid for telegraphs, and call it a bad debt. As the bargain stands, the public have not been losers. A government department cannot compete in economy with an ordinary commercial firm subject to competition; nevertheless, the business done pays a dividend on the capital expended. The balance of profit for the year ending 31st March, 1886, if our accounts were made out on commercial principles, was £160,000.

It is amusing, after this length of time, to read the arguments that were adduced against the absorption of the telegraphs by the state. Every reason has been proved wrong, every prophecy has remained unfulfilled. I can say this with a good grace, for I was one of the prophets.

The advantages of a state-controlled telegraph system have been amply shown. There has been established a cheaper, more widely extended, and more expeditious system of telegraphy; the wires have been erected in districts that private companies could not reach; the cost of telegrams has been reduced, not only in their transmission, but in their delivery; the number of offices opened has been trebled; a provincial and an evening press has been virtually created. Adam Smith said that the post office was the only kind of business that government had always managed with success. We can now add telegraphy.

In the discussion which followed, Mr. J. W. Batten said he was more acquainted with telephony than with telegraphy, being the recipient of one of the first instruments made by Professor Bell, and one of the originators of the United Telephone Company. Comparing telephony in England and America was like comparing an eagle in the Zoological Gardens with one in a state of freedom. Telephony in England was restricted by the post office, but if it were allowed one-quarter of the free play it had in America, very different results would be seen. When he was in California last year, and in Buenos Ayres the year before, he paid special attention to the working of the telephone, and in both places he found it had developed because it was allowed to do so. Here he could not telephone to a shopkeeper to call a cab for him, because the post office said it was a message. In America he could call to a shopkeeper, "Be kind enough to send a cab to my house," and it was done at once; and he was told the cab was coming. In America he could send a message to a commissionaire at one of the offices to send some one to execute a commission, such as to fetch his dress clothes from home, but he could not do that in England, or if he could, he could not send a message to his wife to say where he was going to dine. In San Francisco he counted forty-seven stable-keepers on the telephone, one hundred and eight doctors, and about seventy-four chemists, and the way it was worked was this: Almost every respectable house was on the telephone, and if the doctor was wanted he was telephoned for. While dressing he telephoned for a cab, which was at the door by the time he was ready, and immediately he had seen the patient he telephoned the prescription to the chemist, and in a few minutes the medicine was at the house. In every street in America you found a shop in which were two or three young ladies with telephones, and for 2½d. one of these young ladies would send a message for you or you could do it yourself. At San Francisco there was communication with one hundred and two towns and villages in the neighborhood, one of them, Sacramento, being 104 miles distant. In fact, everything was done to popularize the telephone, and it was entirely free from government supervision or control. Invention always succeeded where there was freedom. It was very respectable to be in livery, but it was much better to be free. He did not say that the post office authorities were not, to a certain extent, right in handicapping the telephone, having bought certain telegraphic rights; but when the public blamed the telephone companies, they should remember that they were not allowed to go along a single line of railway, to enter a railway station, to call a cab, or scarcely to do a single thing which was done in America.

Mr. Alfred Carpmael said it would appear from Mr. Preece's papers, given there from time to time, that they ought to be very much obliged to the post office for having taken over one of the great industries of the country, and he was not disposed to deny that in some cases there might be some slight advantage; isolated villages might have got the telegraph a little sooner than they would have had it if the company of which Mr. Preece was so able an official had still continued to

carry on business; but he very much doubted whether they had the sixpenny telegram as soon as they would have had it if the government had not taken them over. The matter was actually under consideration in 1869. There was one fact which evidently weighed even with Mr. Preece. England at one time was in the foremost rank of inventors. Cooke and Wheatstone got a telegraph to work before any one else did, but from 1869 in this country not one single invention of importance had been produced. As an Englishman, he felt humiliated when he heard such name as Stearns, Delany, and Bell —for we had no one to put beside them. Why was this? Mr. Preece said it was because, whenever they wanted an invention, there came just in the very nick of time some one from America with one. But supposing there had been a government monopoly in America; he doubted whether the some one would have stepped in, in the nick of time, and probably telegraphy would have progressed as slowly in America as it had done here. At the close of the paper, Mr. Preece said that telegraphy was free in the colonies, and that submarine telegraphy had progressed there with immense strides, because there was freedom, and people might do what they liked. He believed, whatever their obligations to the post office might be, they must at least make a deduction for its having stifled inventive genius. Inventors frequently came to him, sometimes with electric telegraph inventions, and said they were told it was no use patenting them, because the government would not touch an untried thing, and they could not try them themselves. If an American came to the government, he had this advantage, that he had been allowed to try his plan in his own country, and it had proved a success, and he was therefore in a position almost to compel the government to take it up. One thing he was glad to hear from Mr. Preece, and that was the right of the public to grumble, and that if their grumbling had any justice in it, it would be attended to. He was extremely glad to hear that, because he had that day addressed a complaint to the department on his own account. A regulation had been passed in his neighborhood, that a telegram with the same address as letters which found him by every post should not be delivered, unless he paid an extra sixpence for what they were pleased to term redirecting. He was quite satisfied that it was not an unmixed advantage that the telegraphs were handed over to the government.

Mr. Preece said he always felt a certain amount of satisfaction in bringing forward anything connected with the post office, because it gave everybody a chance for a growl, and fortunately, the complaints were very easily answered. Taking Mr. Batten first, this was an old subject of his, the restrictions which the post office put on the telephone, and the greater freedom which prevailed in America. He (Mr. Preece) had been to America especially to look into this question, and he found that in America a telephone company could not erect a post in the street of any town without paying a tax of five dollars, nor carry a wire through a street without being taxed four or five dollars a mile for it. In many States, instead of having to pay a tax of ten per cent. to the government, they had to pay more like twenty-five or even thirty per cent., and in one State—Indiana—the government not only restricted the passage of the wires through the town, but restricted the charge to be made to subscribers; so that at present there was not a single telephone at work in a country as large as England, because the companies would not submit to this grand freedom that Mr. Batten spoke of. Had Mr. Batten ever tried to call a cab by telephone? Why, long before he could get his call answered by the young woman at the exchange, a messenger would have gone to the cab stand and brought the cab. The reason why cabs and doctors were not called was not because there was any restriction, but because the thing was so badly worked. A short time ago he was in the North with a friend, and after dining comfortably, they went into the smoking room, where there was a table laid out with telephones upon it, and on applying them they could hear the opera. Could that be done in London? and was it on account of the post office restrictions? If you wanted to know how to work the telephone, you must go away from London, to Newcastle or Liverpool, and away from Mr. Batten's company. Here, if you got hold of a telephone, you had to wait a long time before you were in communication, and then you were constantly interrupted by other persons' messages. Similar restrictions to those in London also existed in Paris, but there were many more telephones in use there.

Mr. Carpmael said there were no inventors in England, but it must not be forgotten that the inventions of Sir William Thomson were used everywhere, as was also the microphone invented by Professor Hughes; in fact, all the great improvements, from those of Varley down, were brought out in England. The most important circuit inventions which were in use in every capital of Europe had been brought out in England. The officials of the post office did not patent inventions, and, consequently, any improvement brought out by them was not identified with any name. The paper itself contained sufficient evidence to refute any notion that invention was stifled in England. On the contrary, he believed the paper proved the very reverse.

THE MELOGRAPH AND MELOTROPE.

If it required a new proof of the fact that intelligence does not suffice to secure success when it is not seconded by energy, perseverance, and labor, we might find it in the history and genesis of the apparatus that we now have the pleasure of presenting to our readers.

Separate solutions, more or less perfect, of the two problems of registering musical improvisations and mechanically reproducing music have existed for a long time, but it was for Mr. Jules Carpentier to solve the first of these by uniting both of them, and thus giving them a practical value that they would not otherwise have had. The first researches of Mr. Carpentier date back many years. His first apparatus was a repeating melograph (applied to a harmonium), which perforated strips of paper, and the music was repeated by means of an electric harmonium. Through gradual simplification and incessant improvement Mr. Carpentier has now succeeded in registering the musical improvisations played upon any keyboard instrument, and in reproducing them through any keyboard instrument whatever by means of apparatus that are admirable for their simplicity and perfect operation, and in which the skill of the constructor disputes with the ingenuity of the inventor.

Mr. Carpentier's device, as a whole, embraces three apparatus:

1. The melograph, an apparatus that electrically registers the music played.

2. The perforator, a translating apparatus that perforates the paper bands of the melograph and renders them suitable for the melotropic reproduction.

3. The melotrope, an apparatus which, placed upon any piano whatever, permits of reproducing musical improvisations, without requiring any other talent than that of knowing how to turn a winch with regularity.

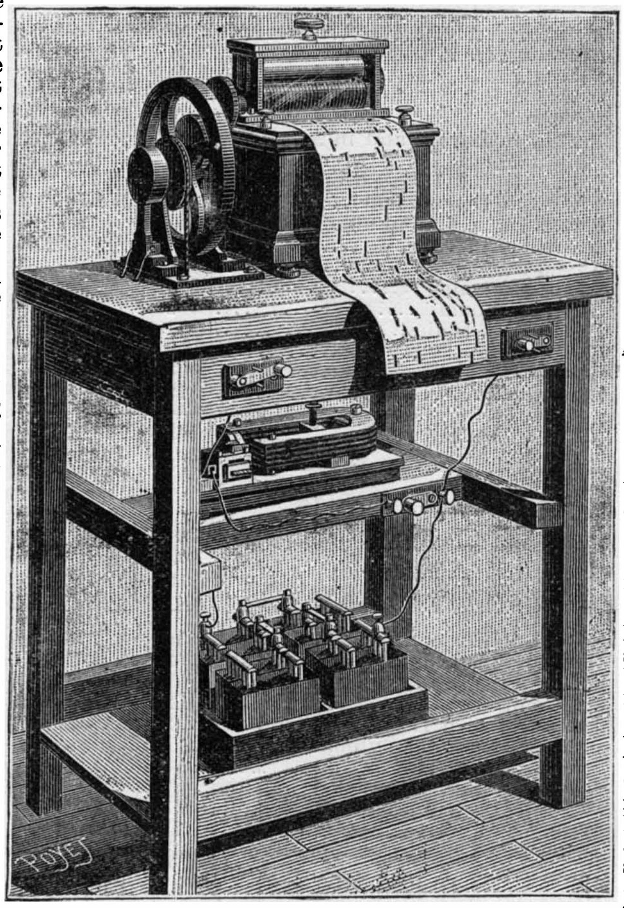


FIG. 1.—THE MELOGRAPH.

1. *The Melograph.*—The melograph is designed to preserve the traces of all motions given to the various keys of a keyboard during the execution of a piece of music. It forms an entirely independent apparatus, which is simply put in connection with the keyboard by a bundle of wires, each of which corresponds to a key and through which electricity serves as the agent of transmission.

Being given the nature of the phenomenon to be registered, that is, the depression of a key, and the mode of transmission adopted, that is, electricity, the problem is naturally reduced to a question of chronography that the Morse telegraph completely solves. So the melograph might well be compared to a Morse telegraph, and the arrangement and operation of it cannot better be understood than by a reference to that apparatus. It furnishes inscriptions in ink upon a continuous band of paper, which must be ideally considered as the union of a certain number of narrow bands, each of which is reserved for a key of the piano. One of the main difficulties that had to be conquered was the reduction of the width of the elementary bands to a minimum, while at the same time rendering their operation sure. In the model here described, each band measures but 3 mm., say 12 cm., including margins, for thirty-seven notes or three complete octaves.

The melograph comprises three parts. The first of these is the transmitter, which is placed under the keys in the narrow, but sufficient, space that is found in all pianos. It is in the form of a narrow wooden ruler carrying a series of flexible strips, each of which is placed under a key, and, lowering and rising with the key, establishes a contact that is so much the more prolonged in proportion as the key itself is kept depressed.

The second part is the motor designed to move along the band regularly upon which the inscription is made. This motor is an electric one, and is actuated by six small accumulators. What distinguishes it is its regulating parts. A fly wheel of an exaggeratedly massive aspect absolutely prevents such disturbances in the velocity as might tend to occur on the entrance into line of any number of tracing devices.

The velocity regulator consists of a centrifugal force apparatus, which, when the velocity tends to exceed a certain limit, breaks the circuit of the accumulators and thus suppresses the motive power. The speed tends to slacken, the contact closes anew, and so on. In practice, these variations do not exist, and the system assumes a state of equilibrium corresponding to an imperfect contact that lets a sufficient current pass to keep the velocity constant, not only in the course of one experiment, but from one experiment to another, whatever be the interval between them. The velocity with which the paper moves is three meters per minute.

The third part of the melograph is the receiver, the principal parts of which may be seen in Fig. 1. A channeled cylinder placed above the band of paper may be considered as a union of a series of pulleys, which, being constantly inked by a roller, placed above them, represent so many inking rollers always ready to deposit the visible traces of the transmitted signals upon the paper.

Under the paper, a series of vertical styles, each opposite a roller and each actuated by an electro, lifts the paper and presses it against the inked rollers every time the corresponding keys are depressed. To this effect the thirty-seven electros are connected with the contacts of the corresponding keys of the piano by thirty-seven wires that serve to close the circuit of each of the electros upon the accumulators, the return current passing through still another wire. The connection between the piano transmitter and the melograph is made by a bundle of thirty-eight wires terminating at the two extremities in contact combs set into each of the apparatus and simultaneously establishing all the communications.

The paper is carried along regularly by the edges, through rollers that are set in action or stopped by a gearing arranged in such a way that the paper can be unwound only for the time strictly necessary without the motor being stopped. The inking roller has a longitudinal to and fro motion that has the effect of securing a regularity in the inking of the small rollers. When the apparatus is not in operation, the inking roller is moved back so as to prevent soiling.

A large number of arrangements, upon which it is unnecessary to dwell, permit of rendering the apparatus of easy management. The model presented by Mr. Carpentier to the learned societies has been operating nearly every day for almost a year without accident or hindrance. It is, therefore, a truly practical apparatus, and one which solves the problem of the automatic inscription of music in as perfect and complete a manner as could be desired. In such melographic inscription each note is represented by a line whose position, with respect to the edges of the sheet, corresponds to its musical pitch in the gamut of the piano, and whose length corresponds to the duration of the note. The *motifs* formed by the succession of the notes in the continuity of time thus find a representation at once faithful and expressive in the characters that are formed in the space occupied by the inscription. Fig. 5 shows an example of such writing.

But melographic writing, though very satisfactory in theory, could not be of any practical application. Although it contains all the elements of the measure, it contains them masked by the thousands of irregularities that sentiment as well as the hesitation or want of skill of the musician introduces, and permits in no wise of seizing that simple and definite accordance that the common notation shows so well. On another hand, as regards musical grammar, the inscription thus obtained is, with respect to the piece that the musician would really have written on staves, what stenography is to speech itself. In a word, a composer put in possession of a melographic inscription of one of his productions would be incapable of reading it on the stand, and, in order to translate it into common notation, would have to devote himself to a long, troublesome work of interpretation.

