# Scientific American Supplement, No. 787, January 31, 1891 eBook

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## THE FRENCH IRONCLAD WAR SHIP COLBERT.

The central battery ironclad Colbert is one of the ten ships of the French navy that constitute the group ranking next in importance to the squadron of great turret ships, of which the Formidable is the largest.  The group consists of six types, as follows:

1. The Ocean type; three vessels; the Marengo, Ocean, and Suffren.
2. The Friedland type, of which no others are built.
3. The Richelieu type, of which no others are built.
4. The Colbert type, of which there are two; the Colbert and the Trident.
5. The Redoubtable type, of which no others are built.
6. The Devastation type, of which no others are built.

[Illustration:  *The French ironclad War ship Colbert*.]

The Colbert was launched at Brest in 1875, and her sister ship, the Trident, in 1876. Both are of iron and wood, and the following are the principal dimensions of the Colbert, which apply very closely to the Trident:  She is 321 ft. 6 in. long, 59 ft. 6 in. beam, and 29 ft. 6 in. draught aft.  Her displacement is 8,457 tons, her indicated horse power is 4,652, and her speed 14.4 knots.  She has coal carrying capacity for 700 tons, and her crew numbers 706.  The thickness of her armor belt is 8.66 in., that protecting the central battery is 6.29 in. thick, which is also the thickness of the transverse armored bulkheads, while the deck is 0.43 in. in thickness.  The armament of the Colbert consists of eight 10.63 in. guns, two 9.45 in., six 5.51 in., two quick firing guns, and fourteen revolving and machine guns.—*Engineering.*

* * * * *

A compound locomotive, built by the Rhode Island Locomotive Works, has been tried on the Union Elevated Railroad, Brooklyn, N.Y.  The engine can be run either single or compound.  The economy in fuel was 37.7 per cent, and in water 23.8 per cent, over a simple engine which was tested at the same time.  The smoothness of running and the stillness and comparative absence of cinders was fully demonstrated.

* * * * *

## STEAM ENGINE VALVES.

[Footnote:  Lecture delivered at Wells Memorial Institute, Boston, in the Lowell Free Course for Engineers.  From report in the *Boston Journal of Commerce*.]

By *Thomas Hawley*.

*Riding cut-off valves—peculiarities and merits of the different Styles*.

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In considering the slide valve in its simple form with or without lap, we find there are certain limitations to its use as a valve that would give the best results. The limitation of most importance is that its construction will not allow of the proper cut off to obtain all the benefits of expansion without hindering the perfect action of the valve in other particulars. At this economical cut off the opening of the steam port is very little and very narrow, and although this is attempted to be overcome by exceedingly wide ports, sixteen inches in width in many cases in locomotive work, this great width adds largely to the unbalanced area of the valve. The exhausting functions of the valve are materially changed at the short cut off, and when much lap is added to overcome this defect, there usually takes place a choking of the exhaust port. You might inquire, why not make the port wider, but this would increase the minimum amount of load on the valve, and this must not be overlooked. Then the cut off is a fixed one, and we can govern only by throttling the pressure we have raised in the boiler or by using a cut off governor and the consequent wastes of an enormous clearance space. You will observe, therefore, that the plain slide valve engine gives the most general satisfaction at about two-thirds cut off and a very low economic result. The best of such engines will require forty-five to fifty pounds of steam per horse power per hour, and to generate this, assuming an evaporation of nine pounds of water to a pound of coal, would require between five and six pounds of coal per horse power per hour. And the only feature that the valve has specially to commend it is its extreme simplicity and the very little mechanism required to operate it.

Yet this is of considerable importance, and in consideration of some special features at its latest cut off, the attempt has been many times made to take advantage of these features. For instance, at 90 deg. advance, the valve opens very rapidly indeed and fully satisfies our requirements of a perfect valve. This is one good point, and in this position also the exhaust and compression can be regulated very closely and as desired without much lap, and as the opening of the exhaust port comes with the eccentric at its most rapid movement the release is very quick and as we would have it. This is only possible at the most uneconomic position of the valve as regards cut off.

The aim of many engineers has been to take advantage of these matters by using the valve with 90 deg. angular advance of eccentric ahead of crank, for the admission, release, and compression of the steam, and provide another means of cutting off, besides the one already referred to, *viz*., cutting off the supply of steam to the chest, and overcome the objection in this one of large clearance spaces. This is done by means of riding cut off valves, often called expansion valves, of which, perhaps, the most widely known types in this vicinity are the Kendall & Roberts engine and the Buckeye. The former is used in the simplest form of riding cut off, while the Buckeye has many peculiar features that engineers, I find, are too prone to overlook in a casual examination of the engine. In these uses of the slide valve, too, means are suggested and carried out of practically balancing the valve.

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The origin of the riding cut off is most generally attributed to Gonzenbach. His arrangement had two steam chests, the lower one provided with the ordinary slide valve of late cut off, and steam was cut off from this steam chest by the expansion valve covering the ports connecting with the upper steam chest. This had the old disadvantage that all the steam in the lower chest expanded with that in the cylinder, at a consequent considerable loss. This was further improved by causing the riding cut off to be upon the top of the main valve, instead of its chest, and resulted in a considerable reduction of the clearance space.

This is the simplest form, and is shown in Fig. 1. The steam is supplied by a passage through the main valve which operates exactly as an ordinary slide valve would. That is, the inside edges of the steam passage are the same as the ordinary valve, the additional piece on each end, if I may so term it, being merely to provide a passage for the steam which can be closed, instead of allowing the steam to pass the edge. The eccentric of the main valve is fastened to the shaft to give the proper amount of lead, and the desired release and compression, and the expansion valve is operated by a separate eccentric fastened in line with or 180 deg. ahead of the crank. When the piston, therefore, commences to move from the crank end to open the port, D, the expansion valve is forced by its eccentric in the opposite direction, and is closing the steam port and would have closed it before the piston reached quarter stroke, thus allowing the steam then in the cylinder to do work by expansion. The eccentric operating this expansion valve may be set to close this steam port at any point in the stroke that is desired, the closing occurring when the expansion valve has covered the steam port. Continuing the movements of the valves, the two would move together until one or the other reached its dead center, when the movements would be in opposite directions.

[Illustration: *Fig*. 1.]

There are three ways of effecting the cut off in such engines, the main valve meanwhile being undisturbed, its eccentric fastened securely so as not to disturb the points of lead, release, and compression. All that is required is to cause the edge of the expansion valve to cover the steam port earlier in the stroke, and this can be done, first, by increasing the angular advance of the cut off eccentric; second, by adding lap to the cut off valve; and third by changing the throw of the eccentric. In all these instances the riding valve is caused to reach the edge of the steam port earlier in the stroke. We will take first, as the simplest, those methods by which the lap of the cut off valve is increased.

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It will be noted that there is but one edge of this valve that is required to do any work, and that is to close the valve. The eccentrics are so placed that the passage in the main valve is opened long before the main valve itself is ready to admit steam to the cylinder, so that only the outer edges are the ones to be considered, and it will be readily seen that the two valves traveling in opposite directions, any lap added to the working edge of the cut off valve will cause it to reach the edge and therefore close the port earlier than it would if there was less lap. And we might carry it to the extreme that we could add lap enough that the steam passage would not be opened at all.

In Fig. 2 is shown the method by which this is accomplished, in what is called Meyer's valve, and such as is used in the Kendall & Roberts engine. We have only one point to look after, the cut off, so we can add all the lap we wish without disturbing anything else. In this engine the lap is changed by hand by means of a little hand wheel on a stem that extends out of the rear of the steam chest. The valve is in two sections, and when it is desired to cut off earlier, the hand wheel is turned in such a direction that the right and left hand screws controlling the cut off valve move one valve portion back and the other forward, which would, if they were one valve and they should be so considered, have the effect of lengthening them, or adding lap to them. The result would be that the riding valve would reach the edge of the steam port earlier in the stroke, bringing about an earlier cut off. If the cut off is desired to be later, the hand wheel is so turned that the right and left hand screws will bring the valve sections nearer together, thus practically taking off lap. Now this may be done by hand or it may be done by the action of a governor.

[Illustration: *Fig*. 2.]

In the latter case the governor at each change of load turns the right and left hand screws to add or take away lap, as the load demands an earlier or later cut off; in other cases the governor moves a rack in mesh with a gear by which the valve sections are brought closer together or are separated. The difficulty with the case where the hand wheel is turned by hand is that the cut off is fixed where you leave it, and governing can only be at the throttle. For this reason anywhere near full boiler pressure would not be obtained in the cylinder of the engine. If the load was a constant one, and the cut off could be fixed at about one-third, causing the throttle to open its widest, very good results would be obtained, but there is no margin left for governing.

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If the load should increase at such a time the governor could not control it under these conditions, and it would lead to a decrease in speed unless the lap was again changed to give a later cut off. On this account the general practice soon becomes to leave the cut off at the later point and give range to the throttle, and we come back once more to the plain slide valve cutting off at half stroke, and the only gain there is, is in a quick port opening and quick cut off. But these matters are more than offset by the wire drawing between the steam pipe and chest, through the throttle, and the fact that there is added to the friction of the engine the friction of this additional slide valve and a considerable liability to have a leaky valve.

In the case where the governor changes the position of the cut off valve a greater decree of economy would result. In this engine, of which the Lambertville engine is a type, the main valve is a long D slide, with multiple ports at the ends through which the steam enters the cylinders. It is operated from an eccentric on the crank shaft in the usual manner. The cut off valve is also operated from the motion on an eccentric fixed upon the crank shaft. The rod or stem of the cut off valve passes through the main valve rod and slide. Upon the outer end of the cut off valve rod are tappets fastened to engage with tappets on the eccentric valve rod. Connection between the cut off eccentric, therefore, and the cut off valve is only by means of the engagement of these tappets. The eccentric rod is fastened to a rocker arm having motion swinging about a pin or bearing in the governor slide, which may be raised or lowered by a cam operated by the governor. The cut off slide is of cylindrical shape and incloses a spring and dash pot with disks attached by means of which the valve is closed. The motion for operating the valves is relatively in the same direction, the cut off eccentric having the greatest throw and greater angular advance to cause it to open earlier and quickly before the main valve is ready to admit steam. The cut off eccentric rod swinging the rocker arm, the tappets thereon engage with those upon the cut off valve rod and open the passages to the main valve, and in their movement compress the spring in the main valve. According as the speed of the engine, the rock arm will be raised or lowered so that the tappets upon the eccentric rod may keep in engagement a shorter or longer time before they disengage, thus allowing the spring that has been compressed by the movement of the cut off valve to close that valve quickly and the supply of steam to the engine, the cut off valve traveling with the main valve for the balance of the stroke. This device will give a remarkably quick opening and a quick cut off, but in view of the fact that the governor has so much to do, its delicacy is impaired and a quick response to the demands of the load changing not so likely to occur. The cut off cannot be as quick as in some other engines, because the valves are moving in opposite directions, and while this fact would help, so far as shortening the distance to be traveled before cut off, the resistance of the valves to travel in opposite directions, or rather the tendency of the valve to travel with the main valve, hinders its rapid action.

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[Illustration: *Fig*. 3.]

This is one great objection to the rack and gear operated by the governor, that two flat valves riding upon each other and sliding in opposite directions at times require a considerable amount of force to move them, and as only a slight change in load is required by the load, the governor cannot handle the work as delicately as it should. It is too much for the governor to do well. To overcome this difficulty the Ryder cut-off, shown in Fig. 3, was made by the Delamater people, of New York. The main slide valve is hollowed in the back and the ports cut diagonally across the valve to form almost a letter V. The expansion valve is V-shaped, and circular to fit its circular-seat. The valve rod of the expansion valve has a sector upon it and operated by a gear upon the governor stem, which rotates the valve rod, and the edge of the valve rod is brought farther over the steam port, thus practically adding lap to the valve. Little movement is found necessary to make the ordinary change in cut-off, and it is found to be much easier to move the riding valve across the valve than in a direction directly opposite. It would require considerable force to move the upper valve by the governor faster than the lower, or in a direction opposite to that in which it is moving, but very little force applied sideways at the same time it is moving forward will give it a sideways motion. In this device the governor has only to exert this side pressure and therefore has less to do than if it were called upon to move the upper valve directly against the movement of the lower.

Something similar is the valve of the Woodbury engine, of Rochester, N.Y. The cut-off valve is cylindrical, covering diagonal ports directly opposite, and is caused to be rotated by the action of the governor that operates a rack in mesh with a segment. Very little movement will effect a considerable change in the lappage of the valve, the valve turning about one-quarter a revolution for the extremes of cut off. The cut off valve rod works through a bracket and its end terminates in a ball in a socket on the end of the eccentric rod. In this case the governor has not as much to do as in other instances.

[Illustration: *Fig*. 4.]

Still another method of effecting this change in cut off, but hardly by increasing the lap of the valve, is shown in the next drawing, Fig. 4. The cut off valve is held upon the main valve by the pressure of steam upon its back and rides with it until it comes in contact with the cut off wedge-shaped blocks, when its motion is arrested, and the main valve continuing its movement the steam port is closed by the main valve passing beneath the cut off valve. Thus the main valve travels and carries the cut off valve upon its back again until the cut off valve strikes the wedge on the other end and the cut off is effected. The relative positions of the blocks are determined by the governor, that will raise or lower them so

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that the cut off valve will engage with them earlier or later as desired. This device was designed specially as an inexpensive method of changing the common slide valve into an automatic cut off. The cut off would not be as quick as in other cases we have cited, depending here upon the movement of the lower valve alone, and that, too, is in its slowest movement; whereas in the other cases, the edges approaching each other, by the differing movement of the valves the cut off is very rapid, provided the distance to travel is not long. In this device considerable noise must result by the cut off valve striking the cut off blocks, and a considerable amount of leakage is likely to occur past this valve.

But there is one great objection in the valve gears thus far cited, that the travel of the expansion valve upon the main valve is variable. I have in mind the case of a Kendall & Roberts engine, which had been run for a long time at no better economy than would be obtained from a plain slide valve engine, and when it was attempted to get an earlier cut off by separating the two cut off valves, they had worn so much in their old place on the valve that shoulders were found sufficient to cause a disagreeable noise and a leaky valve. This is very apt to occur, not only where the valve is run for a long time on one seat, but in cases of variation of the travel of the expansion valve. The result is that a change will bring about a leaky valve, something that every engineer abhors.

The construction of the Buckeye engine, which is also of this type, is such that the travel of the valve on the back of the main valve is always the same, no matter what the cut off may be. Then this engine makes use of our second proposition as a means of effecting the cut off, *viz*., by advancing the eccentric. You will readily observe that anything that will cause the cut off valve to reach a certain point earlier in the stroke will bring about an earlier cut off as it hastens everything all around. This is the plan pursued in the Buckeye, in which the governor, of the shaft type, turns the eccentric forward or back according as the load demands. Then, in addition, the valve is balanced partially, the attempt not being made to produce an absolutely balanced valve, on the ground that there should be friction enough to keep the surfaces bright and to prevent leakage. The most perfect valve will, of course, be entirely balanced under all conditions of pressure so as to move with perfect ease. With the riding cut off valve in connection with the plain slide valve, this is not accomplished, and it does not matter whether it is partially unbalanced to prevent leakage or not, the fact that it is not entirely balanced prevents it reaching the ideal valve.

[Illustration: Fig. 5]

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This valve, Fig. 5, differs from the others also in this particular, that the exhaust takes place at the end of the valve instead of under the arch.  Two eccentrics are used, the one for the main valve being fastened to the shaft and the other riding loosely upon it and connected to the fly wheel governor, by which it may be turned forward or back as the load requires.  The three points of lead, or admission and exhaust and compression, are fixed and independent of the changes and cut off.  The motion of the main eccentric is given to a rocker arm, the pivot of which is at the bottom, and from the upper end the valve rod transfers the motion to the valve without reversing the motion, as is done sometimes in the slide valve to overcome the effects of the angularity of the connecting rod.  The action of the rocker arm, therefore, so far as the main valve in the Buckeye is concerned, is no different than that which would occur if no rocker arm intervened.  The motion of the cut off eccentric, through its eccentric rod, is given to a rocker rocking in a bearing in the center of the main rocker arm (see Fig. 6).  The motion of this eccentric is reversed, so far as the cut off valve is concerned, and when the cut off eccentric is moving forward, the cut off valve is being pushed back.  The main valve rod is hollow, and the cut off valve rod passes through it.