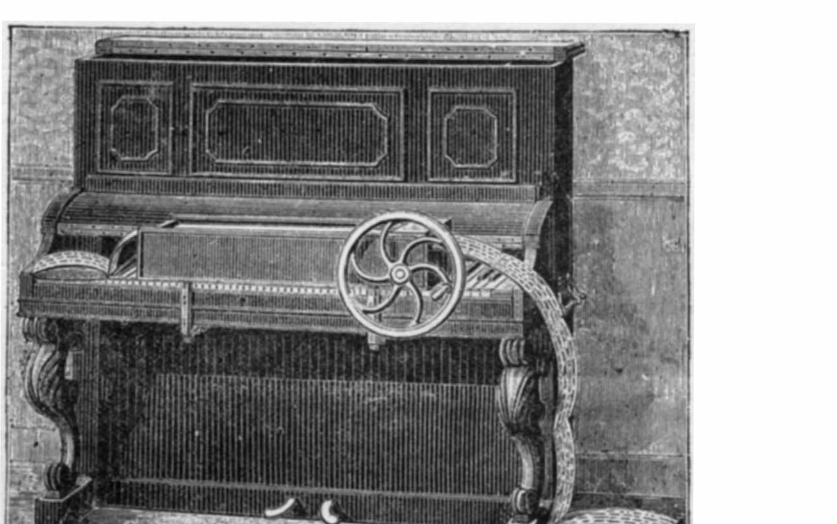


FIG. 2.—MELOTROPE ARRANGED UPON A PIANO.

In order to get around the difficulty, and render the valuable data furnished by the melograph at once utilizable, but one way presents itself to the mind, and that is to ask mechanics to do what the composer cannot, that is to say, to reread in a loud voice the productions registered.

It was for solving such a problem that Mr. Carpentier devised the melotrope and the mechanism called a perforator.

The Perforator.—The role of the perforator is identical with that of the translator in ordinary life, and may be defined thus: Being given an apparatus capable of tracing merely musical writing of a form, A, and a second that reads another writing of form,

B, to convert the writing, A, into the writing, B. The perforator plays such a role by converting the printed band into a perforated one by means of punches maneuvered by the hand of the operator. In reality, the music is inscribed upon a band of fine paper and mounted upon a thicker sheet of paper of sufficient

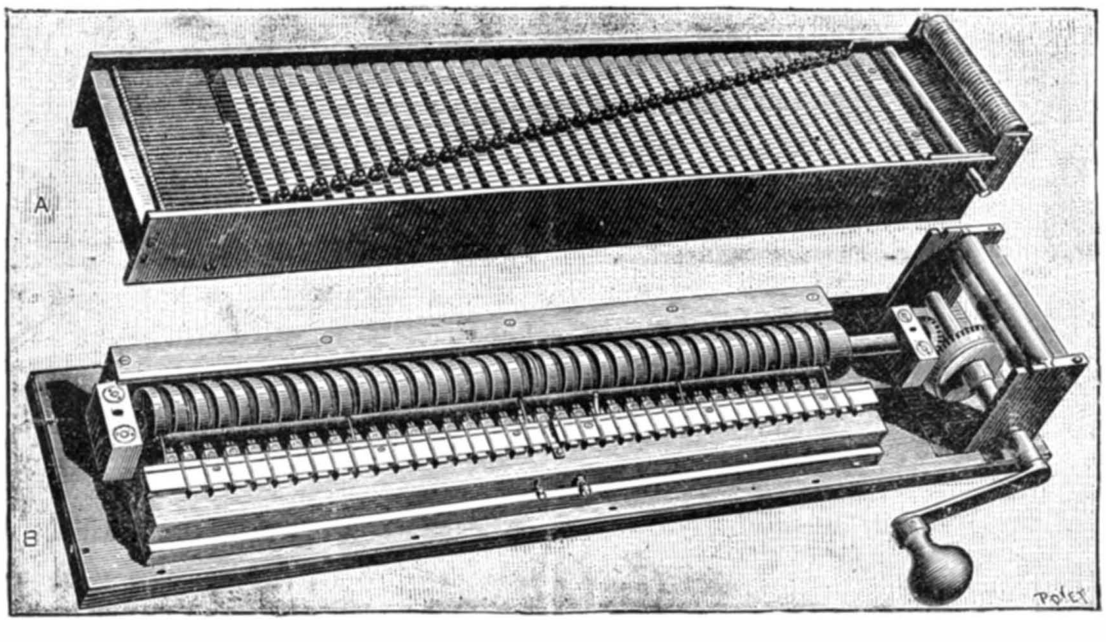


FIG. 3.—INTERIOR OF THE MELOTROPE.

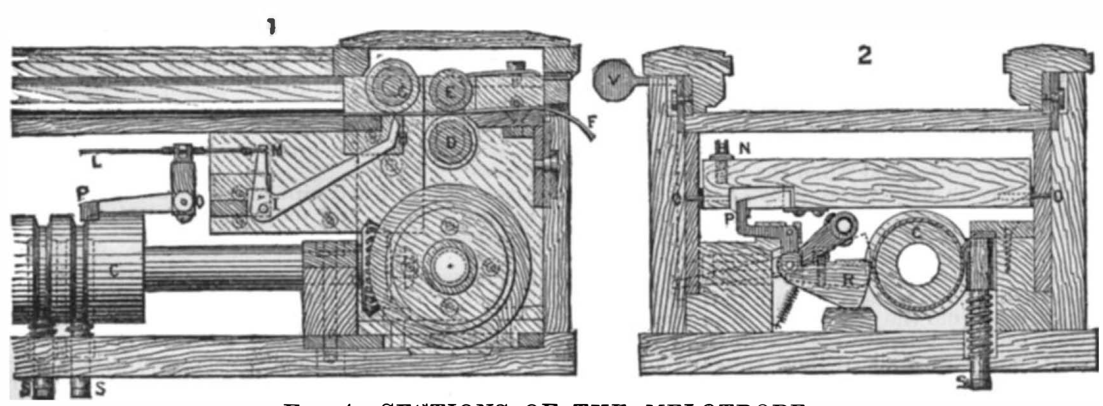


FIG. 4.—SECTIONS OF THE MELOTROPE.

stiffness to actuate the melotrope, and that too for an indefinite number of times.

This perforator is maneuvered by hand but once. For reproducing a piece of music a great number of times, Mr. Carpentier has devised an automatic perforator, in which the first perforation is used as a model.

If a few errors or false notes slip into the execution of the piece, it is easy to correct them by pasting pieces of cardboard on the typical band, and making new apertures in the proper places. Fig. 6 shows the perforated bands on a reduced scale. Their width is 12 cm., and their length depends upon that of the piece played, at the rate of three meters per minute.

The Melotrope.—As its name imports, the melotrope is an apparatus that reproduces melody or music by a rotary motion. The apparatus is of small size as compared with analogous systems. Fig. 2 shows it fixed upon an ordinary piano and ready for operation. Fig. 3 shows its internal arrangement. It consists of an oblong box placed on brackets over the keyboard of a piano or organ. When once the apparatus is in place, we have an automatic piano.

Within the melotrope there is a thirty-seven times repeated mechanism that permits of translating every aperture in the band of paper through a depression of the corresponding key. On turning the winch, B (Fig. 3), we revolve a wooden cylinder, C (Fig. 4), that occupies the entire length of the apparatus, and two cylinders,

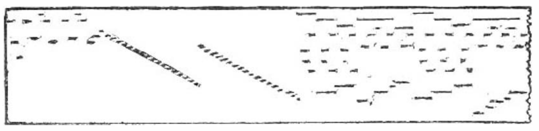


FIG. 5.—MELOGRAPHIC WRITING.

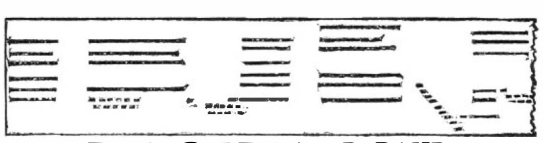


FIG. 6.—PERFORATED BAND.

D and E, which carry along the paper from left to right with a velocity equal to that of the unwinding of the band of the melograph, say three meters per minute if the operator turns the winch very regularly at its normal velocity. The notes are struck by fingers, S, covered with buckskin, which descend upon the notes and actuate them every time a perforation corresponding to the note struck passes before the cylinder, C (Fig. 4).

The perforations favor delicate actions only, while the *touché* of the note, especially in the forte, requires a strong presure. It therefore requires an intermedium for converting the feeble stress exerted near the paper into a strong one exerted by the finger on the key.

For solving this problem we have in the melotrope two mechanical arrangements, whose role is entirely distinct. One of these is a gearing device and the other a servo-motor. The object of the former is to translate each of the perforations in the paper into a slight mechanical movement of a finger, P, placed opposite each note and at a certain distance. To this effect, the paper band, F, carried along by the cylinders, D and E, presses against another cylinder, G, provided with grooves into which can enter the lever ends, H. With a continuous paper having no apertures these levers are held in a normal position; but when an aperture presents itself, the lever arm, pulled by a spring, describes a slight motion around the axis, I, in a direction from right to left. In Fig. 3 may be seen all the springs and steel wires that connect the apices, M, of the levers with the springs.

Upon each of the wires, M and L, and at a distance varying with each note, there is a second lever, N O P, whose extremity, P, which is covered with woolen, drops a few millimeters. The fingers, P, may be likened to those of a performer playing a selection with insufficient power to actuate the keys.

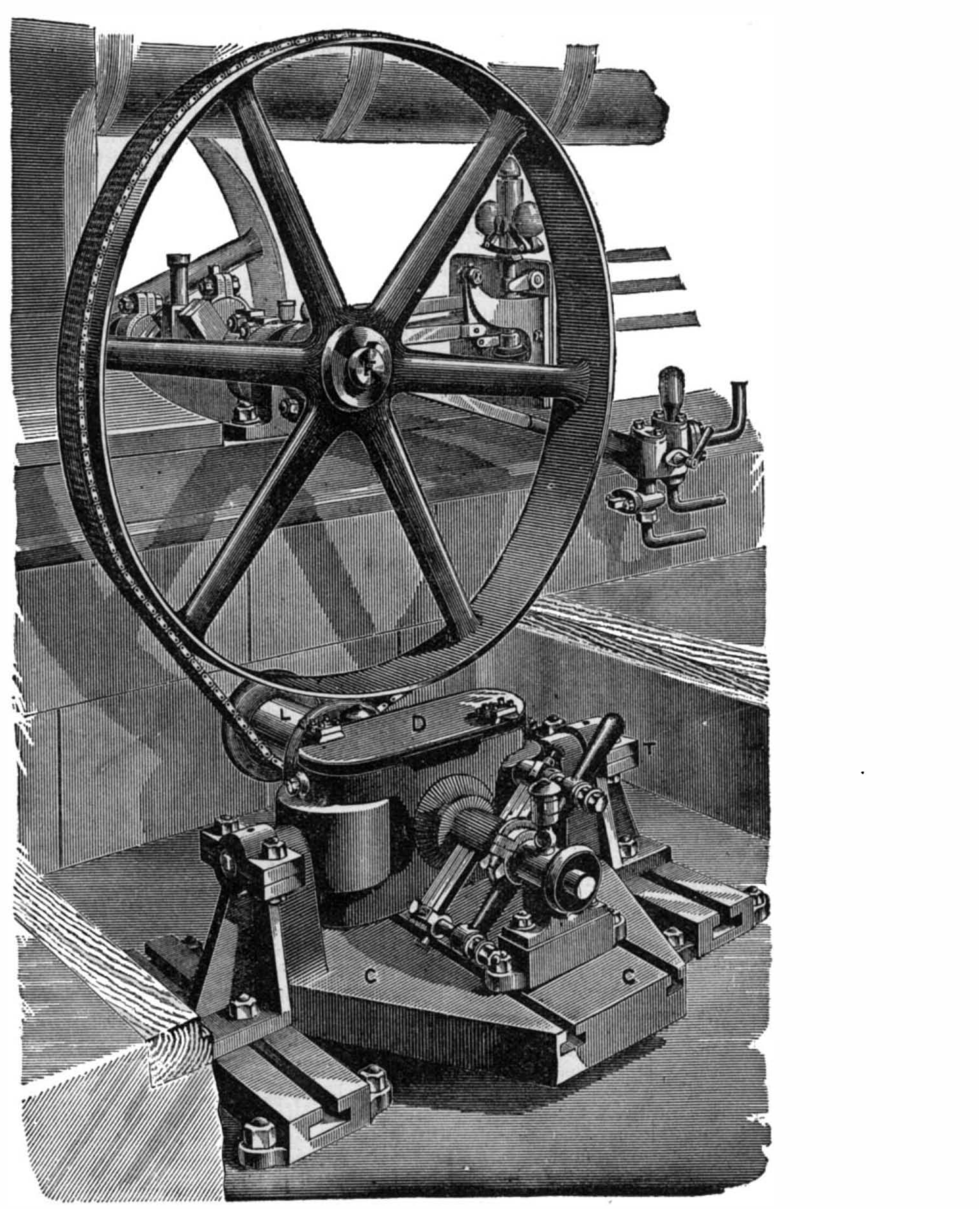
The servo-motor is an intermedium that borrows the necessary energy to allow such performer to make the instrument resound from the work expended on the winch to make the cylinder, C, revolve. To this effect, there is fixed to each finger, by one of its extremities, a cord that makes two and a half revolutions in a groove in this cylinder, and is attached by the other extremity to a small wooden sector, R. The circumference of the latter is, at rest, very near the surface of the motive cylinder, but does not touch it, so that the motive cylinder can revolve without carrying along the sector. If, through the play of the gearing, the sector chances to be led, by a slight motion of the fingers, P, into contact with the motive cylinder, it rises through friction, and exerts a tension on the cord affixed to it, and at the other extremity of the cord we obtain an incomparably greater force, the effect of which is to depress the finger and the corresponding piano key. Just as an approach of the sector brings about a gearing and a forward motion of the finger, just so does a slight recoil of the sector produced by the recoil of P permit of an ungearing and a backward motion of the finger through the action of a spiral spring.

The entire principle of the apparatus resides in this ingenious application of the laws of friction.

Expression, that is to say, the musical sentiment that characterizes a composition and gives it its whole value, resides partly in the irregularity of the measure and partly in the energy with which each key is struck, the pedals of the instrument naturally preserving their special role, since they remain entirely independent of the melotrope. Expression in the measure is rendered perfectly, and this is what so clearly distinguishes the combination of the melograph and melotrope from all former automatic devices of the kind. Expression in the intensity of the sound struck is obtained by limiting the depression of the key. In front of the apparatus, there is a small button, V (Fig. 4, 2), which, through a combination of levers, depresses or lifts a rod that limits the travel of the sectors, and consequently that of the fingers and keys. Such are the principal arrangements of Mr. Carpentier's ingenious and remarkable apparatus. Among the numerous applications in store for them, we may mention one that is essentially philanthropic, and that is the reproduction of improvisations played by the blind, in whom the musical sentiment is generally so highly developed. The compositions thus registered will permit of a host of delightful pieces of music being preserved and distributed.—*La Nature.*

NEW DRIVING GEAR FOR DYNAMOS.