[Illustration:  Fig. 6]

The cut off eccentric can be placed in any position to cause it to cut off as desired, and by drawing the valve forward, by increasing the angular advance of the eccentric, the cut off valve is caused to reach and cover the steam passage in the main valve earlier in the stroke.  Instead of being ahead of the crank, the main eccentric in this arrangement follows the crank, on account of the exhaust and steam edges being exactly opposite from those in the ordinary slide.  What is the steam edge of the common slide is in this the exhaust edge, and what is the exhaust edge in the common valve is the steam edge in this one.  The valve, therefore, must be moved in the opposite direction from what is ordinarily the case, the main eccentric being not 90 deg. behind the crank.  It has a rapid and full opening just the same, for it is at this point behind the crank, or ahead of it, that the eccentric gives to the valve its quickest movement, or between the eccentric dead centers.  The cut off eccentric is considerably ahead of the main eccentric, and about even with the crank.  If it was not for the reversal of motion of the cut off valve through the rocker arm this eccentric would be about in line with the crank, but on the other end.  The movement of the cut off valve, therefore, at the time of port opening is very little, being about on its dead center, passing which, it immediately commences to close.

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The object of the peculiar construction of the rocker arm, and the pivot for the cut off rocker being placed thereon, is to provide equal travel on the back of the main valve, no matter what the cut off. I have already explained, in connection with the slide valve, that advancing the eccentric does not change the movement of the valve on its seat, but simply its relation to the movement of the piston. You will see that this is unchanged as using the main valve as a seat or any other seat. If the main valve was to remain stationary, and only the cut off valve to be operated by its eccentric, the movement of this cut off valve on a certain plane would be the same for all positions of the eccentric.

Moving the main slide does not affect the matter in any way, for it moves at the same time the pivot of the cut off, and while the cut off seat has assumed a different position with reference to the engine, it is still as though stationary so far as the cut off valve is concerned. This is the object of this peculiar construction, and not, as some engineers suppose, simply to make an odd way of doing things. And the object of it all is to give at all cut offs the same amount of travel, so that there might be no unequal wear to bring about a leak, to prevent which a perfect balancing has been sacrificed.

Referring to the valve and this engine as to how it will satisfy our requirements of a perfect valve gear, we find that the first requirement of a rapid and full opening is met, in that the opening occurs when the main eccentric is moving very rapidly, yet not its fastest, and while this opening will be very satisfactory, it is not so rapid an opening as is obtained in some other forms of valves and valve gears, but this could be overcome very readily by increasing the lead a trifle, and in my experience with these engines I find that the practice is very general by engineers and by builders themselves to give them a considerable amount of lead. As to the second requirement, the maintenance of initial pressure until cut off, giving a straight steam line, cards from this engine will not be found to show that the engine satisfies this requirement, and for this reason, that the cut-off valve commences to close the port immediately after the piston commences to move. The cut off eccentric you will remember is set to move with the crank or very nearly so, and the lighter the load, the greater will this fact appear. For the lightest loads the governor places the eccentric in advance of the crank, so that the cut off valve will commence to close the port before steam is admitted by the main valve to the engine. Now, the later the cut off, the less will this wire drawing appear at first, and the shorter the cut off, the amount of wire drawing increases sensibly. The operation of the valve, therefore, in this particular, cannot be considered as meeting our requirement that the port shall be held open full width until ready to be closed.

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Many men claim for this engine that the closing occurs when the cut off eccentric is moving its fastest. This is a fact, and if we consider the point of cut off only to be the point of absolute cut off, the cut off must be instantaneous, for there is an instantaneous point where the cut off is final only to be considered. The reasoning applied here would hold good also to a less extent on the slide valve, but is not the point of absolute cut off. We want to note how long it is from the time the valve commences to close at all until finally closed, and, as I have shown you, this is considerable in this engine.

Referring to the point of cut off finally, it is determined upon by a governor of the fly wheel type. The eccentric is loose about the shaft, and arms projecting therefrom are connected by other arms to the extremity of an arm upon which is mounted a weight, and which is attached to the spokes of the fly wheel, or special governor wheel in this case, and which is fastened to the crank shaft. As the speed increases through throwing off a portion of the load the governor weights fly out, and this movement is transferred through the lever connections to the eccentric, causing it to be turned ahead, and the manner hastening the movement of the cut off valve on its seat and causing it to reach and cover the edge of the steam port earlier in the stroke. This engine was the pioneer in governors of this character, the advantage being, in addition to its necessity for the work of turning the eccentric ahead or back, that the liability of the engine to run away, as very often happens from the breaking of the governor belt or a similar cause, was not possible.

The cut off valve has a travel considerably beyond the edge of the steam passage after the valve is closed, and this has one advantage, that the valve is less liable to leak, and to this must be added the loss from the friction of this moving valve, and moving too in opposition to the main valve. In our perfect valve, as we outlined it, the valve does not move after the port is closed. The exhausting functions of the valve are very good, giving a quick opening and a full opening, because this opening occurs when the eccentric is moving its fastest. The engine also possesses a distinct advantage in having remarkably small clearance spaces. The length of the steam passage is very small in comparison with any form of engine, and having but two ports instead of four, as in the Corliss and four valve type.

In these there must be included in the clearance, that to the exhaust port as well as the steam port, adding a considerable amount where the piston comes close to the head. As the engines leave the maker’s hand the engines are provided with a considerable amount of lap to give plenty of compression, but are, of course, capable of having more added to increase compression, or some planed off to decrease it.

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One of the peculiar things about this engine is the failure to realize anywhere near boiler pressure, noticeable in every case that has come under my notice. The considerable lead gives it for an instant, but it soon falls away, indicating the steam chest pressure only by a peak at the junction of the admission and steam lines. This is probably due to the fact that the cut off valve commences closing the steam passage so soon after steam is admitted, and in this particular does not satisfy the requirements of a perfect valve. There is this about the engine, that above all others of this type there has come under my notice fewer engines of this type with a maladjustment of valves from tampering by incompetent engineers.

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## FIRING POINTS OF VARIOUS EXPLOSIVES.

An apparatus, devised by Horsley, was used, which consisted of an iron stand with a ring support holding a hemispherical iron vessel, in which paraffin or tin was put. Above this was another movable support, from which a thermometer was suspended and so adjusted that its bulb was immersed in molten material in the iron vessel. A thin copper cartridge case, 5/8 in. in diameter and 1-5/16 in. long, was suspended over the bath by means of a triangle, so that the end of the case was 1 in. below the surface of the liquid. On beginning the experiment the material in the bath was heated to just above the melting point, the thermometer was inserted in it, and a minute quantity of the explosive was placed in the bottom of the cartridge case. The temperature marked by the thermometer was noted as the *initial temperature*, the cartridge case containing the explosive was inserted in the bath, and the temperature quickly raised until the explosive flashed off or exploded, when the temperature marked by the thermometer was again noted as the *firing point*. The tables given show the results of about six experiments with each explosive. The initial temperatures range from 65 deg. to 280 deg. C. in some cases, but as the firing points remained fairly constant, only the extremes of the latter are quoted in the following table:

| Description of Explosive. | Firing Point in deg. C. |
|---|---|
| Compressed military gun-cotton. | 186 — 201 |
| Air-dried military gun-cotton. | 179 — 186 |
| " " " | 186 — 189 |
| " " " | 137 — 139 |
| " " " | 154 — 161 |
| Gun-cotton dried at 65 deg. C. | 136 — 141 |
| Air-dried collodion gun-cotton. | 186 — 191 |

| | |
|---|---|
| " " " | 197 — 199 |
| " " " | 193 — 195 |
| Air-dried gun-cotton. | 192 — 197 |
| " " | 194 — 199 |
| Hydro-nitrocellulose. | 201 — 213 |
| Nitroglycerin. | 203 — 205 |

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| | |
|---|---|
| Kieselghur dynamite. No. 1. | 197 — 200 |
| Explosive gelatin. | 203 — 209 |
| Explosive gelatin, camphorated. | 174 — 182 |
| Mercury fulminate. | 175 — 181 |
| Gunpowder. | 278 — 287 |
| Hill's picric powder. | 273 — 283 |
| " " " | 273 — 290 |
| Forcite, No. 1. | 184 — 200 |
| Atlas powder, 75 per cent. | 175 — 185 |
| Emmensite, No. 1. | 167 — 184 |
| Emmensite, No. 2. | 165 — 177 |
| Emmensite, No. 5. | 205 — 217 |

_—C.E. Munroe, J. Amer. Chem. Soc._

* * * * *

## STATION FOR TESTING AGRICULTURAL MACHINES.

The minister of agriculture has recently established a special laboratory for testing agricultural *materiel*. This establishment, which is as yet but little known, is destined to render the greatest services to manufacturers and cultivators.