AT a recent meeting of the Society of Telegraph Engineers and Electricians, Professors Ayrton and Perry described their new method of driving a dynamo with a very short belt. When such machines are driven in the usual way by belting, there is a certain waste of power in overcoming the friction of the driving spindle and its bearings, created by the pull of the belt, which becomes greater when the space is confined, as in that case the belt must be put on with additional tension. The usual method of driving dynamos by a horizontal belt has also the inconvenience of requiring considerable floor space, and to overcome these objections Professors Ayrton and Perry have devised a method of driving which is illustrated in the accompanying engraving. The dynamo is placed below the fly wheel, its pulley coming within an inch or so of the periphery of the latter, and it receives motion from a very short belt, as shown. If the dynamo were rigidly fixed in this position, the initial tension of the belt would have to be very great indeed to trans-



NEW DRIVING GEAR FOR DYNAMOS.

mit the necessary power; but by pivoting the whole frame of the dynamo in trunnions very smooth driving is obtained, with comparatively small tension in the belt and little strain on the spindle. Our illustration shows a hole cut in the floor, into which is placed a fixed cradle having bearings for the trunnions, T. The dynamo, D, is mounted in the cradle, C, hanging from these trunnions, and slots are provided in the cradle for shifting the dynamo in a direction at right angles to the axis of the trunnions, so that the weight can be more or less overhung on the pulley side, by which means the initial tension of the belt is adjusted. The dynamo is not placed plumb under the center of the fly wheel, but to one side of the center line, so that the tight side of the belt forms a greater angle with the horizontal than the slack side. By suitably selecting the distance from the center line, the component of the two tensions in the belt can be made to pass vertically upward through the center of the pulley, thus relieving the bearing of the weight of the pulley and of that of a portion of the armature. An extra heavy pulley is recommended for this type of gear, and if it should be found impossible to put sufficient weight in the pulley, the dynamo can be shifted on its cradle in the manner above alluded to, so as to bring additional weight upon the belt. A gear of this description has been running for some time with perfect success in the laboratory of the City and Guilds Central Institute.—*Industries.*

MICROSCOPICAL RESEARCHES INTO THE CAUSE, ORIGIN, AND PROPAGATION OF DIPHTHERIA.

By Dr. CARL BUNSEN, Galesville, Wisconsin.

YEARS have passed since I began to study the microscopical causes of diseases, and my thoughts have been engaged ever since in finding the cause of diphtheria. We know of many instances where a loving mother, by giving the parting kiss to the little darling just breathing its last upon earth, has taken the deadly contagion into her own system, thus innocently helping to spread a destructive epidemic.

When we examine the throat of a child that has died of diphtheria, we are at a loss to account for the cause, as we do not find anything except perhaps little, shining, transparent dots in the muscular layers below the *epithelium.*

We find, further, that the epithelium of the throat has been partly destroyed. Again, in other places where the epithelium has not been destroyed, the epithelial cells are transformed into a leathery, tough-looking mass, that is closely dotted with the same kind of globular bodies, scarcely perceptible through a powerful microscope.

Microscopy teaches us that these are the microbes or micrococci of a parasite; but with all our advanced knowledge, we are not able to produce any propagation of these microbes. We shall see if we can find the reason for this.

If a child dies of diphtheria croup, and we examine the bronchial tubes and the trachea, we find the same closed, but not with a tough mucus, but with the same mass of opaque, epithelial cells, which often reach into the smaller tubes, so as to prevent respiration, thus producing asphyxia.

It is not my purpose to describe how the diphtheria dead look after death, as the reader can find this in books on pathology, which will nevertheless give him no sure microscopical cause.

I, therefore, concluded to make my experiments (the description of which will follow) upon living animals. Vivisection is the only means to study the causes of diseases. By examining a cadaver, we may learn what destructive changes have occurred, but we are in most cases ignorant as to the causes of the same. These little microscopical causes that have produced these destructive effects usually fall to pieces as soon as the supply of blood and animal heat ceases, and leave perhaps only their immature microbes behind. If we consider that diphtheria is physiologically and pathologically closely connected with the so-called *specific fevers*, we may learn from the observations of the microscopical anatomy of the local lesion that this lesion is chiefly connected with infiltration of the affected tissue with a granular substance, which, by closer inspection, is found to consist of microbes.

How these microbes get there is a matter of speculation, as these minute bodies can be detected in all stages of disease and in all affected tissues.

We find them in the smallest and most superficial spots. We find them deposited in millions in the tissues, and we notice, further, that the greater the quantity of micrococci present, the greater will be the toxical affection. Diphtheria always commences with a severe catarrhous affection of the larynx, fauces, and, afterward, of the nares, and it was this affection which guided me in my examinations. This catarrh may last sometimes for weeks before we find that diphtheria has taken possession of the patient. Let us, therefore, study the nature of catarrh. We find in nature different kinds of cells that produce catarrh, and we must classify these cells as ferments. We shall only study the nature of two kinds, which will represent all the rest.

First, if an invasion of common, ordinary catarrh cells has begun, we find that the mucous membrane becomes covered in a few hours with millions of minute, round, globular bodies, which are fastened to the underlying epithelial cells by two minute, hook-like appendages or rootlets, being covered at the same time with mucus, which protects the cells against the influence of saliva. When we examine these little, round bodies on a *living* subject, we see that the inside of these cells contains a clear blastema. After a while, we see that upon the surface of these cells there appears a nucleus, and, just before bursting, we discover from three to five nucleoli.

These little cells will, during their growth and propagation, absorb the nitrogen and water at the expense of the mucus, which, in consequence of this process of fermentation, will be soon transformed, and can then no longer protect the little fermenting bodies against the ptyalin of the saliva, which soon puts a stop to their life, and the catarrh ceases, and the invaded parts become normal again. In those places which the saliva is not able to penetrate, the process of fermentation ceases on account of exhaustion, and the little cells will die.

Entirely different in its action is a cell of a catarrh which has its origin in the disease we call gonorrhea, the so-called syphilitic or gonorrheic catarrh. In the first stage of life we cannot microscopically discover a marked difference between the common catarrh cell and that of a gonorrheic origin. Their outer form is the same. Both kinds of cells are round, globular bodies, which are fastened between the rims and the walls of the underlying epithelial cells by two minute little hook-like appendages, which adhere so firmly in their places that they usually remain in their fastening places, and tear from the cell body if we try to remove these cells for the purpose of examination and propagation. When we look at these transparent bodies uninterruptedly (that is, during the life of the subject upon which they grow and propagate), we shall soon detect that the nucleus of these latter cells is suspended and surrounded by the blastema, and that just before bursting, double nucleoli can be seen, usually three to five appearing in one nucleus. As soon as the outer integument is rent, we notice that the blastema is forming a protecting cover over the nucleus, which resists the influence of the ptyalin of the saliva, which latter ferment does not produce any change in such kind of blastema. If we make our examinations with a powerful microscope, and during the lifetime of the animal, where a constant and fresh supply of lymph and blood is circulating for the support of the epithelial cells, we shall soon discover that the double nucleoli begin to lengthen at one end and to penetrate through the nucleus and blastema. Being fed on the liquid remains of the mother cell, it simply follows that the lengthened nucleoli are of the same nature.

If we can keep the animal that we use for purposes of experiment alive for a few hours, and we follow the growth of this medusa (for I am inclined to call it by this name), we shall soon see that these filaments are lengthening rapidly with a jerking motion. In the course of about three hours, we shall see that these filaments stop growing, and that they grow larger around, and soon we may notice that minute light gray dots appear inside of the now transparent integument, which continue to grow larger and larger until the integument bursts. With my micrometer I have calculated that there are sometimes from 200 to 300 little dots in one integument. After the integument has fallen to pieces, the little microbes will pierce the little hooklets in the wall of the epithelial cells and begin life on their own responsibility. Having been originated and fed on a poisonous mother germ, we can easily comprehend why such a catarrh will never stop; and having transformed the mucus into a thin corroding fluid that is unable to protect the underlying epithelial cells, the parasites will then attack the epithelium, and will destroy the same. So much, then, about these catarrh cells. We shall return to them later.

I only wish to observe that each of these microbes will soon follow the same process of propagation, and we must not wonder that, in a couple of days, millions of these filaments are crossing and recrossing the whole mucous surface of the larynx, fauces, and nares. But these are not the only parts that will be infected. They will reach down into the pharynx and esophagus; but it seems that, below the epiglottis, they do not prosper, as these filaments do not grow much and their propagation is much retarded and disturbed. Their *nidus* ceases below the epiglottis. The reader may wonder why I have been so very particular in describing the cause and progress of this latter catarrh. I shall simply state here that diphtheria and this gonorrheic catarrh are closely connected, that diphtheria is *only* the *second state* of this catarrh, as I *am well able to prove* through my experiments on live rats. I know that the medical profession will attack me on this simple statement, but I am prepared to defend what I am asserting and writing. I have not referred to any books, and I openly confess that I have studiously avoided reading any publication on diphtheria, as I do not wish to argue with books and references. It is only the facts that the eye can see that I can accept, and not theories. I am well able to show and prove that which I have seen.

Let me now relate how I came to the idea to attribute the cause of diphtheria to a gonorrheic origin.

The first case of the disease that I saw here, in America, originated from a young man, who came to my office suffering with gonorrhea. There were no cases

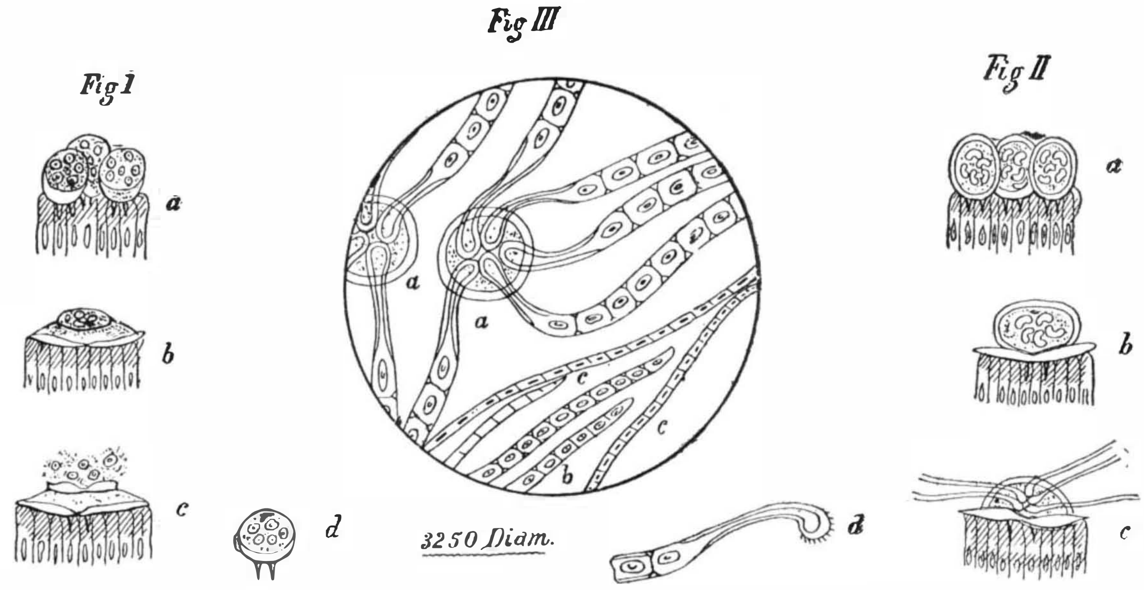
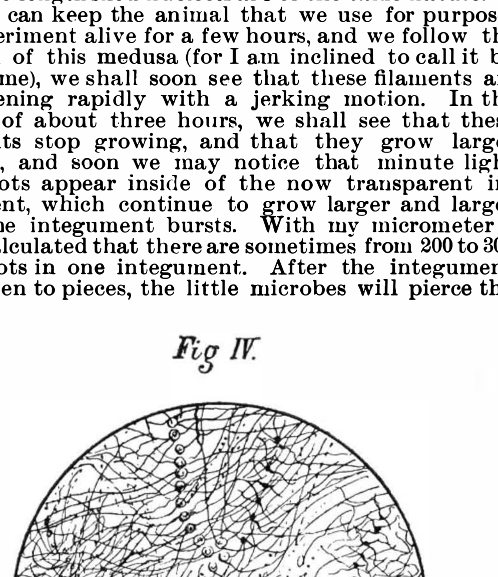


FIG. 1.—COMMON CATARRH CELLS.—*a*, ripe cells. *b*, ripe cell after the integument is rent, nucleus with nucleoli sitting on top of the blastema. *c*, ripe cell with nucleus bursted. The nucleoli are scattered in the neighborhood. *d*, ripe cell showing the harpoon-like antennæ by which it is fastened in the rim of the wall of the epithelial cells.

FIG. 2.—GONORRHEIC CATARRH CELLS.—*a*, ripe cells. *b*, ripe cells with the integument rent, showing the nucleus surrounded and protected by the blastema. Inside of the nucleus are the double (*identical*) nucleoli visible. *c*, ripe cell, with *nucleoli* penetrating the nucleus and blastema. The center of the *sporoides* is protected by the surrounding nucleus and blastema. *d*, the protected inner end or root of the parasite, showing the forming micrococci or microbes. This root is never exposed until the process of growing and propagation ceases and when the integument falls to pieces.

FIG. 3.—*a*, parasite with parts of five nearly full-grown arms, full of nearly ripe microbes and micrococci, the roots still protected by nucleus and blastema of the mother cell. *b*, end part of an arm of a parasite. *c*, arms in the state of growing. Both *b* and *c* represent the parasite in an immature condition.

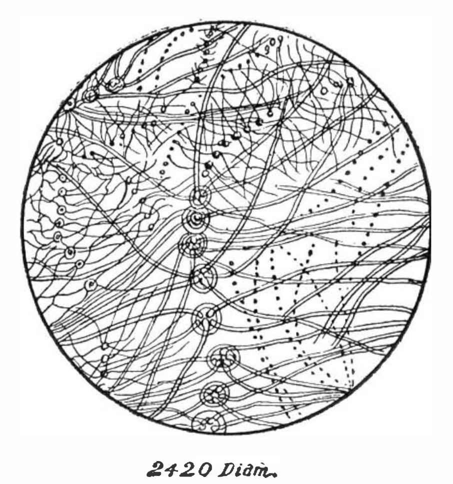
Fig IV.



1600 Diam

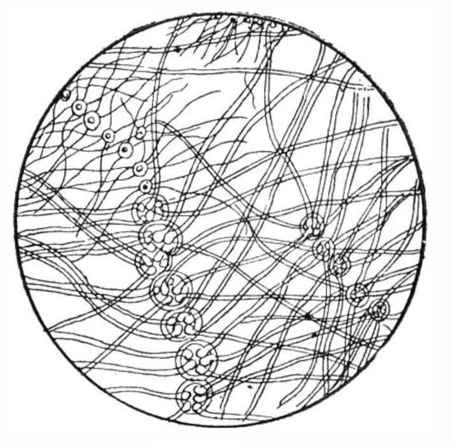
Piece of an epithelium covered with gonorrheic catarrh cells in the state of growing and propagation.

Fig V



2420 Diam.

Fig VI



2420 Diam.

The same as Fig. IV.

of diphtheria in our neighborhood. But after I had had him ten days under my treatment, his four smaller brothers and two sisters, also his mother, were infected through him with a bad catarrh. The mother and one little boy (the latter sleeping with the sick young man) became blind through a blennorrhea, and the whole house came down with diphtheria, after having had this catarrhous affection from eight to twelve days after first infection; but not alone that, the young man visited five other families, which he also infected with the bad catarrh, and which was followed, after an incubation of eight days, with diphtheria. But this is no proof, and I may openly confess that this was only a thought or theory—that is to suppose a connection between diseases apparently so different.

So following the usual book treatment recommended for diphtheria, I had the same success that the other physicians had, namely, that most of the patients died.

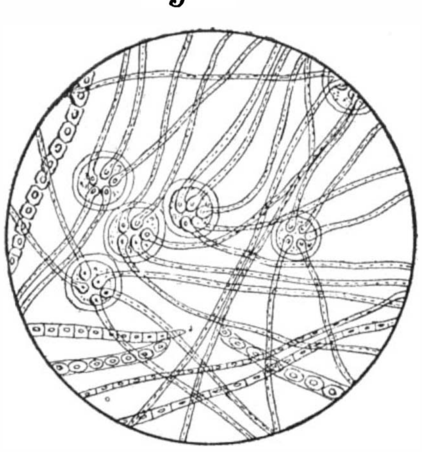
Not being satisfied with the result of my treatment, I inquired and tried to find the cause through *post-mortem* examination, but with *no result.* Thus I came back to the idea of finding the cause of this constant catarrhous affection preceding every attack of diphtheria, which I considered a most essential step toward solving the problem. Here, in Wisconsin, we had formerly two regular epidemics every year—one in the spring, when our logging camps discharged their men and they returned to their homes.

Sometimes the diphtheria croup broke out in the camp, and they had to be closed on account of the scourge before the cutting of the logs was finished.

The second epidemic appeared in the fall, after the strolling harvest hands had left our vicinity. But this was more of a sporadic character, and has ceased entirely since self-binders have come into use. I have kept strict watch of these sporadic cases, and these latter ones are those which have helped me to get the first hint as to the cause. And further, I undertook a journey to the logging camps near Neillsville, and examined the inmates of each carefully, especially those where I found men suffering with a suspicious catarrh. In seven camps I found some cases, and could in each place trace without difficulty the origin to one or two men, who had come into the camp with a gonorrhea. I predicted an outbreak of diphtheria in every one of these seven camps, and had the satisfaction of *seeing five of them closed within three weeks on account of* diphtheria. From the inmates of the other two camps I received intelligence that shortly after they reached their homes, after being dismissed, their families came down with diphtheria. The men had slept in the rooms with the persons infected with gonorrhea and a severe catarrh, and had breathed the same atmosphere, used the same towels, etc., and carried the germs of the deadly foe, diphtheria, to their innocent families. I know that this idea will probably be attacked, but I can prove what I am writing. I have for years never lost a case of diphtheria if I was called before the croup had set in, and I can give the names of prominent physicians who will vouch for the truth of this statement. I shall relate one more case to support my position among the many cases that I could recount, and which would be pertinent illustrations. I was called seventeen miles from my residence to a family sick with the scourge. A few days later I was called to some families living seven miles farther, but in an entirely different direction from the first family, and I was called in on my way back to two more families living in two different valleys.

They informed me that they had neither visited nor seen each other for months, most of them being total strangers to each other; but that a young man, whose parents lived in the vicinity, had called on them (he being a great horse trader). I followed up his track, and found that he had been in every place a week or ten days before the diphtheria broke out. I had the satisfaction to see Mr. M. coming a few days later into my office, suffering with a severe gonorrhea. Upon examination of his throat, I found it in a state of high catarrhous inflammation; he told me upon inquiry that about five days after the gonorrhea had set in, he began to have trouble with his throat, and that he had

Fig VII



3250 Diam.

The same parasites magnified 3,250 diameters.

been infected with his disease in Muscatine, Iowa. As I was called too late to one of the latter families, they lost two children of diphtheria croup, which had set in before I was called.

Of the other seventeen patients I did not lose a single case. If we look over the reports of the armies in Europe, we may easily learn that, whenever a soldier is found out to be suffering with gonorrhea, he is not allowed in a ward where there are wounded men, as, in spite of all precautions, these wounds will not heal on account of the so-called false membranes forming.

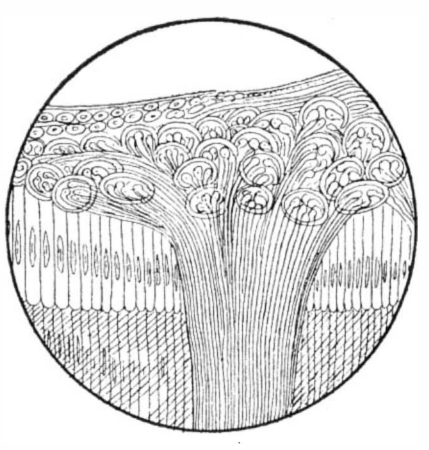
Diphtheria always follows an army, as the reports of the Franco-Prussian war show, and some of the most promising young men lost their lives in consequence of the infection.

But let me now direct your attention to the proofs which I am going to give you. I mean *my experiments* on *live rats* for the purpose of studying the nature of the disease germ. I am a great admirer of Louis Pasteur, and have adopted his method of investigation.

It is said that it is difficult to inoculate an animal with disease germs, and that diphtheria germs have never been successfully inoculated.

This is true; for every disease germ has its own *nidus* in which it propagates, and it would be a nonsensical task to inoculate diphtheria, even if we could get microbes without lacerating them; for whenever we try to remove a diphtheria germ, that is, a cell originating from a gonorrheic catarrh, we shall readily observe that the two hook-like antennæ with which it is fastened in the rim and walls of the epithelial cells will remain in their place, and the outer integument will become lacerated. Such a cell will *never grow*, and further, if we should try to remove an entire medusa,

Fig VIII



3250 Diam.

Gap in the epithelium filled with parasites forcing their arms below the epithelium.

and if we should lacerate any part of the filament, it will at the same moment lose its liquid contents, and the integument will disappear from our sight.

I have never been able to get either a *cell* or a *medusa* entire so as to mount it. *I have never seen diphtheria inoculated in the common way able to propagate.* We have to consider from the beginning that the cause of diphtheria has to be studied during life, for, as I stated, the germs will fall to pieces as soon as the bodily warmth and the supply of blood and lymph ceases.

I began, therefore, to feed thirteen rats with soft bread and butter seasoned with the sputum and nasal exudation of a child dying with the scourge. These animals were separated and carefully fed upon soft food. I also brushed direct from the dying child's mouth the saliva into the open mouth of some of the rats, depositing it as far back as possible. As I had already kept the animals for weeks in a very cool place and got their bodily warmth to about 99° or 100° Fahr., I concluded that the disease germs would find a congenial soil for propagation. I will state that I infected no animal which showed a higher or a lower degree of temperature, and I watched carefully that the warmth of the infected animals remained at 99° or 100° Fahr. In the course of four days the rats seemed to lose their appetite, and would eat only sparingly of the food. After the lapse of three more days some of them showed symptoms of a severe catarrhous affection, and I noticed that the rats were running around with regular snot noses. Then I concluded to begin my investigations. I selected one specimen that I considered well advanced. I put the rat under the influence of chloroform, and with a sharp knife I carefully opened the esophagus and pharynx as far as the epiglottis. This operation I had tried on dozens of rats previously, so as to become well accustomed to the operation. Having fastened the separated parts with small, sharp-pointed tenacula to the outside of the clamp with which I held the rat securely and firmly in its place, I brought my microscope, a large Nachez instrument, into position and directed a reflector to the cut, so as to illuminate the exposed parts. First, I directed my instrument to a place just below the epiglottis, and I saw a fine film, composed of thousands of fine silky threads of light gray color, most of them in a jerking motion, crossing and recrossing each other in every direction, some of them lying apparently motionless. My attention was directed especially toward these motionless threads, which by closer inspection appeared to become dotted lengthways with minute dots. After a while I noticed that the thread had swollen, and suddenly the integument had bursted, leaving only the little gray dots in their former place.

But just at the most interesting point, my rat expired, and three minutes later the jerking motion of the filaments had ceased, and they began to disappear, and a few minutes later there was nothing more left but a gum-like substance (probably protoplasm) in which the shining little dots were visible now and then. Opening the mouth by cutting through the masseter, and laying the pharynx open, I could not see a *trace of diphtheria*, that is, of the *identical white* superficial false membranes, and I considered myself upon the wrong track. But in order to be sure of that, I took the second rat, chloroformed it and fastened it securely. Then I took more pains in preventing loss of blood, and made my incision of the esophagus not so far below as in the case of the first rat, and then I made a cross cut below the epiglottis, so as to get a good view. This time I did not sever any of the larger branches of the jugularis and carotid, and the animal breathed slowly, and whenever it showed symptoms of reviving, I gave it a little more of the anæsthetic, so as to keep it under the *first* influence.

My microscope was now directed to a place back of the epiglottis, but I saw nothing but filaments and dots. I took, therefore, a glass of 3,250 diameters and directed it at the dots, which became visible as round, globular cells, securely fastened to the underlying epithelial cells by two little harpoon-like rootlets, which had always fastened between the walls of two epithelial cells. Keeping the instrument directed on them, I saw that these parasitical cells began to grow, and after a while I could discern a nucleus suspended in and surrounded by a blastema. In a short time I had the satisfaction to see these globes ripening, as there appeared inside the nucleus from four to five *double* nucleoli, that is, nucleoli with *two* ends.

I did not have to wait long, when one of these globes burst, and I perceived that the blastema protected the nucleus, and at the same time the double nucleoli lengthened at one end and perforated the nucleus, but their other ends I could see joining into a kind of a knob, resembling a rose.

The parasite now began to come forth from under the blastema, constantly spreading its arms with a jerking motion, the whole looking like a medusa. Having watched these little arms continually growing, I could plainly see that all the filaments that I perceived in the first rat were nothing but the arms of the same kind of parasites. But in moving my instrument all over the exposed pharynx, I could not see *any white* superficial false membranes, which I was most anxious to find. *But no trace of them was to be seen.*

Disgusted, I was about ready to kill the animal, when, by accident, I looked sideways where the epithelial cells had been separated at the time I opened the esophagus, and there I perceived a white rim at the cut. As I had no idea as to the origin of this white seam, I directed my instrument on the same.

Then I could plainly see that the filaments of the parasite had grown over the cut, and were already reaching *below* the epithelial cells, and were spreading with great rapidity. The death of the rat put a stop to my further investigation, but one thought struck me. Is it possible that the filaments will gradually weaken the epithelial layers through their constant growth, so as to force a gap in the close-filling cells, from which they will penetrate and spread under the cells, thus absorbing and appropriating their nutritious supply for their own use? Is this, therefore, the cause of the white spots? Then I took five rats, chloroformed them, and scraped and scratched with the point of a small knife the fauces and also the roots of their tongues, some deep enough so as to draw blood, and of others I only lacerated the epithelium lightly. After this operation I removed them to their former compartments, where they soon became as lively as ever.

Six hours later I brought one specimen under the influence of chloroform, and opened its pharynx, as before described.

When my instrument was directed upon the lacerated places, I found that where the scratches were only light, there was a greater accumulation of parasitical cells, but no white superficial false membranes, and that where the incisions were deep enough, so as to separate the epithelial cells, the filament had already penetrated through the artificial gap below, and that the parasites were propagating and absorbing the nutritious supply and spreading in every direction under the epithelial cells, which became weaker and weaker, until they twined into an opaque, shriveled substance, *a secure cover for the parasite.*

I opened two more rats, and saw the same phenomenon, for wherever the lacerations were deep enough, so as to destroy the cohesion of the cell walls, the parasites had forced their filaments and the identical white superficial false membranes were formed, as described above. In places where the laceration was only slight, the great accumulation of parasitical cells soon weakened the cohesion of the epithelial cells which soon gave way to the penetrating filaments, and I could plainly see that from these places the plaques (superficial false membranes) spread farther and farther. Being now satisfied as to how the diphtheria plaques (superficial false membranes) formed, I left the two other rats to their fate. One of them died fifty-two hours after laceration, and the other fifty-nine hours after.

When I opened the first one, I found the trachea filled with the white plaques (superficial false membranes). Even the bronchial tubes were filled with them, and the animal had died of asphyxia.

Fig IX.

3250 Diam

Same parasites below the epithelium.

Closer examination revealed the fact that the inside membrane was pressed together through an accumulation of a gum-like substance, through which thousands of little microbes were visible. The other rat had died of the asphyxia; but as the animals had been deeply lacerated, I found the root of the tongue and a part of the larynx a mass of foul-smelling sores.

Having described to you truthfully the result of my experiments on live rats, infected through the exudation and sputum of a child in the last state of diphtheria, and having convinced myself concerning the nature of the identical germ, I came back to the first visible symptom, the catarrh, concerning the origin of which I was by no means satisfied.

I know by the great many cases of gonorrhea which I have treated during my life time, that not every person needs to come down with a gonorrheic catarrh,

and, also, that persons exposed to gonorrhea are not always forced to take the infection.

The reason of that is easily explained, when we consider the nature of a contagion-germ, and by comparing the so called specific fevers, whose origin is only to be found in the invasion and propagation of their identical contagion-germs. Every contagion-germ has its *nidus* in which it may propagate. We do not find any germ of the disease which it represents in any other part of the body except in the *nidus*, where it finds the necessary congenial supply of nutriment for life and propagation.

If a person has been infected but once, it is not likely that he will have a repetition of the same kind of a contagious disease, as the first invasion usually uses up all possible nutrition and exhausts its reproduction com pletely. This is plainly to be seen in small-pox or scarlet fever, which, as a rule, are never repeated after the first invasion ; but if this invasion has been incomplete, then the nutritious supply for the disease germs will be replenished, and there follows a second invasion, however, not severe.

Gonorrhea is usually *stopped through medical interference before* its cells have exhausted their supply of nutrition, and *this* is the cause of its *repetition.*

The knowledge of the causes of the specific fevers led me to these investigations as to the relation between gonorrhea and the diphtheria catarrh. It is a known fact that a gonorrheic person will not always get a nasal or pharyngeal catarrh, even if he should bring the escaping infectious discharges in contact with the tongue or upon the nasal mucosa. The reason for this is to be found in the lack of congenial nutrition.

Such persons are not carriers of diphtheria. But why is it that the contagium-germs of gonorrhea cannot produce diphtheria in another person? Why do diphtheria microbes produce no chancre ?

These are questions that I shall be obliged to answer.