In fact, agriculture now has recourse to physics and mechanics as well as to chemistry. Now, although there were agricultural laboratories whose mission it was to fix the choice of the cultivator upon such or such a seed or fertilizer, there was no official establishment designed to inform him as to the value of machines, the models of which are often very numerous. *Chemical* advice was to be had, but *mechanical* advice was wanting. It is such a want that has just been supplied. Upon the report presented by Mr. Tisserand, director of agriculture, a ministerial decree of the 24th of January, 1888, ordered the establishment of an experimental station. Mr. Ringelmann, professor of rural engineering at the school of Grignon, was put in charge of the installation of it, and was appointed its director. He immediately began to look around for a site, and on the 17th of December, 1888, the Municipal Council of Paris, taking into consideration the value of such an establishment to the city's industries, decided that a plot of ground of an area of 3,309 square meters, situated on Jenner Street, should be put at the disposal of the minister of agriculture for fifteen years for the establishment thereon of a trial station. This land, bordering on a very wide street and easy of access, opposite the municipal buildings, offers, through its area, its situation, and its neigborhood, indisputable advantages.  A fence 70 meters in extent surrounds the station.  An iron gate opens upon a paved path that ends at the station.

The year 1889 was devoted to the installation, and the station is now in full operation. The tests that can be made here are many, and concern all kinds of apparatus, even those connected with the electric lighting that the agriculturist may employ to facilitate his exploitation.  However, the tests that are oftenest made are (1) of rotary apparatus, such as mills, thrashing machines, *etc*.; (2) of traction machines, such as wagons, carts, plows, *etc*.; and (3) of lifting apparatus.  It is possible, also, to make experiments on the resistance of materials.

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The experimental hall contains a 7 horse power gas motor, dynamometers with automatic registering apparatus, counters, balances, *etc*. A small machine shop contains a lathe, a forge, a drilling machine, *etc*. The main shaft is 12 meters in length and is 7 centimeters in diameter. It is supported at a distance of one meter from the floor by four pillow blocks, and is formed of three sections united by movable coupling boxes. Out of these 12 meters, 9 are in the hall and 3 extend beyond the hall to an annex, 14 meters in length and 4 in width, in which tests are made of machines whose operation creates dust. When the machines to be tested require more than the power of seven horses that the motor gives, the persons interested furnish a movable engine, which, placed under the annex, actuates the driving shaft. Alongside of the main building there is a ring for experimenting upon machines actuated by a horse whim. There will soon be erected in the center of the grounds an 18 meter tower for experiments on pumps. Platforms spaced 5 meters apart, a crane at the top, and some gauging apparatus will complete this hydraulic installation.

The equipment of the hall is very complete, and is fitted for all kinds of experiments.

[Illustration: *Station for testing agricultural machines—dynamometer for testing rotary machines*.]

The tests of rotary machines are made by means of a dynamometer (see figure). Two fast pulleys and one loose pulley are interposed between the machine to be tested and the motor. The pulley connected with the motor carries along the one connected with the machine, through the intermedium of spring plates, whose strength varies with the nature of the apparatus to be tested. The greater or less elongation of these plates gives the tangential stress exerted by the driving pulley to carry along the pulley that actuates the machine to be tested. This elongation is registered by means of a pencil connected with the spring plates, and which draws a diagram upon a sheet of paper. At the same time, a special totalizer gives the stress in kilogrammeters. Besides, the pulley shaft actuates a revolution counter, and a clock measures the time employed in the experiment. In order to obtain a simultaneous starting and stopping point for all these apparatus, they are connected electrically, and, through the maneuver of a commutator, are all controlled at once. The electric current is furnished by two series of bichromate batteries.

The tests of traction machines are effected by means of a three-wheeled vehicle carrying a dynamometer. The front wheel is capable of turning freely in the horizontal plane, and the dynamometer is mounted upon a frame provided with a screw that permits of regulating its position according to the slope of the ground. The method of suspension of the dynamometer allows it to take automatically the inclination of the line of traction without any torsion of the plates. There are two models of this vehicle, one designed to be drawn by a man, and the other by a horse.

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The station is provided, in addition, with registering pressure gauges, a large double dynamometric indicator, a counter of electricity, balances of precision, *etc*.

An apparatus designed for measuring the rendering of presses is now in course of construction.

Although the station has been in operation only from the 1st of January, twenty-five machines have already been presented to be tested.—*Extract from Le Genie Civil*.

* * * * *

## WATER SOFTENING AND PURIFYING APPARATUS.

We have recently had brought under our notice a system of water and sewage purification which appears to possess several substantial advantages. Chief among these are simplicity in construction and operation, economy in first cost and working and efficiency in action. This system is the invention of Messrs. Slack & Brownlow, of Canning Works, Upper Medlock Street, Manchester, and the apparatus adopted in carrying it out is here illustrated. It consists of an iron cylindrical tank having inside a series of plates arranged in a spiral direction around a fixed center, and sloping downward at a considerable angle outward. The water to be purified and softened flows through the large inlet tube to the bottom, mixing on its way with the necessary chemicals, and entering the apparatus at the bottom, rises to the top, passing spirally round the whole circumference, and depositing on the plates all solids and impurities.

All that is needed in the way of attention, even when dealing with sewage, or the most polluted waters, is stated to be the mixing in the small tanks the necessary chemical reagents, at the commencement of the working day; and at the close of the day the opening of the mud cocks shown in our engraving, to remove the collected deposit upon the plates. For the past six months this system has been in operation at a dye works in Manchester, successfully purifying and softening the foul waters of the river Medlock. It is stated that 84,000 gallons per day can be easily purified by an apparatus 7 feet in diameter. The chemicals used are chiefly lime, soda, and alumina, and the cost of treatment is stated to vary from a farthing to twopence per 1,000 gallons, according to the degree of impurity of the water or sewage treated.

The results of working at Manchester show that all the visible filth is removed from the Medlock's inky waters, besides which the hardness of the water is reduced to about 6 deg. from a normal condition of about 30 deg.. The effluent is fit for all the varied uses of a dye works, and is stated to be perfectly capable of sustaining fish life. With results such as these the system should have a promising future before it in respect of sewage treatment, as well as the purification and softening of water generally for industrial and manufacturing purposes.—*Iron.*

[Illustration: *Water softening and purifying apparatus*.]

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## THE TRISECTION OF ANY ANGLE.

By *Frederic* R. *Honey*, Ph.B., Yale University.

The following analysis shows that with the aid of an hyperbola any arc, and therefore any angle, may be trisected.

If the reader should not care to follow the analytical work, the construction is described in the last paragraph—referring to Fig.  II.

Let a b c d (Fig.  I.) be the arc subtending a given angle.  Draw the chord a d and bisect it at o.  Through o draw e f perpendicular to a d.

We wish to find the locus of a point c whose distance from a given straight line e f is one-half the distance from a given point d.

In order to write the equation of this curve, refer it to the co-ordinate axes a d (axis of X) and e f (axis of Y), intersecting at the origin o.

Let g c = x

Therefore, from the definition c d = 2x

Let o d = D
[Hence] h d = D-x

Let c h = y
[Hence] (2x) squared = y squared + (D-x) squared
or 4x squared = y squared + D squared-2Dx + x squared
[Hence] y squared-3x squared + D squared-2Dx = o [I.]

This is the equation of an hyperbola whose center is on the axis of abscisses.  In order to determine the position of the center, eliminate the x term, and find the distance from the origin o to a new origin o'.

Let E = distance from o to o'
[Hence] x = x' + E

Substituting this value of x in equation I.

y squared-3(x' + E) squared + D squared-2D(x' + E) = o
or y squared-3x squared-6Ex'-3E squared + D squared-2Dx'-2*de* = o [*ii*.]

In this equation the x’ terms should disappear.

[Hence] -6Ex’ — 2Dx’ = o
[Hence] -E = — D/3

That is, the distance from the origin o to the new origin or the center of the hyperbola o’ is equal to one-third of the distance from o to d; and the minus sign indicates that the measurement should be laid off to the left of the origin o. Substituting this value of E in equation *ii*., and omitting accents—

We have

y squared — 3x squared + 2Dx — D squared/3 + D squared — 2Dx + 2D squared/3 = o
[Hence] y squared — 3x squared = — 4D squared/3

[Illustration: Fig I]

[Illustration: Fig *ii*]

This is the equation of an hyperbola referred to its center o’ as the origin of co-ordinates. To write it in the ordinary form, that is in terms of the transverse and conjugate axes, multiply each term by C, *i.e.*,

Let $\sqrt{C}$ = semi-transverse axis.

[TEX: \sqrt{C} = \text{semi-transverse axis.}]

Thus Cy squared — 3Cx squared = — 4CD squared/3. [III.]

When in this form the product of the coefficients of the x squared and y squared terms should be equal to the remaining term.

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That is

3C squared = — 4CD squared/3.
[Hence] C = 4D squared/9.

And equation III. becomes:

```
4D squared        4D squared          16D^{4}
----- y squared - ----- x squared =  - ---------
9          3               27
```

[TEX: \frac{4D^2}{9} y^2 — \frac{4D^2}{3} x^2 = -\frac{16D^4}{27}]

```
 ____
/ 4D squared      2D
The semi-transverse axis =  \/ ----- =   ----
9         3
```

[TEX: \text{The semi-transverse axis} = \sqrt{\frac{4D^2}{9}}
= \frac{2D}{3}]

```
                          _____
                         / 4D squared 2D
  The semi-conjugate axis = \/ ----- = -----
                            3 ___
                               \/ 3
```

[TEX: \text{The semi-conjugate axis} = \sqrt{\frac{4D^2}{3}} = \frac{2D}{\sqrt{3}}]

Since the distance from the center of the curve to either focus is equal to the square root of the sum of the squares of the semi-axes, the distance from o' to either focus

```
_____________
4D squared     4D squared     4D
= \  /----- +  ----- =  ----
\/   9        3       3
```

[TEX: \sqrt{\frac{4D^2}{9} + \frac{4D^2}{3}} = \frac{4D}{3}]

We can therefore make the following construction (Fig. II.) Draw a d the chord of the arc a c d. Trisect a d at o' and k. Produce d a to l, making a l = a o' = o' k = k d. With a k as a transverse axis, and l and d as foci, construct the branch of the hyperbola k c c' c”, which will intersect all arcs having the common chord a d at c, c’, c”, *etc*., making the arcs c d, c’ d, c” d, *etc*., respectively, equal to one-third of the arcs a c d, a c’ d, a c” d, *etc*.

* * * * *

# TEST CARD HINTS.

By Dr. F. *Ogden stout*.

I know it is the custom with a great many if not the majority of opticians to fit a customer without knowing whether he has presbyopia, hypermetropia, or any of the other errors of refraction.  Their method is first to try a convex, and if this does not improve, a concave, *etc*., until the proper one is found.  This, of course, amounts to the same thing if the right glass is found.  But in practice it will be found both time saving and more satisfactory to first decide with what error you have to deal.  It is very simple, and, where you have no other means of diagnosing (such as the ophthalmoscope), it does away with the necessity of trying so many lenses before the proper one is found.  You should have a distance test card placed at a distance of twenty feet from the person you are examining, and in a good light.

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A distance test card consists of letters of various sizes which it has been found can be seen at certain distances by people with good vision. Thus the largest letter is marked with a cc, meaning that this should be seen at two hundred feet, and another line, XX, at twenty feet, which is the proper distance for testing vision for distance, for the reason that a normal eye is at rest when looking at any object twenty feet from it or beyond, and the rays coming from it are parallel and come to a focus on the retina. You must also have a near vision test card with lines that should be seen by a normal eye from ten to seventy-two inches, and a card of radiating lines for astigmatism. With this preparation you are ready to proceed. To illustrate, the first customer comes and tells you that up to six months ago he had very good vision, but he finds now that, especially at night, he has trouble in reading or writing, and that he finds he can see better a little farther away. His head aches and eyes smart. You will of course say that this is a very simple case. It must be old sight (presbyopia). Probably it is if he is old enough (45), but you must prove this for yourself, without asking his age, which is embarrassing in the case of a lady. If you direct him to the distance card twenty feet away, and find that he can see every one down to and including the one marked XX, his vision is up to the standard for distance, and you know that he can have no astigmatism worth correcting, nor any near sight, as both of these affect vision for distance, but he may have far sight or old sight or both combined. You must find which it is.

If, while he is still looking at the twenty-foot line, you place in front of the eyes a weak convex and he tells you he sees just as well with as without, it proves the existence of far-sight or hypermetropia, and the strongest convex that still leaves vision as good for distance as without any, corrects the manifest. But if the weak convex blurs it, it shows that there is some defect in focusing, if the near vision is below normal. You therefore know that you have a case of old sight or presbyopia, requiring the weakest convex to correct it, that will enable your customer to see the finest line on the near card at the required distance.

The next customer that comes to be fitted with glasses can only see the line marked XL on the distance card at 20 feet or about one-half of what he should see, which leads you to think that there is no far sight, for vision for distance is good except in very high degrees of this error. Nor can there be old-sight, for vision for distance is good in old-sight until after the fifty-fifth year, but it can be near sight (myopia) or astigmatism, or both. We next try the near card and find that even the finest line can be seen clearly if held sufficiently close to the eyes. We now know that this is a case of near sight, and we must fit them with glasses for distance. The weakest concave that will enable him to see the line that should be seen on the distance card at 20 feet is the proper one to give him for use.—*The Optician.*

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## CHARLES GOODYEAR.

Charles Goodyear was born in New Haven, December 29, 1800. He was the son of Amasa Goodyear, and the eldest among six children. His father was quite proud of being a descendant of Stephen Goodyear, one of the founders of the colony of New Haven in 1638.

Amasa Goodyear owned a little farm on the neck of land in New Haven which is now known as Oyster Point, and it was here that Charles spent the earliest years of his life. When, however, he was quite young, his father secured an interest in a patent for the manufacture of ivory buttons, and looking for a convenient location for a small mill, settled at Naugatuck, Conn., where he made use of the valuable water power that is there. Aside from his manufacturing, the elder Goodyear ran a farm, and between the two lines of industry kept young Charles pretty busy.

In 1816, Charles left his home and went to Philadelphia to learn the hardware business. He worked at this very industriously until he was twenty-one years old, and then, returning to Connecticut, entered into partnership with his father at the old stand in Naugatuck, where they manufactured not only ivory and metal buttons, but a variety of agricultural implements, which were just beginning to be appreciated by the farmers. In August of 1824 he was united in marriage with Clarissa Beecher, a woman of remarkable strength of character and kindness of disposition, and one who in after years was of the greatest assistance to the impulsive inventor. Two years later he removed again to Philadelphia, and there opened a hardware store. His specialties were the valuable agricultural implements that his firm had been manufacturing, and after the first distrust of home made goods had worn away—for all agricultural implements were imported from England at that time—he found himself established at the head of a successful business.

This continued to increase until it seemed but a question of a few years until he would be a very wealthy man. Between 1829 and 1830 he suddenly broke down in health, being troubled with dyspepsia. At the same time came the failure of a number of business houses that seriously embarrassed his firm. They struggled on, however, for some time, but were finally obliged to fail. The ten years that followed this were full of the bitterest struggles and trials to Goodyear. Under the law that then existed he was imprisoned time after time for debts, even while he was trying to perfect inventions that should pay off his indebtedness.