The first question is a difficult one to answer, but experiments upon live rats will easily explain it. I have never been able to produce in them any symptoms of catarrh, if I fed them on fresh gonorrheic discharges. Vivisection never showed any germs or filaments. To ascertain the cause of this, I determined to observe what persons were infected with the identical catarrh at the time of a gonorrhea, and the fact was revealed, namely, *that persons who had already previously had one or more infections were free from catarrh, provided they had had the identical catarrh once at the time of a previous infection.*

Experiments upon rats, which followed this observation, proved that only animals infected with the catarrhous exudations of the throat of gonorrheic persons showed by vivisection the identical double nucleoli and filaments, and that animals fed upon the exudations of gonorrhea (not catarrh) were devoid of the germs, and showed only new mucous formation and *no* filaments—*no trace of the cells with the double nucleoli.*

From this it follows that the germs must first propagate upon the epithelial cells of the throat of a person at the time of a gonorrhea, and must produce the identical catarrh before they can be transferred from person to person, and that persons whose gonorrheic catarrh stopped on account of an exhaustion of the necessary supply of nutrition will *never come down with diphtheria a second time.*

The same is the case with persons who are naturally devoid of the needed supply for the propagation of the germs. Consequently, the disease germ must find the necessaries for life and propagation upon the epithelium, and diphtheria can only form after the cohesion of the epithelium cells has been destroyed through an overtaxation of their supplying power of nutrition.

If the filaments, as we have seen, have penetrated below the epithelial cells, and are spreading and absorbing all nutrition for themselves, the epithelium will die and will form a protection for the parasites which is not easily removed.

This is the second state of the invasion, and consequently we must call the appearance of the white plaques (superficial false membranes—*the only true proof of diphtheria*) the secondary state.

The second question, why diphtheria germs produce *no chancre*, needs no explanation, as the places where we inoculate the disease germs are not their *nidus*, and are devoid of the nutrition to supply their life.

Having stated as plainly as possible what I experienced through a series of experiments, I hope that others who are younger and more skilled than I am will follow up the suggestions that I have given, as we can only by personal investigation trace the subtile causes of diseases.

I have used no book for references, nor can I be satisfied with them, as an answer and objection to my experiments. In the solution of scientific questions, *proofs*, and *not theories*, are required.

It is needless to state that I continued, after having found the cause of diphtheria, my experiments to find the method of destroying it, and my treatment is based upon these latter experiments. The result of these experiments has been eminently satisfactory, as I have never lost a case of diphtheria where I was called in time, and in case the people strictly followed out my directions, and hundreds of people can and will testify to the truth of my statement.

[Journal of Comparative Medicine and Surgery.]

DANIEL E. SALMON, D.V.M.,

CHIEF OF THE BUREAU OF ANIMAL INDUSTRY.

We present the portrait of Dr. D. E. Salmon, the chief of the United States Bureau of Animal Industry, who has been connected with the official veterinary work of this country since Congress first instituted investigations of contagious animal diseases, as provided by the appropriation of 1878.

Dr. Salmon was born in Morris County, New Jersey, July 23, 1850. He entered Cornell University at its opening in 1868, being a member of its first freshman class. Here he became acquainted with Prof. James Law, who had just come to America to fill the chair of veterinary science at this new institution. At that time there were few who realized the importance of veterinary instruction, or who could see any reason for including such studies among the branches taught by our higher institutions of learning.

It was the aim of Ezra Cornell, however, "to found an institution where any person could find instruction in any study;" and with an admirable appreciation of the value of knowledge in regard to the diseases of animals, he determined that veterinary medicine and surgery should be well represented in the faculty of the university which was to bear his name.

Very early in his course at Cornell the subject of our sketch became impressed with the importance of the position and work which were soon to be assumed in this country by educated veterinarians. Even then the flocks and herds of the land had multiplied until their numbers were enormous. And many of our States were without a single veterinarian competent to diagnose the most important animal plagues, much less to give intelligent advice for their control.

Massachusetts had succeeded only two years before in ridding that State, after a six years' struggle, of an outbreak of pleuro-pneumonia which might have been stamped out in as many months. The investigations just made by Prof. Gamgee, under the direction of the United States Department of Agriculture, had shown the existence of contagious pleuro-pneumonia from Long Island, N. Y., on the north to the District of Columbia on the south. The most extensive outbreak of Texas fever which this country has ever known, and which resulted from the wide distribution of Southern cattle over the Northern States in 1868, was then in progress, and was attracting attention everywhere. Hog cholera, although considerably disseminated before the war, was lost sight of for a few years on account of the stirring events of the times, but in 1868 it was again attracting the attention of writers on agricultural and sanitary subjects. These diseases, widely distributed in our own land, and threatening our food supply; with foot and mouth disease and rinderpest ravaging the herds of Europe and liable to be imported at any time, gave unmistakable evidence that our national and State governments would soon appreciate the value of veterinary science.

Dr. Salmon graduated at Cornell in 1875 with the degree of bachelor of veterinary science. The same year he visited Europe, and after remaining two terms at the French veterinary school at Alfort returned to America and began the practice of his profession in Newark, N. J. In 1875, on account of impaired health, he went to Asheville, N. C., for the benefit of the Southern mountain climate. In 1876 Cornell gave him the advanced degree of doctor of veterinary medicine. In 1877 we find him non-resident lecturer on veterinary science in the University of Georgia.

The appropriation by Congress in 1878 of $10,000 for the investigation of animal diseases led to the appointment of a number of veterinarians, including Dr. Salmon, who were directed to devote two months to the study of swine diseases. The reports of these investigations were published by the department of agriculture in a special volume in 1879. It was not expected that this brief investigation would solve all the problems connected with hog cholera, but it was hoped that certain leading questions, such as its nature, cause, and identity in different parts of the country would be settled, and that some practical measures of prevention would be suggested. To this extent the inquiry was successful, and it was demonstrated that there was a specific contagious disease of swine widely disseminated over the country, and that prevention could only be accomplished by controlling the spread of the contagion and destroying it wherever found.

The State of New York attempted to stamp out contagious pleuro-pneumonia in 1879, and Dr. Salmon was appointed on the veterinary staff as an assistant of Prof. Law. The spring and summer were devoted to this work, but with the exhaustion of the appropriation in the autumn, he accepted a commission from the United States Department of Agriculture to investigate animal diseases in the Southern States, and was instructed to make a specialty of Texas or Spanish fever. He then began that extensive series of studies of cattle diseases in the South which demonstrated the identity of the "distemper" of Virginia and North Carolina with the "murrain" of Georgia, Alabama, Mississippi, and Tennessee, and with the Texas fever of the Northern and Western States. During the investigation he directed a careful survey of the whole country, from the Atlantic seaboard in Virginia to the Rio Grande river, accurately outlining the district permanently infected with this contagion, and showing that Northern cattle taken into that district contracted the same disease that was disseminated by Texas and other Southern cattle when taken North.

In 1880 he began the investigation of fowl cholera, demonstrating that the germ found in that disease was its essential cause, that it could be made to multply locally and act as a vaccine by simply diluting the virus to a sufficient degree.

He also at this time formulated a theory of immunity from contagious diseases, based upon numerous experiments, in which were foreshadowed a new method of prevention through the use of chemical substances formed during the growth of the specific germs. This method has since been satisfactorily demonstrated by experiments made under his direction, and he is still at work upon the details which must be learned before it is put into practical operation.

Early in 1883 he was called to Washington by Commissioner Loring to establish a veterinary division in the Department of Agriculture. In doing this a plan was adopted for an extensive investigation of animal diseases, a laboratory for histological and bacteriological research was fitted up, and seven acres of land near the city were rented for an experimental station. Within a year Congress passed an act establishing the Bureau of Animal Industry, and Dr. Salmon was at once appointed chief of this bureau, a position which he still holds.

Since he has been in the Department of Agriculture a large number of investigations have been made under his direction. In 1883 he was appointed by President Arthur a member of a commission to investigate the quality of our pork products and the prevalence of trichiniasis, and his report on this subject has been received without question.

In 1884 he investigated the supposed outbreak of foot and mouth disease in Kansas, Missouri, and Illinois, and allayed apprehensions by deciding it to be ergotism. In the same year he discovered and traced out an outbreak of pleuro-pneumonia which extended to Ohio, Illinois, Kentucky, and Missouri, and succeeded by co-operating with State authorities in securing its extermination wherever found. It has since been discovered, however, that it had crept into Chicago unobserved, where it smouldered for two years and was brought to light by the State veterinarian in September, 1886. The Bureau of Animal Industry now has a force of inspectors engaged there for its suppression.

Since its organization the Bureau of Animal Industry has made an inspection of the district from Long Island, New York, to virginia, to determine the extent and prevalence of pleuro-pneumonia. In August, 1886, an agreement of co-operation for stamping out this disease was made with the State authorities of Maryland, and the slaughter of diseased animals has been going steadily on. In March of this year, an appropriation of five hundred thousand dollars was made to the bureau, and the work extended to the slaughter of exposed as well as of diseased animals.

New and efficient rules and regulations for co-opera-

tion with State authorities were at once prepared and sent to the governors of the various States for their acceptance. And in States where no laws existed authorizing co-operation a suitable bill was drawn and presented to the legislature for its consideration. As a result of such energetic work, more than half of the States have already given the bureau full authority to stamp out the lung plague should it be found within any of their borders, and hardly sixty days have passed since the effort was begun. This work is now in active progress in the States of New York, New Jersey, Maryland, Virginia, and Illinois.

While this executive work has been pushed so successfully, the scientific work of the bureau has not been neglected. In the report of 1885, the first accurate description was given of the germ of hog cholera and of the lesions which it produces in swine and other animals. Since then another swine disease resembling or identical with the *schweineseuche* of Schutz has been studied, and will be fully described in the forthcoming report for 1886. A thorough study of the entozoa affecting the domesticated animals of the United States is also in progress, and is yielding very important results.

In addition to this work, which comes strictly within the province of the Bureau of Animal Industry as defined in the act of Congress establishing it, the quarantine of imported animals, formerly in charge of the Treasury Department, is now under the direction of Dr. Salmon.

As a recognition of Dr. Salmon's labors in behalf of veterinary science, the Royal College of Veterinary Surgeons of Great Britain has recently conferred upon him the degree of honorary associate—the highest professional honor which that body can bestow. He is also a fellow of the American Association for the Advancement of Science, and chairman of the Committee on Animal Diseases and Animal Foods of the American Public Health Association. W. J. C.

THE WORK OF THE INTERNATIONAL CONGRESS OF GEOLOGISTS.*

By G. K. GILBERT.

THE presence of a number of European geologists at the Buffalo meeting of the A. A. A. S. in 1876 naturally suggested the formation of an association of the geologists of the world. This suggestion was the foundation of the International Congress of Geologists. This body met at Paris in 1878, at Bologna in 1881, at Berlin in 1885, and will meet in London next year.

The work of the congress lies in geologic nomenclature and classification, and the conventions of geologic maps. The particular classifications attempted are the establishment of the major divisions used in historic and in stratigraphic geology, and the subdivision of volcanic rocks. In nomenclature three things are undertaken : first, the determination of the names of historic and of stratigraphic divisions; second, the formulation of rules for nomenclature in paleontology and mineralogy; third, the establishment and definition of the taxonomic terms of chronology (period, epoch, etc.) and of stratigraphy (system, series, etc.). The map conventions most discussed are colors. The congress is also preparing a large map of Europe, to be issued in forty-nine sheets.

Briefly stated, the work accomplished to the present time is as follows : Agreement has been reached as to the rank and equivalence of taxonomic terms employed in chronology and stratigraphy ; a set of rules for paleontologic nomenclature has been adopted ; and many sheets of the map of Europe have been prepared for the engraver. A partial classification of stratified rocks and a partial scheme of map colors has been agreed upon.

In the terminology of zoology and botany the words, kingdom, class, etc., though difficult of definition, are always used in the same order of inclusion ; but in geology there is no such uniformity of usage. The terms and order adopted by the congress are as follows : Of stratigraphic divisions the highest is *group*, then *system*, *series*, and *stage;* the corresponding chronologic divisions are *era*, *period*, *epoch*, and *age*. The strangeness of this order of rank and the use of *stage* will not seriously retard the adoption of the convention in view of the utility of uniformity and perspicuity. The introduction of the word *stage*, or at least of some new word for that part of the column, was necessitated by the restriction of the word *formation* to a special meaning—the designation of mineral masses with reference to their origin. This restriction also keeps us from using *formation* to denote indefinitely an aggregation of strata. I would suggest the word *terrane* for this meaning of *formation*. The fixation of the chronologic terms creates a similar difficulty; and, on the whole, *time* seems open to the least objection for use in the indefinite sense.

There are propositions before the congress to distinguish the names of individual groups, systems, series, and stages by means of terminations, those of the same rank having the same ending, *e. g.*, every name of a group should end in "ary," as "Primary." The adoption of such a plan would enable a writer to indicate the taxonomic rank of a terrane without adding a word for that purpose. Conversely, the reader or hearer would always learn the taxonomic rank, or supposed rank, whenever a terrane was mentioned. These are advantages, but several disadvantages would also arise from the use of such terminations. One could not discuss terranes from any point of view without expressing an opinion as to their taxonomy. Again, geologists who differed as to the rank of a terrane would terminate its title differently, and a needless synonymy would thus be introduced. In the third place, the created necessity for taxonomic discrimination on all occasions would tend to direct undue attention to taxonomic problems.

The congress also adopted rules for the establishment of the names of genera and species in paleontology.

It is surprising that a body of geologists assumed to speak with authority on this subject. Paleontologists should unite with biologists in the adoption of rules of nomenclature. No action in regard to the nomenclature of mineralogy has yet been taken.

Another projected work of the congress is the classification of eruptive rocks. As there is no agreement as to the fundamental principles on which their classification should be based, owing to the lack of an accepted theory of volcanism, it is to be hoped that the congress will adopt no scheme of classification.

The two most important undertakings of the congress are the classification of terranes and the unification of map colors. The congress is attacking these by means of a third undertaking, the preparation of a geologic map of Europe, and this method of approach has had the effect of making it difficult properly to interpret its action. The original idea was the adoption of a stratigraphic classification to be applied to the whole earth, and of a color scheme for use in all geologic maps. But at the Berlin session the committee in charge of work on the map of Europe pressed the congress for the determination of questions on which hung the completion of the map, and many hasty decisions were reached, while not a few disputed points were referred to the map committee. In view of the uncertainty thus occasioned, I shall not attempt to characterize the attitude of the congress on the subject of classification, but shall merely develop my individual view.

The problem demands a true conception of a *system*, instead of the false conception that is abroad. We begin with the elements. The land surface is removed by erosion and spread on the ocean floor as a deposit, but the relations of land and ocean have varied irregularly, until every part of the surface has been changed. Simultaneous deposits are not everywhere the same, the variations depending largely upon the depth of water and the distance from the shore. Animals and plants are grouped in provinces with shifting boundaries. From the earliest to the present time, species have been progressively modified according to local conditions. There are two antagonistic tendencies, one toward diversity of life and the other toward its unification. Taking some limited area, we find its nature complex as the result of these processes. There are breaks at different horizons, lithologic and life changes, both being, at times, abrupt, and it is thus that the stratigraphic column is classified into groups, systems, series, and stages, according as the breaks are great or small. As the criteria differ only in degree, precise definitions are impossible, and classification is largely a matter of convenience. It should be remembered that the breaks are natural, not artificial. In some other area distant from the first, with different fauna and flora, the same principles apply, but the systems do not coincide. Hence, while we can compare systems, we may not be able to compare their details. This ideal case represents the common experience of those who have tried to correlate remote districts with the geologic history of Europe. There does not exist a worldwide system, nor group, but every system and every group is local. If the term system be made universal, it must be artificial, or if natural in one geologic province, it must be artificial in all others. It is highly convenient to use time divisions, but the adoption of the European time scale was due to the accident that Europe was explored first. The scale being recognized as arbitrary, it may be modified by the congress, thus putting it in the best possible shape.

Confusion of ideas should not be fostered for the sake of artificial uniformity. I suggest for the ending of time words the syllable "*al*." Thus while Jurassic and Devonian would be purely European, Jural and Devonal would denote divisions of the standard time scale.

The adoption of a good color scheme is more important than even the taxonomic nomenclature ; because at present scarcely two individuals use colors in the same way. Every new geologic atlas has a new alphabet of colors. A universal language should be substituted for this confusion of tongues. Hence the provisional work of the congress should be freely criticised at its present stage, as the adoption of an ill-arranged color scheme would occasion continual loss.

A perfect map must be clearly legible, readily adjustable to facts, applicable to both large and small maps, inexpensive, easily remembered, pleasing to the eye, and should occasion the least inconvenience in changing from the present systems. The number of hues and tones that can be combined in a map is small, not exceeding perhaps twenty ; but textures admit of great variation, and as many as a hundred may be used without confusion. In the color scheme prepared for the map of Europe thirty-eight distinctions of hues and tones are made, adjusted to rock systems of Europe exclusively, and not applicable to other parts of the earth. The scheme cannot be applied to America, India, New Zealand, or Australia without misrepresentation. It would be better to adopt no convention at all as regards map colors than a false one. Flexibility can and ought to be combined with other desirable qualities. I suggest that the continuous prismatic spectrum be adopted as the standard universal scale for continuous geologic time ; and that the conventional time scale based on the geologic history of Europe be complemented by a color scale, prismatic but discontinuous. I propose that the students of each geologic district assign for its systems a set of selected prismatic colors ; and that provisional colors for undetermined systems be distinguished by a special device. If the number of subdivisions is small, they may be represented by tones of the hue assigned to the system, and if great, by monochromatic textures. It is provided that hues shall have no other function ; this secures the integrity of the distinction between systems, however minute the subdivision. In my judgment, the maximum number of hues that can be used is from fifteen to twenty. I would assign the browns to volcanic rocks and leave the grays unassigned. These are the main features of the proposed prismatic scheme, which is constructed for the express purpose of securing a degree of flexibility that will fit it for universal use. Such an alphabet of colors will be orderly, familiar, and easily learned, while it scientifically differentiates the functions of hues and tones.

Finally, when the matter is proposed for regulation by the congress, the first question to be asked is whether it falls within the legitimate purview of a convention of geologists, or belongs to some other science. It should also be understood that geologic *facts* are not subject to settlement by convention. Facts can be established only by observation. The science of geology consists in the aggregation and arrangement of facts ; and a classification should merely be a generalized expression of those facts. Hence I regard it as ill-advised that the geological congress undertook to classify the sedimentary formations and the volcanic, and that it also undertook to prepare a map of Europe, further than as a work of compilation.

The other undertakings of the congress as to taxonomic terms, systematization of terminations, the selection of a scale of colors, and other conventional signs, all belong to the means of intercommunication of geologists, and affect only the verbal and graphic technology of the science. The arbitrary time scale is a conventional terminology for the facts of correlation. We may therefore say in general that the proper function of the congress is the establishment of common means of expressing the facts of geology. It should not meddle with the facts themselves. It may regulate the art of the geologist, but it must not attempt to regulate his science. Its proper field of work lies in the determination of questions of technology ; it is a trespasser if it undertakes the determination of questions of science. It may decree terms, but it must not decree opinions.

* Abstract, vice-presidential address, read to section E of the American Association for the Advancement of Science, New York, Aug. 10, 1887.

WHAT AMERICAN ZOOLOGISTS HAVE DONE FOR EVOLUTION.*

By EDWARD S. MORSE.

ELEVEN years ago I had the honor of reading before this association an address in which an attempt was made to show what American zoologists had done for evolution. My reasons for selecting this subject were, first, that no general review of this nature had been made ; and second, that many of the oft-repeated examples in support of the derivative theory were from European sources, and did not carry the weight of equally important facts the records of which were concealed in our own scientific journals. Darwin was pleased to write to me that most of the facts I had mentioned were familiar to him, but, to use his own words, he was amazed at their number and importance when brought together in this manner. The encouragement of his recognition has led me to select a continuation of this scheme as a subject for the customary presidential address, a task which is at best a thankless if not a profitless one. Had I faintly realized, however, the increasing number and importance of the contributions made by our students on this subject, I should certainly have chosen a different theme.

Incomplete as is this record of ten years' work, I am compelled to present it. In the Buffalo address two marked periods in the work of the zoologists in this country are recognized ; the one period embracing the work of the topographers, the field surveyors in the science ; the other period dating from the advent of Agassiz, with the wonderful impulse he imparted to the study by his enthusiasm and devotion. A third period in American zoological science, and by far the most important awakening, dates from the publication of Darwin's "Origin of Species." Its effect on zoological literature was striking. The papers were first tinged with the new doctrine, then saturated, and now, without reference to the theory, derivation is taken for granted.

As zoologists we are indebted to Darwin for the widespread public interest in our work. Before Darwin the importance of our special studies was far outweighed by the practical value placed upon science in the application of which an immediate material gain was assured. Chemistry, physics, geology, were important to the public only because a practical application of these sciences was capable of showing an immediate material return.

Agassiz, in his appeal to the State for appropriations for the great museum at Cambridge, insisted that there were higher dividends than money ones to be looked for in endowments for zoological museums, and these were intellectual dividends. While the force of this appeal will always remain true, the transcendent importance of the naturalist's studies from the standpoint of Darwin is widely recognized. Man now becomes an object of rigid scientific scrutiny from the new position which has shed such a flood of light upon the animals below him. His habits, behavior, the physical influences of his environment and their effects upon him, transmission of peculiarities through the laws of heredity—all these factors are directly implicated in the burning questions and problems which agitate him to-day. Questions of labor, temperance, prison reform, distribution of charities, religious agitations, are questions immediately concerning the mammal man, and are now to be seriously studied from the solid standpoint of observation and experiment, and not from the emotional and often incongruous attitude of the church. To a naturalist it may seem well nigh profitless to discuss the question of evolution since the battle has been won, and if there be any discussion, it is as to the relative merits and force of the various factors envolved. The public, however, are greatly interested in the matter, as may be seen by a renewal of the fight in the English reviews, and the agitation is still kept up by well meaning though ignorant advisers, who insist that science has not yet accepted the doctrine ; and great church organizations meet to condemn and expel their teachers of science from certain schools of learning because their teachings are imbued with the heresy.

Dr. Asa Gray, in his discriminating biographical memoir of Darwin, says in regard to the "Doctrine of Descent," "It is an advance from which it is evidently impossible to recede. As has been said of the theory of the Conservation of Energy, so of this. The proof of this great generalization, like that of all other generalizations, lies mainly in the fact that the evidence in its favor is continually augmenting, while that against it is continually diminishing, as the progress of science reveals to us more and more the working of the universe." Let us examine then the evidences, trivial as well as important, that have been recorded by American zoologists within the past ten years in support of the derivative theory.

Without further apology for the very imperfect character of this survey, let me at once begin by calling attention first to the testimony regarding the variation in habits and evidences of reasoning power in animals. The establishment of individual variation in mental powers, change in habits, etc., lies at the foundation of Darwinism as furnishing material for selective action. There is no group of animals which exceeds the birds in varied and suggestive material for the evolutionist. It is a significant fact that the birds, which appeared

* Address of the retiring President of the American Association for the Advancement of Science, New York, August 11, 1887.

to Cuvier and his contemporaries a closed type, a group that seemed to fulfill the ideal conception of a class archetype as compared to other groups which had their open as well as obscure relationships, should be of all groups the one that first yielded its exclusive characteristics. In fact, there is no group in which the barriers have been so completely demolished as in this apparently distinct and isolated class.

An attentive and patient study of the birds has established almost every point defined by Darwin in his theory of natural selection. One has only to recall the marked reptilian affinities as shown in their embryological and paleontological history. Besides all these structural relationships, the birds possess as a group remarkable and striking illustrations of variation in color, size, marking, nesting, albinism, melanism, moulting, migration, song, geographical variation, sexual selection, secondary sexual characters, protective coloring; and in their habits show surprising mechanical cunning and ingenuity, curious and inexplicable freaks, parental affection, hybridity—indeed, the student need go no further than the birds to establish every principle of the derivative theory.

The many observations on the nesting habits of birds would form a curious chapter as illustrating the individual peculiarities of these creatures.

Dr. A. S. Packard records the fact, as related to him by Mr. Wyatt, of wild geese nesting in large cottonwood trees on Snake river, west of the Rocky mountains; and Doctor Coues, in his "Birds of the Northwest," says wild geese "nest in various parts of the Upper Missouri and Yellowstone regions in trees." Mr. H. W. Turner observes a robin nesting on the ground. The late Dr. T. M. Brewer points out some very curious "Variations in the Nests of the Same Species of Birds." He not only observes individual variation in nest structure, but shows that in different regions of the country birds of the same species build different kinds of nests, and in reflecting on these peculiarities he is led to say: "If we cannot understand what it can be that stimulates an *Empidonax* in Staten Island to build a pensile nest, while its fellow in Indiana builds one like a deep cup and surrounded with thorns, and another group in Pennsylvania puts theirs on an exposed tree top, and so flat that the eggs seem liable to roll out, we must see that some cause, hidden to us, is gradually effecting changes that sooner or later may become universal in the species, though which it is to be we may not be able to imagine."

Mr. J. A. Allen, in writing on the inadequate theory of birds' nests, shows grave and important exceptions to Wallace's theory, though he subscribes heartily to his philosophy of birds' nests. He expresses surprise that closely allied species of birds should oftentimes build divers kinds of nests, overlooking the fact that even closely allied varieties of man build entirely unlike houses.

Mr. F. H. Knowlton records a cliff swallow appropriating, for the construction of its own nest, pellets of mud which were being brought by another swallow. Also the curious fact that a number of swallows were observed busily engaged in sealing up a nest in which one of their comrades lay dead. Among the curious traits of birds Mr. H. B. Bailey communicates some new ones observed in the red-headed woodpecker by Mr. Agersborg of Dakota territory. This gentleman had observed one of these birds wedging grasshoppers in a large crack of an old oak post. Nearly a hundred were stored away in this manner, the bird afterward feeding at leisure on the supply. This parallels the habit of the California woodpecker storing acorns in holes in the tree and subsequently feeding on the fully developed larvæ within the seed.

Mr. O. P. Hay, in a late number of the *Auk*, has an interesting paper on the red-headed woodpecker as a hoarder, showing that the bird makes accumulations of beech nuts, pounding them between the shingles of a roof, wedging them into crevices, and storing them in cavities in trees.

The plausible suggestion made by Darwin as to the agency of aquatic birds in the wide dispersal of fresh water mollusks was singularly confirmed several years after by Mr. Arthur F. Gray shooting a duck which had clinging to one of its toes a fresh water mussel. Dr. J. W. Fewkes has recently recorded the shooting of a duck in Sebec, Maine, which was in like manner transporting a fresh water mussel. The same bird had been observed several days before with this curious companion clinging to its foot, and had the duck been migrating at the time it might have transported the mussel many hundreds of miles. In this connection it would be an interesting inquiry as to how far the similarity observed in north temperate and circumpolar animals is due to the annual migration of birds north and south.

Mr. William Brewster notes some interesting features in the habits of a young Kittiwake gull of the St. Lawrence. He brought home a young one, its mate having died of thirst, the other one surviving through the accidental discovery that the bird drank only salt water. Both the birds obstinately refused to drink fresh water. Observations on this bird by Prof. A. Hyatt showed how slowly and timidly it acquired the art of swimming and flying. The bird when first forced to fly was thrown into the air, and to the surprise of Professor Hyatt flew with great rapidity and precision, circling about the house and through the apple trees, and, finally, flew near him several times in the greatest agitation till he caught the bird, which was completely exhausted. For a long time the bird went through this maneuver, showing that while he knew how to fly it could not alight, though it finally acquired this faculty. Prof. L. A. Lee records a remarkable attack made on him by a marsh hawk, and Mr. Abbott M. Frazer tells of a tame crow deliberately standing on an ant hill and permitting the ants to remove the parasites from its feathers. In this connection a paper by Mr. Joseph F. James should be read in which he shows by a number of arguments that animals not only present a reasoning faculty, but that this faculty has been the result of slow evolution.

Mr. Xenos Clark, in an exceedingly interesting article on the music of animals, and particularly the music of birds, concludes by saying there is "a theory for the origin of melody, whether human or extra-human, which, besides the usual basis of physiological acoustics, employs the law of modified, inherited, selected, and adapted structure, *i. e.*, the law of evolution."

Mr. Ruthven Deane records cases of albinism and melanism in a great many families of birds, and Mr. N. C. Brown shows the variable abundance of birds at the same locality in different years. In this connection it will be of interest to read Dr. L. P. Gratacap's paper entitled "Zoic Maxima; or, Periods of Numerical Variation in Animals."

The behavior of wild birds when kept in confinement, and the attempts made in domesticating them, has always furnished an interesting field for study. The curious freaks and impulses which they often betray, the changes they show under the new conditions, indicate in some measure the plasticity of their organization.

Hon. John D. Caton, in an interesting paper on "Unnatural Attachments among Animals," records a curious fondness shown by a crane for a number of pigs; and in another paper, on the "Wild Turkey and its Domestication," this writer has made some valuable records of the successive changes which take place in the bird during this process—changes in color, during which the more conspicuous features of protective coloring are lost; changes in habit, in which are seen the undoing or relaxing of those features which indicate constant vigilance, from carrying itself in a semi-erect attitude, perching on the tallest trees, covering up the eggs carefully with leaves when off the nest, etc., to moving in a horizontal attitude, perching near the ground, covering the eggs but slightly or carelessly, etc., and losing that wildness which characterizes the bird in its wild state. At the breeding season, however, the females became wild again, but this was a feature too deeply implanted to show modification in the time allotted to Mr. Caton's experiment. The same writer has also observed in the Hawaiian Islands the effects of reversion to a wild state of different kinds of domestic animals which have from time to time been carried there. Among other animals he was fortunate enough to observe the undoing stages in the domestic turkey and the assumption of those features which characterize the wild bird.

A great many facts illustrating the plainest features of natural selection, protective coloring, mimicry, etc., have been recorded in our journals from time to time. A brief allusion may be made to a few of these.