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Between the years 1831 and 1832 he began to hear about gum elastic and very carefully examined every article that appeared in the newspapers relative to this new material. The Roxbury Rubber Company, of Boston, had been for some time experimenting with the gum, and believing that they had found means for manufacturing goods from it, had a large plant and were sending their goods all over the country. It was some of their goods that first attracted his attention. Soon after this Goodyear visited New York, and went at once to the store of the Roxbury Rubber Company. While there, he examined with considerable care some of their life preservers, and it struck him that the tube used for inflation was not very perfect. He, therefore, on his return to Philadelphia, made some tubes and brought them down to New York and showed them to the manager of the Roxbury Rubber Company.

This gentlemen was so pleased with the ingenuity that Goodyear had shown in manufacturing these tubes, that he talked very freely with him and confessed to him that the business was on the verge of ruin, that the goods had to be tested for a year before they could tell whether they were perfect or not, and to their surprise, thousands of dollars worth of goods that they had supposed were all right were coming back to them, the gum having rotted and made them so offensive that it was necessary to bury them in the ground to get them out of the way.

Goodyear at once made up his mind to experiment on this gum and see if he could not overcome its stickiness.

He, therefore, returned to Philadelphia, and, as usual, met a creditor, who had him arrested and thrown into prison. While there, he tried his first experiments with India rubber. The gum was very cheap then, and by heating it and working it in his hands, he managed to incorporate in it a certain amount of magnesia which produced a beautiful white compound and appeared to take away the stickiness.

He therefore thought he had discovered the secret, and through the kindness of friends was put in the way of further perfecting his invention at a little place in New Haven. The first thing that he made here was shoes, and he used his own house for grinding room, calender room, and vulcanizing department, and his wife and children helped to make up the goods. His compound at this time was India rubber, lampblack, and magnesia, the whole dissolved in turpentine and spread upon the flannel cloth which served as the lining for the shoes. It was not long, however, before he discovered that the gum, even treated this way, became sticky, and then those who had supplied the money for the furtherance of these experiments, completely discouraged, made up their minds that they could go no further, and so told the inventor.

[Illustration: *Charles Goodyear*.]

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He, however, had no mind to stop here in his experiments, but, selling his furniture and placing his family in a quiet boarding place, he went to New York, and there, in an attic, helped by a friendly druggist, continued his experiments. His next step in this line was to compound the rubber with magnesia and then boil it in quicklime and water. This appeared to really solve the problem, and he made some beautiful goods. At once it was noised abroad that India rubber had been so treated that it lost its stickiness, and he received medals and testimonials and seemed on the high road to success, till one day he noticed that a drop of weak acid, falling on the cloth, neutralized the alkali, and immediately the rubber was soft again. To see this, with his knowledge of what rubber should do, proved to him at once that his process was not a successful one. He therefore continued experimenting, and after preparing his mixtures in his attic in New York, would walk three miles to the mill of a Mr. Pike, at Greenwich village, and there try various experiments.

In the line of these, he discovered that rubber, dipped in nitric acid, formed a surface cure, and he made a great many goods with this acid cure which were spoken of, and which even received a letter of commendation from Andrew Jackson.

The constant and varied experiments that Goodyear went through with affected his health more or less, and at one time he came very near being suffocated by gas generated in his laboratory. That he did not die then everybody knows, but he was thrown then into a fever by the accident and came very near losing his life.

It was there that he formed an acquaintance with Dr. Bradshaw, who was very much pleased with the samples of rubber goods that he saw in Goodyear’s room, and when the doctor went to Europe he took them with him, where they attracted a great deal of attention, but beyond that nothing was done about them. Now that he appeared to have success, he found no difficulty in obtaining a partner, and together the two gentlemen fitted up a factory and began to make clothing, life preservers, rubber shoes, and a great variety of rubber goods. They also had a large factory, with special machinery, built at Staten Island, where he removed his family and again had a home of his own. Just about this time, when everything looked bright, the great panic of 1836-1837 came, and swept away the entire fortune of his associate and left Goodyear without a cent, and no means of earning one.

His next move was to go to Boston, where he became acquainted with J. Haskins, of the Roxbury Rubber Company, and found in him a firm friend, who loaned him money and stood by him when no one would have anything to do with the visionary inventor. Mr. Chaffee was also exceedingly kind and ever ready to lend a listening ear to his plans, and to also assist him in a pecuniary way. It was about this time that it occurred to Mr. Chaffee that much of the trouble that they had experienced in working India rubber might come from the solvent that was used. He therefore invented a huge machine for doing the mixing by mechanical means. The goods that were made in this way were beautiful to look at, and it appeared, as it had before, that all difficulties were overcome.

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Goodyear discovered a new method for making rubber shoes and got a patent on it, which he sold to the Providence Company, in Rhode Island.

The secret of making the rubber so that it would stand heat and cold and acids, however, had not been discovered, and the goods were constantly growing sticky and decomposing and being returned.

In 1838 he, for the first time, met Nathaniel Hayward, who was then running a factory in Woburn.  Some time after this Goodyear himself moved to Woburn, all the time continuing his experiments.  He was very much interested in Hayward's sulphur experiments for drying rubber, but it appears that neither of them at that time appreciated the fact that it needed heat to make the sulphur combine with the rubber and to vulcanize it.

The circumstances attending the discovery of his celebrated process is thus described by Mr. Goodyear himself in his book, "Gum Elastic."  It will be observed that he makes use of the third person in all references to himself:

"In the summer of 1838 he became acquainted with Mr. Nathaniel Hayward, of Woburn, Mass., who had been employed as the foreman of the Eagle Company at Woburn, where he had made use of sulphur by impregnating the solvent with it.  It was through him that the writer (Charles Goodyear, who makes use all through his book of the third person) received the first knowledge of the use of sulphur as a drier of gum elastic."Mr. Hayward was left in possession of the factory which was abandoned by the Eagle Company.  Soon after this it was occupied by the writer, who employed him for the purpose of manufacturing life preservers and other articles by the acid gas process.  At this period he made many novel and useful applications of this substance.  Among other fancy articles he had newspapers printed on the gum elastic drapery, and the improvement began to be highly appreciated.  He therefore now entered, as he thought, upon a successful career for the future.  A far different result awaited him."It was supposed by others as well as himself that a change was wrought through the mass of the goods acted upon by the acid gas, and that the whole body of the article was made better than the native gum.  The surface of the goods really was so, but owing to the eventual decomposition of the goods beneath the surface, the process was pronounced by the public a complete failure.  Thus instead of realizing the large fortune which by all acquainted with his prospects was considered certain, his whole invention would not bring him a week's living."He was obliged for the want of means to discontinue manufacturing, and Mr. Hayward left his employment.  The inventor now applied himself alone, with unabated ardor and diligence, to detect the cause of his misfortune and if possible to retrieve the lost reputation of his invention.  On one occasion he made some experiments to ascertain

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the effect of heat upon the same compound that had decomposed in the articles previously manufactured, and was surprised to find that the specimen, being carelessly brought in contact with a hot stove, charred like leather. He endeavored to call the attention of his brother as well as some other individuals who were present, and who were acquainted with the manufacture of gum elastic, to this effect as remarkable and unlike any before known, since gum elastic always melted when exposed to a high degree of heat. The occurrence did not at the time appear to them to be worthy of notice. It was considered as one of the frequent appeals that he was in the habit of making in behalf of some new experiment. He, however, directly inferred that if the process of charring could be stopped at the right point, it might divest the gum of its native adhesiveness throughout, which would make it better than the native gum.“He made another trial of heating a similar fabric, before an open fire. The same effect, that of charring the gum, followed, but there were further and very satisfactory indications of ultimate success in producing the desired result, as upon the edge of the charred portions of the fabric there appeared a line, or border, that was not charred, but perfectly cured.

“These facts have been stated precisely as they occurred in reference to the acid gas, as well as the vulcanizing process.