Prof. Samuel F. Clarke notices a pronounced case of natural selection—a case which must often occur in nature. He kept in large glass jars masses of eggs of Amblystoma. As soon as these eggs began to hatch, he found it difficult to provide the young with suitable food, and yet they seemed to thrive. On examination, many of them were seen to be engaged in nibbling the branchia of others, and as they increased in size they were seen to swallow the weaker individuals bodily, and hence grow with increased rapidity. "Here, then," he says, "was a very interesting case of natural selection by survival of the fittest. All the weaker individuals being destroyed and actually aiding the stronger ones by serving them as food until they could pass through their changes and escape to other regions where food was more abundant." Prof. B. G. Wilder has recorded a similar condition of things in a species of spider, where the young spiders within the case inclosing the eggs were feeding on the weaker ones. Prof. Henry L. Osborn observes a curious case of mimicry at Beaufort, in the coloring of a species of Ovulum which frequents a species of Leptogorgia. The Ovulum was yellow in color on the yellow variety of this sea fan, and purple when living on the purple variety. Dr. R. E. C. Stearns has made some interesting notes on protective coloring in Phrynosomæ. Having collected these horned lizards (or toads, as they are commonly called) in Central California, he has noticed that if the ground region they frequent is yellowish, the lizards are without exception of that color; if ashen gray, then that color is simulated, and this without exception. Further than this, he is "led to believe that a sufficient number of living specimens will show a similar protective factor, in degree of development of the scale imbrications, tubercles so called, and horns—or, in brief, in the sculpture aspect as related to the surface texture of the ground which forms the local habitat of these forms." Dr. A. S. Packard has observed the partiality of white butterflies for white flowers. He noticed the European cabbage butterfly, which is white, go directly to the white aster and rarely visit the golden rod, while the yellow sulphur butterfly visits the yellow flowers of the golden rod oftener than those of the aster. The same author also observed a harmless Egerian moth which deceived the sharp eye of a trained entomologist by its resemblance to a wasp, and asks, Why may not a bird be equally deceived? Miss Sarah P. Monks observed a case of mimetic coloring in tadpoles, their tails precisely resembling the leaves of an aquatic plant, Ludovidgia.

Miss Mary E. Murtfeldt having noticed that the butterfly *Pyrameis hunteri* always deposited its eggs on the plant Antennaria, she was surprised to find a number of larvæ of this butterfly on Artemisia. The customary plant being rare in the immediate vicinity, the butterfly had been misled by the surface resemblance of the white cottony leaves of the Artemisia to those of the accustomed food plant. In this case the larvæ all died.

An unquestionable fact has been finally established by recent methods of observation on the habits of insects and other animals, and that is that individuals of the same species vary in intelligence; that they are not automata; that they are not impelled by a blind instinct to perform certain acts with unerring accuracy, but, on the contrary, that they vary and often greatly vary in their ability to provide for their young, in their skill to secure sufficient food, in their wit to avoid danger: in other words, they make blunders and mistakes and involve their progeny, and even their colony, in ruin. This individual variation in intelligence is brought out very clearly by a patient series of observations made by Drs. G. W. and E. G. Peckham on the special senses of wasps. They not only repeated many of the experiments of Sir John Lubbock, but many new and ingenious experiments were devised. Their studies were for the purpose of investigating the mental power, sense of hearing, color, direction, memory, emotion, power of communication, general intelligence, etc. An interesting result of their painstaking work was the determination of individual differences as to the faculty of memory and power of distinguishing color and direction. This kind of study of the habits of insects has brought to light features of the most surprising character. The remarkable studies of Sir John Lubbock, Dr. Moggridge, and others in Europe have been paralleled in this country not only by the observations above quoted, but notably by the labors of Rev. H. C. McCook in his studies of the American ants and spiders. In various papers published in the proceedings of the Philadelphia Academy of Natural Sciences and in the *American Naturalist* he has shown many extraordinary and curious features in the life histories of these animals. The great variety and extent of his work must be my excuse for not referring to it in detail.

Prof. G. F. Atkinson, in studying a new species of trap-door spider, confirms the observations of others as to the creature deliberately attaching fragments of moss to the lid of its nest in order to conceal its position. Dr. Thomas Meehan describes a hornet that was gifted with great intelligence. He saw this insect struggling with a large locust in unsuccessful attempts to fly away with it. After several fruitless efforts to fly up from the ground with his victim, he finally dragged it fully thirty feet to a tree, to the top of which he laboriously ascended, still clinging to his burden, and having attained this elevated position, he flew off in a horizontal direction with the locust. Dr. Meehan truly says: "There was more than instinct in this act; there was reasoning on certain facts and judgment accordingly, and the insect's judgment had proved correct."

A curious case of circumspection in ants is recorded by Dr. Joseph Leidy. In an empty house he observed some ants feeding on crumbs of bread left by the workman. He at once placed pieces of bread in the different rooms in the house, only to find them the next day covered with ants, which he destroyed by causing them to fall into a dish of turpentine. After a few days the ants no longer visited the bread, and he supposed they had been exterminated. A few days after, however, he observed a number of ants in the attic feeding on the body of a dead fly. He immediately got a lot of grasshoppers and distributed their bodies in all the rooms, only to find that they were soon covered with ants, which he destroyed as before. This treat continued attractive for a few days only, when the ants abandoned the food. In brief, he tried meat, cake, and various other articles in turn; the ants for a while frequenting these snares, only to learn the danger involved, and finally avoided them.

The gradual dispersion of species in recent times is of great interest, and careful records should be made of the facts as observed and a collection of large numbers of individuals made, in order to compare them with specimens of the same species in future years, to ascertain the variation which may have taken place and the tendency of that variation. A number of observations have been published within the last ten years showing new areas of distribution. *Litorina litorea*, which has been creeping along the coast since 1869, as recorded by Gray, Verrill, and others, has now reached the southern side of Long Island Sound, as observed by Mr. Henry Prime. *Lioplax sub-carinata*, an Ohio river species, has been found in the Hudson river at Catskill landing. *Limax maximus*, first found at Newport, R. I., by Mr. Powel, has since been found at Cambridge, Mass., by Professor Hyatt. *Bythinia tentaculata*, first recorded from Oswego, N. Y., by Rev. W. M. Beauchamp, is reported as having been found at Burlington, Vt., by G. H. Hudson. In the Mohawk river is a thriving community of this species, the first having been placed there by Dr. James Lewis.

Dr. R. E. C. Stearns, in commenting on the occurrence of *Mya arenaria* in San Francisco bay, states that the first record of the species in California was made by Dr. Newcomb in 1874. Within a few years it has increased in great numbers, furnishing a new food supply for the people. The evidence that it is a recent introduction is seen in the fact that so large and conspicuous a species could not have escaped the eye of the collector. No trace of it has ever been found in the numerous shell heaps of California, though it is found on the Asiatic coast from Kamtchatka to the southernmost limits of Japan. Dr. Stearns believes it to have been imported with the oyster transplanted from the Atlantic coast. From large numbers of the shells that I measured, the low index would show that it came from some southern point on the Atlantic coast.

The delicate balance of conditions between organisms, whether it be between individuals of the same species or between widely separated groups, is an important feature in the question of survival. Prof. S. A. Forbes, in a thoughtful study of certain species of entomostraca in Lake Michigan and the surrounding waters, calls attention to the important part played by these minute crustaceans, showing how they furnish almost the entire food for young fishes, larger crustaceans, and even insect larvæ. He writes: "Mollusca, one would say, could afford to be indifferent to them, since they neither eat them nor are eaten by them, nor seem to come in contact with them anywhere, through any of their habits or necessities. But for this very reason these two classes afford an excellent illustration of the stringent system of reactions by which an assemblage of even the most diverse and seemingly independent organisms is held together. . . . If there were no entomostraca for young fishes to eat, there would be very few fishes indeed to feed upon mollusca, and that class would flourish almost without restraint; while, on the other hand, if there were no mollusca for the support of adult fishes, entomostraca would be relieved from a considerable part of the drain upon their numbers, and would multiply accordingly." He is much struck with the fact that in the larger bodies of water, the species of entomostraca show an inferior development in numbers, size and robustness, and in reproductive power. Their smaller number and size are doubtless due to the relative scarcity of food. "The difference of reproductive energy, as shown by the much smaller egg masses borne by the lacustrine species, depends upon the vastly greater destruction to which the paludinal crustacea are subjected. Many of the latter occupy waters liable to be exhausted by drought, with a consequent enormous waste of entomostracan life. The opportunity for reproduction is here greatly limited—in some situations to early spring alone—and the chances for destruction of the summer eggs in the dry and often dusty soil are so numerous that only the most prolific species can maintain themselves under such conditions.

"Further, the marshes and shallower lakes are the favorite breeding grounds of fishes, which migrate to them in spawning time, if possible, and it is from the entomostraca found here that most young fishes get their earliest food supplies—a danger from which the deep water species are measurably free. Not only is a high reproductive power therefore rendered unneces-

sary among the latter by their freedom from many dangers to which the shallow water species are exposed, but in view of the relatively small amount of food available for them, a high rate of multiplication would be a positive injury, and could result only in wholesale starvation."

The effect of birds on insect life has engaged the attention of the same author. His inquiry was to ascertain whether birds originated any oscillations in the numerical proportion of insects upon which they feed. Many interesting facts are given which space forbids quoting.

A number of contributions have been made on the influence of environment and on geographical variation, to some of which reference must be made. Prof. Alpheus Hyatt bears unequivocal testimony to the derivative theory, and recognizes clearly the influence of external surroundings in a memoir on the cephalopods, when, in stating the law of organic equivalence, he says: "The action of physical changes takes effect upon an irritable organism, which necessarily responds to external stimulants by an internal reaction or effort. This action from within upon the parts of the organism modifies their hereditary forms by the production of new growths or changes which are, therefore, adapted to the conditions of the habitat or the physical agents and forces from which they directly or indirectly originate," or slightly changing this interpretation in accordance with the same facts, each individual is more or less susceptible to the action of physical influences, and those which respond quickest to physical influences come more promptly in harmony with their environment, which is natural selection pure and simple.

Mr. Charles Morris in a series of papers on "Organic Physics" and the "Polar Organization of Animals," presents many new and suggestive thoughts on the physico-chemical action in life and development. He concludes that "there are inherent in the germ energies and tendencies, chemical, molecular, or whatever we choose to call them, adapted to the complete unfoldment of the typical form. But as appears evident, their operation can be checked by influences from external nature. There is a struggle between these contact influences and the innate organic tendencies."

Under geographical variation many interesting facts have been added since Professor Baird, Dr. Allen, and Mr. Ridgway published their capital discoveries calling attention to the variations observed in birds and mammals coincident with their latitudinal range. William Bartram, grandnephew of the famous botanist John Bartram, alludes to the effect of climate in modifying species. In speaking of birds he says: "The different soil and situation of the country may have contributed in some measure in forming and establishing the difference in size and qualities betwix them."

Dr. J. A. Allen shows marked geographical variation among North American mammals in respect to size. He shows that: "1. The maximum physical development of the individual is attained when the conditions of environment are most favorable to the life of the species. 2. The largest species of a group (genus, sub-family, or family, as the case may be) are found when the group to which they severally belong reaches its highest development, or when it has what may be termed its center of distribution. 3. The most typical or most generalized representatives of a group are found also near the center of distribution, outlying forms being generally more or less aberrant or specialized." In the study of the eggs of birds of the same species, north and south, Dr. Allen shows that in the south the eggs are less in number and smaller in size. Mr. Robert Ridgway calls attention to the geographical variation observed in Dendrœca.

The same author in a discussion of a paper by Salvin in the Transactions of the Zoological Society of London, on the relationships between the birds of Gaudalupe and the mainland, refers to the present genesis of species, and points to the increase in size of the bill and feet, the shorter tail and wings and darker colors, as characterizing them.

Dr. E. C. Coues in his studies regarding geographical variation in color among North American insectivorous mammals says: "My studies up to the present go to show a very interesting parallelism with the state of the case I have determined for other small mammals, notably the mice and gophers, and which my friend Mr. Allen has admirably brought out in his studies of the squirrels. In some cases I find almost identical effects of climatic or other conditions upon the shrews and the mice of particular localities, by which they acquire the same *facies loci*. Present indications are that the normal variability of the shrews in size, shape, and color is not less than has been determined to hold good in various other families of mammals." In this memoir Dr. Coues has verified a curious fact, first pointed out by Professor Baird, of the modifications of the premolar dentition which the western species collectively, as compared with the eastern, have undergone. "A striking peculiarity of all the western species, no matter how diverse in other respects, is to have the third premolar decidedly smaller than the fourth, while in all the species east of the Rocky mountains (with one possible exception), the same tooth is as large as, or larger than, the other. Of the fact there is no question. It may be observed in an instant, and is unmistakable. Its significance is another thing. Some of the western species are scarcely distinguishable, if at all, from the respective eastern analogues except by this character, and they all show it."

Prof. A. Hyatt finds in sponges geographical variation in color, referring to similar features in birds as recorded by Baird and others.

Prof. David S. Jordan, in a paper on the distribution of fresh water fishes, presents a concise series of propositions which govern these animals in the United States. They all point to the action and importance of physical conditions as governing distribution. Space will permit only the quoting of the last proposition, which is a summing up of his conclusions: "The distribution of fresh water fishes is dependent on (*a*) fresh water communication; on (*b*) character of stream, that is, of water, as to purity, depth, rapidity, vegetable growth, etc.; on (*c*) the character of the river bed, as to size, condition of bottom, etc.; on (*d*) climate, as determined by latitude and by elevation above the sea; and finally on (*e*) various unknown factors arising from the nature of the past history of the species in question, or from the geological history of the rivers."

Dr. James Lewis has observed a not unlike condition of things in the distribution of the fresh water mussels of Ohio and Alabama. By a series of tables he calls attention to what he believes is the occurrence of identical and equivalent species in the two systems of drainage, and suggests that, owing to the number of varieties characterizing the *Unionidæ*, they may be identical. This author has also studied the genus Io and its habits, and notices its variation coincident with latitude and temperature.

Dr. R. E. C. Stearns, in a paper on the circumpolar distribution of certain fresh water mussels and the identity of certain species, unites many hitherto recognized species of *Anodonta*. Dr. J. G. Cooper, in a study of the fossil and sub-fossil land shells of the United States, sees the strongest evidence in support of the idea that the older ones are the direct ancestors of certain forms living to-day.

Mr. R. P. Whitfield read a paper before the Boston Society of Natural History, showing changes produced in *Limnæa megasoma* when kept in an aquarium. Having at the outset three specimens, two of them finally died, and from the remaining one eggs were produced, presumably unimpregnated. These eggs hatched, and from these the next year came a second generation, which in turn produced a third generation the following year. The animal of *Limnæa* is hermaphrodite. Nevertheless, besides diminished size in the shell, it was observed that the male parts had disappeared and the liver had become considerably reduced in size. He shows that a diœcious species had in a short time become monœcious as a result of the new physical conditions of life in the constricted quarters of an aquarium.

An instructive paper by Dr. W. D. Hartman on the genus *Partula* of the Hawaiian Islands shows in the most convincing manner the effect of environment in modifying the species. He finds a common occurrence of hybrids among certain forms, the result of the union of proximate species, this hybridization occurring even between arboreal and ground species. Dr. Hartman states that "gravid females are often washed by heavy rains from a favored position to drier levels, where after a few generations the progeny becomes depauperated, and so stunted in size as to be mistaken for distinct species." Dr. W. H. Dall, in some general considerations regarding the environment of the deep-sea mollusks as compared with the shallow-water and littoral forms, shows how much the littoral forms have to contend with in the struggle for existence as compared with the deep-sea forms, and the delicate sculpture and extreme fragility of many of the shells occurring in the deeper abysses of the sea are to be explained on the ground of their habitat. Dr. Carl F. Gissler has presented some interesting evidences of the effect of chemico-physical influences in the evolution of the branchipod crustaceans.