“The incidents attending the discovery of both have a strong resemblance, so much so they may be considered parallel cases. It being now known that the results of the vulcanizing process are produced by means and in a manner which would not have been anticipated from any reasoning on the subject, and that they have not yet been satisfactorily accounted for, it has been sometimes asked, how the inventor came to make the discovery? The answer has already been given. It may be added that he was many years seeking to accomplish this object, and that he allowed nothing to escape his notice that related to the subject. Like the falling of an apple, it was suggestive of an important fact to one whose mind was previously prepared to draw an inference from any occurrence which might favor the object of his research. While the inventor admits that these discoveries were not the results of scientific chemical investigations, *he is not willing to admit that they were the result of what is commonly termed accident*; he claims them to be the result of the closest application and observation.“The discoloring and charring of the specimens proved nothing and discovered nothing of value, but quite the contrary, for in the first instance, as stated in the acid gas improvement, the specimen acted upon was thrown away as worthless and left for some time; in the latter instance, the specimen that was charred was in like manner disregarded by others.“It may, therefore, be considered

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as one of those cases where the leading of the Creator providentially aids his creatures, by what are termed ‘accidents,’ to attain those things which are not attainable by the powers of reasoning he has conferred on them.”

Now that Goodyear was sure that he had the key to the intricate puzzle that he had worked over for so many years, he began at once to tell his friends about it and to try to secure capital, but they had listened to their sorrow so many times that his efforts were futile. For a number of years be struggled and experimented and worked along in a small way, his family suffering with himself the pangs of the extremest poverty. At last he went to New York and showed some of his samples to William Ryder, who, with his brother Emory, at once appreciated the value of the discovery and started in to manufacturing. Even here Goodyear’s bad luck seemed to follow him, for the Ryder Bros. failed and it was impossible to continue the business.

He had, however, started a small factory at Springfield, Mass., and his brother-in-law, Mr. De Forest, who was a wealthy woolen manufacturer, took Ryder’s place, and the work of making the invention practical was continued. In 1844 it was so far perfected that Goodyear felt it safe to take out a patent. The factory at Springfield was run by his brothers, Nelson and Henry.

In 1843 Henry started one in Naugatuck, and in 1844 introduced mechanical mixing in place of the mixture by the use of solvents.

In the year 1852 Goodyear went to Europe, a trip that he had long planned, and saw Hancock, then in the employ of Charles Macintosh & Co. Hancock admitted in evidence that the first piece of vulcanized rubber he ever saw came from America, but claimed to have reinvented vulcanization and secured patents in Great Britain, but it is *a remarkable fact* that Charles Goodyear’s French patent was the first publication in Europe of this discovery.

In 1852 a French company were licensed by Mr. Goodyear to make shoes, and a great deal of interest was felt in the new business. In 1855 the French emperor gave to Charles Goodyear the grand medal of honor and decorated him with the cross of the legion of honor in recognition of his services as a public benefactor, but the French courts subsequently set aside his French patents on the ground of the importation of vulcanized goods from America by licenses under the United States patents. He died July 1, 1860, at the Fifth Avenue Hotel, New York City.—*India Rubber World*.

* * * * *

[Continued from SUPPLEMENT, No. 786, page 12558.]

# THE ELECTROMAGNET.

[Footnote: Lectures delivered before the Society of Arts, London, 1890. From the Journal of the Society.]

BY PROFESSOR SILVANUS P. THOMPSON, D. SC., B.A., M.I.E.E.

## III.

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RESEARCHES OF PROFESSOR HUGHES.

[Illustration: FIG. 51.—HUGHES' ELECTROMAGNET.]

His object was to find out the best form of electromagnet, the best distance between the poles, and the best form of armature for the rapid work required in Hughes' printing telegraphs. One word about Hughes' magnets. This diagram (Fig. 51) shows the form of the well known Hughes' electromagnet. I feel almost ashamed to say those words "well known," because on the Continent everybody knows what you mean by a Hughes' electromagnet. In England scarcely anyone knows what you mean. Englishmen do not even know that Professor Hughes has invented a special form of electromagnet. Hughes' special form is this: A permanent steel magnet, generally a compound one, having soft iron pole pieces, and a couple of coils on the pole pieces only. As I have to speak of Hughes' special contrivance among the mechanisms that will occupy our attention later on, I only now refer to this magnet in one particular. If you wish a magnet to work rapidly, you will secure the most rapid action, not when the coils are distributed all along, but when they are heaped up near, not necessarily entirely on, the poles. Hughes made a number of researches to find out what the right length and thickness of these pole pieces should be. It was found an advantage not to use too thin pole pieces, otherwise the magnetism from the permanent magnet did not pass through the iron without considerable reluctance, being choked by insufficiency of section: also not to use too thick pieces, otherwise they presented too much surface for leakage across from one to the other. Eventually a particular length was settled upon, in proportion about six times the diameter, or rather longer. In the further researches that Hughes made he used a magnet of shorter form, not shown here, more like those employed in relays, and with an armature from 2 to 3 millimeters thick, 1 centimeter wide and 5 centimeters long. The poles were turned over at the top toward one another. Hughes tried whether there was any advantage in making those poles approach one another, and whether there was any advantage in having as long an armature as 5 centimeters. He tried all the different kinds, and plotted out the results of observations in curves, which could be compared and studied. His object was to ascertain the conditions which would give the strongest pull, not with a steady current, but with such currents as were required for operating his printing telegraph instruments; currents which lasted but one to twenty hundredths of a second. He found it was decidedly an advantage to shorten the length of the armature, so that it did not protrude far over the poles. In fact, he got a sufficient magnetic circuit to secure all the attractive power that he needed, without allowing as much chance of leakage as there would have been had the armature extended a longer distance over the poles. He also tried various forms of armature having very various cross sections.

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## POSITION AND FORM OF ARMATURE.

In one of Du Moncel's papers on electromagnets[1] you will also find a discussion on armatures, and the best forms for working in different positions. Among other things in Du Moncel you will find this paradox: that whereas using a horseshoe magnet with fat poles, and a flat piece of soft iron for armature, it sticks on far tighter when put on edgeways; on the other hand, if you are going to work at a distance, across air, the attraction is far greater when it is set flatways. I explained the advantage of narrowing the surfaces of contact by the law of traction, B squared, coming in. Why should we have for action at a distance the greater advantage from placing the armature flatway to the poles? It is simply that you thereby reduce the reluctance offered by the air gap to the flow of the magnetic lines. Du Moncel also tried the difference between round armatures and flat ones, and found that a cylindrical armature was only attracted about half as strongly as a prismatic armature having the same surface when at the same distance. Let us examine this fact in the light of the magnetic circuit. The poles are flat. You have at a certain distance away a round armature; there is a certain distance between its nearest side and the polar surfaces. If you have at the same distance away a flat armature having the same surface, and, therefore, about the same tendency to leak, why do you get a greater pull in this case than in that? I think it is clear that if they are at the same distance away, giving the same range of motion, there is a greater magnetic reluctance in the case of the round armature, although there is the same periphery, because, though the nearest part of the surface is at the prescribed distance, the rest of the under surface is farther away; so that the gain found in substituting an armature with a flat surface is a gain resulting from the diminution in the resistance offered by the air gap.

[Footnote 1: "La Lumiere Electrique," vol. ii.]

## POLE PIECES ON HORSESHOE MAGNETS.

Another of Du Moncel's researches[2] relates to the effect of polar projections or shoes—movable pole pieces, if you like—upon a horseshoe electromagnet. The core of this magnet was of round iron 4 centimeters in diameter, and the parallel limbs were 10 centimeters long and 6 centimeters apart. The shoes consisted of two flat pieces of iron slotted out at one end, so that they could be slid along over the poles and brought nearer together. The attraction exerted on a flat armature across air gaps 2 millimeters thick was measured by counterpoising. Exciting this electromagnet with a certain battery, it was found that the attraction was greatest when the shoes were pushed to about 15 millimeters, or about one-quarter of the interpolar distance, apart. The numbers were as follows:

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| Distance between shoes. Millimeters. | Attraction, in grammes. |
| --- | --- |
| 2 | 900 |
| 10 | 1,012 |
| 15 | 1,025 |
| 25 | 965 |
| 40 | 890 |
| 60 | 550 |

[Footnote 2: “La Lumiere Electrique,” vol. iv., p. 129.]

With a stronger battery the magnet without shoes had an attraction of 885 grammes, but with the shoes 15 millimeters apart, 1,195 grammes. When one pole only was employed, the attraction, which was 88 grammes without a shoe, was *diminished* by adding a shoe to 39 grammes!