(To be continued.)

THE AFRICAN DIAMOND MINES.

THE engravings represent sections of the famous Kimberley diamond mine, South Africa. The mine was discovered in 1871, and is by far the richest in South Africa. Gardner F. Williams, of Oakland, Cal., recently read a paper describing these mines before the American Institute of Mining Engineers, and from this we take the descriptions of the mine. Figs. 1 and 2 are sections. The drawings are taken from government reports, and represent the work done up to the end of 1883. The Kimberley has been worked as an open mine to a depth of nearly 500 feet, and a prospecting shaft has been sunk in the bottom to a further depth of 100 ft. The outer line, Fig. 1, shows the opening at the surface.

Below the red soil of the surface was found a decomposed or disintegrated diamond-bearing earth, which gradually changed into the "blue" or hard diamantiferous cement. This deposit was surrounded on the surface to the depth of 60 to 70 feet by yellow shale. Underlying the yellow shale is a black shale about 200 feet thick. Below the black shale is a very hard, amygdaloidal, igneous rock. It will be seen by the engravings presented that in depth the surrounding rocks are gradually encroaching on the diamond-bearing earth.

This is proved to be the case on the north, south, and east sides of the mine, and until recently it was assumed that the hard rock would also be found to encroach on the west side. The "blue" ground in that part of the mine has been very poor, and the hard wall rock has not been reached except by tunnels or drifts. This work shows the hard rock at two points, one 50 ft. above the other, to have a decided dip to the northwest or outward dip. Further work will have to be done to prove beyond a doubt the position of the wall rock on the west end of the claim.

The greatest drawback to the working of the mine has been the caving of the friable shales which surround it. As soon as the claims lying adjacent to the shale had been worked to any depth, the shale (or "reef," as it is usually called) commenced to subside and fall into the pit. The greater the depth attained, the more extensive have been the falls of the shale.

During the past few years the work of hoisting "blue" ground has been almost entirely stopped for months at a time, owing to the great masses of fallen reef which have to be removed.

The mining board had moved, under their direction, from the commencement of work on its mine to May 1, 1883, 10,328,489 loads of 16 cubic ft. It is assumed that

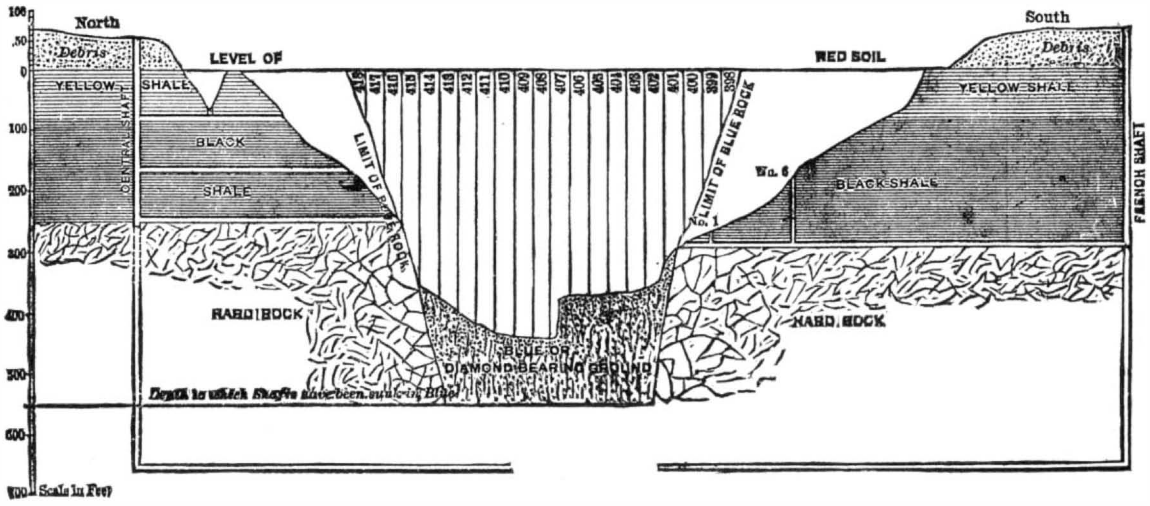


FIG. 1.—NORTH AND SOUTH SECTION OF KIMBERLEY DIAMOND MINE.

a load is equal to 10 cubic ft. of rock in place, which would give 3,824,440 cubic yards of solid rock.

In September, 1884, there was an immense cave, which completely buried a large portion of the mine and destroyed a large amount of machinery.—*Min. and Sci. Press.*

POULTRY NOTES.

LICE.

IT is sometimes an easy matter to get rid of lice on fowls, but the poultry house is not so easily managed. During July the lice will be active and increase rapidly. It is no use to attempt to rid the fowls of lice until the premises are thoroughly cleaned, as such labor is lost. If the houses are kept clean, the hens will, with the use of the dust bath, clean themselves. To rid the house of lice, first remove all filth from the roosts, floors, walls, and nests. Scrub the roosts with coal oil, not overlooking a single spot. Take the nests outside, clean them out, and with a whitewash brush apply a light coating of coal oil to them inside and outside. Now touch a lighted match to the nest boxes and let them burn. No damage will be done, as the oil will be quickly consumed, but such work should not be done inside the houses. Now make a bucket of whitewash, and add to it an ounce of liquid carbolic acid and a pint of tobacco water, which may be made by pouring boiling water over tobacco refuse and allowing the water to remain overnight with the tobacco. Apply the whitewash profusely, and dust Persian insect powder through the feathers of the hens, holding them by the legs for that purpose. Do not use grease on little chicks. Persian insect powder will remove lice from them. Little's chemical fluid is an excellent article to use in the place of carbolic acid, it being efficacious and non-poisonous.

WHY THERE ARE NO EGGS.

Many poultry raisers provide their fowls with warm quarters, and feed regularly and on a variety, but yet they get no eggs. Such cases are numerous, and we will endeavor to point out a remedy for the difficulty. We well know that if we keep a horse in the stable, and feed him well, that he becomes restless and unhappy, and in order to keep him in good health he must be exercised. With fowls, the winter prevents foraging, and our kind readers go to the coops in the

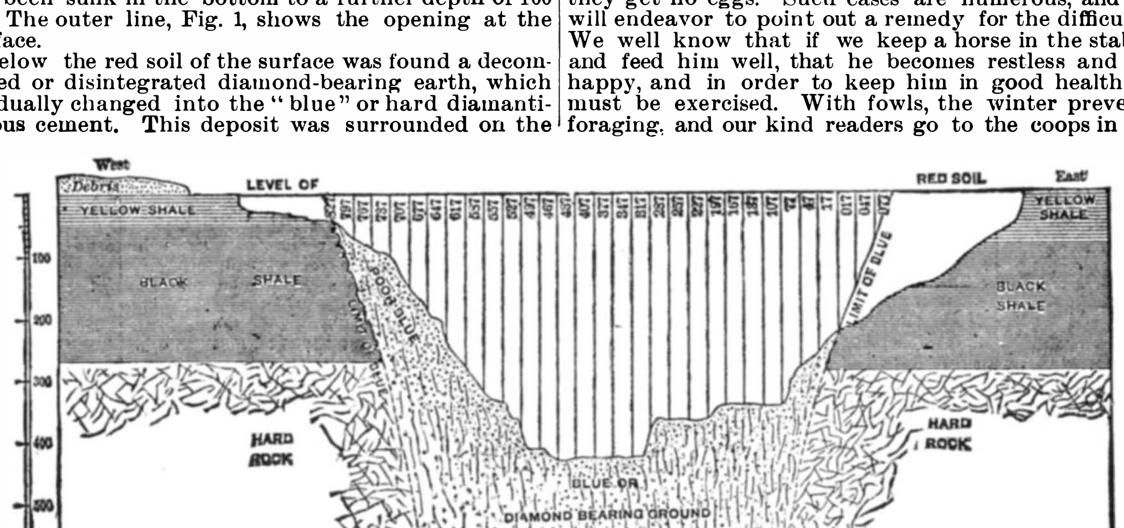


FIG. 2.—EAST AND WEST SECTION OF KIMBERLEY DIAMOND MINE.

morning and give the hens a good, heavy feeding. The hens being full are *satisfied*, and have no inducement to ramble, consequently do not take any exercise, and become too fat. The better plan is to get some chaff, cut straw, leaves, or even dirt, and place it where the hens can scratch in it.

In the morning give the hens a mess of warm food, but *only a little*. Now throw some grain into the scratching heap, and make them *work* for the balance of their meal. Feed nothing but what they will have to *work* for. At night feed them all they will eat. The object is to keep the hens busy during the day, but let them go on the roost full. Hens that are compelled to work will lay better and keep in good health, while the eggs will produce stronger chicks. They should always have a warm mess early in the morning, especially in the winter, but the meal should be so given as to leave them somewhat hungry. Do not feed them at noon, except by putting their food in the scratching heap, and never give soft food in the scratching heap. In other words, keep them scratching for oats, wheat, seeds, and even for ground shells. Give no corn except at night, and give them their night's meal without making them scratch for it.

CORN FEEDING.

Corn, when fed by itself, has a tendency to fatten hens rather than of producing the more profitable egg-laying result. Fat should be avoided on laying hens. Feed all the corn you desire to hens that are to be sent to market, but little to the laying hens. Wheat, oats, and buckwheat may be given, but not exclusively. Hens should have bulky food, the same as cows, allowing grain as a complement to the ration rather than to feed it entirely.

THE CHEMISTRY OF EGGS.

Chemistry shows us that a fair sized hen's egg weighs about 1,000 grains—600 grains constitute the white, 300 the yolk, and 100 the shell. The white of a hen's egg contains 84·8 per cent. of water, 12 of albumen, 2 of fat, sugar, and membranes, and ½ per cent. of mineral matter. The yolk shows a much greater degree of richness than the white; it contains 51·5 per cent. water, 15 of casein and albumen, 30 of oil and fat, 2·1 coloring matter and extract, and ¼ per cent. of mineral matter. Therefore to produce an egg we must first have the hen—then feed her what she needs to form eggs.

The hen is literally an egg machine—her chief purpose being the production of eggs. Like any other kind of machine, she must have the raw material with which to manufacture her products. Her instinct teaches her how to select; all that is necessary is to place within her reach that which she requires, and everything will be well, and eggs abundant and complete.

To produce an egg, the hen must have a certain kind of food for the yolk, or fat portion, known as carbonaceous; and for the white, she needs food rich in nitrogen, from which she makes albumen. For the shell she needs lime; while many other substances enter into the composition which it is unnecessary to detail, the omission of any of them being detrimental to good work on her part. Thus, while we may feed a hen liberally, apparently, by omitting to allow that which is needed to complete the laying process, she may remain idle for want of a single substance, though fully supplied with everything else necessary.

If the reproductive organs are unhealthy, the whole system and products are likewise affected.

PRECAUTIONS AGAINST HAWKS.

A large number of young chicks can be kept together in a small yard, if they are properly cared for. If we will take into consideration the number of chicks destroyed by hawks every year, the matter of providing some kind of protection would not appear as expensive as may be feared. Let us allow a yard 10 by 30 for 100 chicks, which is large enough, and as soon as the hens leave the chicks—that is, wean them—they may be put together. To cover this yard would require 300 feet of two-inch mesh wire netting, the price per foot being one cent, or $3 for the covering. To show the economy of this let it be considered, also, that on many farms hawks get one-half the chicks, and that, too, after they are quite large. The cost of the wire covering would be less than the price of ten chicks, while even the cats will have no chance to eat the chicks; and we will here state that the cat does as much damage as the hawk, only she knows enough not to let you find it out. A cheap covered yard will enable you to always have the chicks under control; they will grow faster, the loss will be less, and much anxiety will be saved, while the cost is very little.

ITEMS.

A little fresh meat occasionally in the absence of insects is good.

Eggs that are to be sold for hatching purposes should receive extra attention.

Poultry can be kept in small runs, even in large numbers, if their keeper understands the business and is scrupulously clean and careful.

The poultry business takes lots of hard work. It is no business for the lazy man to embark in. The labor given to it is ennobling; it disgraces no man.

Do not get any "bad habits" in your work among poultry. Ben Jonson says: "The chains of habit are generally too small to be felt till they are too strong to be broken."

Take good care of the young broods during chilly nights. Watch them carefully and take every precaution against vermin. Lice cause more loss in poultry than anything else.

Confine the old hen in a coop placed near the garden and see what havoc the chicks will make among the insects which have so worried you and destroyed your garden crops. Try it and you will be surprised at results.

Work quietly and gently among your fowls. Never allow them to become frightened. Never allow a strange dog on the place. Never allow yourself or your hens to become excited, and you will surely find the poultry business a profitable one.

Milk in any form, sweet or sour, is greatly relished by birds of all ages. Buttermilk is very acceptable and highly nutritious. It pays far better to feed your spare milk to chickens than it does to feed it to pigs. Try it one season.

One thing which favors the cultivation of poultry is the division of labor. Few industries to-day in the United States that show a healthier growth, or yield so fast a return to the American people in proportion to the amount of capital required and employed in carrying it on.

If we wish to accomplish the best results from our laying hens, we must give them gentle and agreeable exercise. We may feed them on the best and most varied kinds of food, all their other wants regularly and abundantly supplied, and unless they get the exercise they require, they will return but a small share of their real value.

BONE MEAL.

Bone dust for mixing in poultry food should be on an average about the fineness of fine oatmeal. There are usually large pieces interspersed, but these need not be taken out, as any too large will be rejected, though the meal may be sifted from any larger than peas if desired. The price never being very much more per pound than good meal, it should be used liberally with all the soft food, and about one ounce mixed with every half pint of dry meal before adding the milk or water. In small yards, cut grass must be liberally supplied as well to the mixture, and on such food the birds will grow wonderfully, and acquire a constitution which in confinement we have never been able to attain in any other way. We may say that burnt bones pounded have not by any means the same effect, being reduced to mere phosphate of lime, with some amount of animal charcoal; neither have crushed raw bones, which have been stated by some to produce similar results. On the contrary, raw bones have been proved by the very simple test of experiment to hasten laying in the pullet, and furnishing or feathering out to maturity in the cockerels, as might be expected from the amount of fresh jelly they contain; hence, while excellent in moderation for laying stock, or during a limited time to prepare cockerels for actual exhibition, they are not adapted for the regular food of chickens whose period for maturity the breeder for exhibition rather desires to postpone. That this postponement and with it continuous growth is effected by dry bone meal, we have most fully proved; and in the case of weakly breeds, which have it for its strengthening power, but which it is not wished to increase in size, the changing it at the proper time for raw bones will produce all the desired effect. We have often proved the value of bone meal, both in the rearing of laying and exhibition birds, but it is better not to use it for those that are intended to be killed for the table, as we do not want in them to develop bone.—*The Poultry Keeper.*

THE

Scientific American Supplement.

PUBLISHED WEEKLY.

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