## CONTRAST BETWEEN ELECTROMAGNETS AND PERMANENT MAGNETS.

Now I want particularly to ask you to guard against the idea that all these results obtained from electromagnets are equally applicable to permanent magnets of steel; they are not, for this simple reason. With an electromagnet, when you put the armature near, and make the magnetic circuit better, you not only get more magnetic lines going through that armature, but you get more magnetic lines going through the whole of the iron. You get more magnetic lines round the bend when you put an armature on to the poles, because you have a magnetic circuit of less reluctance with the same external magnetizing power in the coils acting around it. Therefore, in that case, you will have a greater magnetic flux all the way round. The data obtained with the electromagnet (Fig. 42), with the exploring coil, C, on the bend of the core, where the armature was in contact, and when it was removed are most significant. When the armature was present it multiplied the total magnetic flow tenfold for weak currents and nearly threefold for strong currents. But with a steel horseshoe, magnetized once for all, the magnetic lines that flow around the bend of the steel are a fixed quantity, and, however much you diminish the reluctance of the magnetic circuit, you do not create or evoke any more. When the armature is away the magnetic lines arch across, not at the ends of the horseshoe only, but from its flanks; the whole of the magnetic lines leaking somehow across the space. Where you have put the armature on, these lines, instead of arching out into space as freely as they did, pass for the most part along the steel limbs and through the iron armature. You may still have a considerable amount of leakage, but you have not made one line more go through the bent part. You have absolutely the same number going through the bend with the armature off as with the armature on. You do not add to the total number by reducing the magnetic reluctance, because you are not working under the influence of a constantly impressed magnetizing force. By putting the armature on to a steel horseshoe magnet you only *collect* the magnetic lines, you do not *multiply* them. This is not a matter of conjecture. A group of my students have been making experiments in the following way:

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They took this large steel horseshoe magnet (Fig. 52), the length of which, from end to end, through the steel, is 421/2 inches. A light, narrow frame was constructed so that it could be slipped on over the magnet, and on it were wound 30 turns of fine wire, to serve as an exploring coil. The ends of this coil were carried to a distant part of the laboratory, and connected to a sensitive ballistic galvanometer. The mode of experimenting is as follows:

The coil is slipped on over the magnet (or over its armature) to any desired position. The armature of the magnet is placed gently upon the poles, and time enough is allowed to elapse for the galvanometer needle to settle to zero. The armature is then suddenly detached. The first swing measures the change, due to removing the armature, in the number of magnetic lines that pass through the coil in the particular position.

[Illustration: FIG. 52.—EXPERIMENT WITH PERMANENT MAGNET.]

I will roughly repeat the experiment before you: The spot of light on the screen is reflected from my galvanometer at the far end of the table. I place the exploring coil just over the pole, and slide on the armature; then close the galvanometer circuit. Now I detach the armature, and you observe the large swing. I shift the exploring coil, right up to the bend; replace the armature; wait until the spot of light is brought to rest at the zero of the scale. Now, on detaching the armature, the movement of the spot of light is quite imperceptible. In our careful laboratory experiments, the effect was noticed inch by inch all along the magnet. The effect when the exploring coil was over the bend was not as great as 1-3000th part of the effect when the coil was hard up to the pole. We are, therefore, justified in saying that the number of magnetic lines in a permanently magnetized steel horseshoe magnet is not altered by the presence or absence of the armature.

You will have noticed that I always put on the armature gently. It does not do to slam on the armature; every time you do so, you knock some of the so-called permanent magnetism out of it. But you may pull off the armature as suddenly as you like. It does the magnet good rather than harm. There is a popular superstition that you ought never to pull off the keeper of a magnet suddenly. On investigation, it is found that the facts are just the other way. You may pull off the keeper as suddenly as you like, but you should never slam it on.

From these experimental results I pass to the special design of electromagnets for special purposes.

# ELECTROMAGNETS FOR MAXIMUM TRACTION.

These have already been dealt with in the preceding lecture; the characteristic feature of all the forms suitable for traction being the compact magnetic circuit.

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Several times it has been proposed to increase the power of electromagnets by constructing them with intermediate masses of iron between the central core and the outside, between the layers of windings. All these constructions are founded on fallacies. Such iron is far better placed either right inside the coils or right outside them, so that it may properly constitute a part of the magnetic circuit. The constructions known as Camacho's and Cance's, and one patented by Mr. S.A. Varley, in 1877, belonging to this delusive order of ideas, are now entirely obsolete.

Another construction which is periodically brought forward as a novelty is the use of iron windings of wire or strip in place of copper winding. The lower electric conductivity of iron, as compared with copper, makes such a construction wasteful of exciting power. To apply equal magnetizing power by means of an iron coil implies the expenditure of about six times as many watts as need be expended if the coil is of copper.

## ELECTROMAGNETS FOR MAXIMUM RANGE OF ATTRACTION.

We have already laid down the principle which will enable us to design electromagnets to act at a distance. We want our magnet to project, as it were, its force across the greatest length of air gap. Clearly, then, such a magnet must have a very large magnetizing power, with many ampere turns upon it, to be able to make the required number of magnetic lines pass across the air resistance. Also it is clear that the poles must not be too close together for its work, otherwise the magnetic lines at one pole will be likely to curl round and take short cuts to the other pole. There must be a wider width between the poles than is desirable in electromagnets for traction.

## ELECTROMAGNETS OF MINIMUM WEIGHT.

In designing an apparatus to put on board a boat or a balloon, where weight is a consideration of primary importance, there is again a difference. There are three things that come into play—iron, copper, and electric current. The current weighs nothing, therefore, if you are going to sacrifice everything else to weight, you may have comparatively little iron, but you must have enough copper to be able to carry the electric current; and under such circumstances you must not mind heating your wires nearly red hot to pass the biggest possible current. Provide as little copper as you conveniently can, sacrificing economy in that case to the attainment of your object; but, of course, you must use fireproof material, such as asbestos, for insulating, instead of cotton or silk.

## A USEFUL GUIDING PRINCIPLE.

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In all cases of design there is one leading principle which will be found of great assistance, namely, that a magnet always tends so to act as though it tried to diminish the length of its magnetic circuit.  It tries to grow more compact.  This is the reverse of that which holds good with an electric current.  The electric circuit always tries to enlarge itself, so as to inclose as much space as possible, but the magnetic circuit always tries to make itself as compact as possible.  Armatures are drawn in as near as can be, to close up the magnetic circuit.  Many two-pole electromagnets show a tendency to bend together when the current is turned on.  One form in particular, which was devised by Ruhmkorff for the purpose of repeating Faraday's celebrated experiment on the magnetic rotation of polarized light, is liable to this defect.  Indeed, this form of electromagnet is often designed very badly, the yoke being too thin, both mechanically and magnetically, for the purpose which it has to fulfill.

Here is a small electric bell, constructed by Wagener, of Wiesbaden, the construction of which illustrates this principle.  The electromagnet, a horseshoe, lies horizontally; its poles are provided with protruding curved pins of brass.  Through the armature are drilled two holes, so that it can be hung upon the two brass pins; and when so hung up it touches the ends of the iron cores just at one edge, being held from more perfect contact by a spring.  There is no complete gap, therefore, in the magnetic circuit.  When the current comes and applies a magnetizing power, it finds the magnetic circuit already complete in the sense that there are no absolute gaps.  But the circuit can be bettered by tilting the armature to bring it flat against the polar ends, that being indeed the mode of motion.  This is a most reliable and sensitive pattern of bell.

[Illustration:  FIG. 53.—ELECTROMAGNETIC POP-GUN.]

*Electromagnetic Pop-gun.*—Here is another curious illustration of the tendency to complete the magnetic circuit.  Here is a tubular electromagnet (Fig. 53), consisting of a small bobbin, the core of which is an iron tube about two inches long.  There is nothing very unusual about it; it will stick on, as you see, to pieces of iron when the current is turned on.  It clearly is an ordinary electromagnet in that respect.  Now suppose I take a little round rod of iron, about an inch long, and put it into the end of the tube, what will happen when I turn on my current?  In this apparatus as it stands, the magnetic circuit consists of a short length of iron, and then all the rest is air.  The magnetic circuit will try to complete itself, not by shortening the iron, but by *lengthening* it; by pushing the piece of iron out so as to afford more surface for leakage.  That is exactly what happens; for, as you see, when I turn on the current, the little piece of iron shoots out and drops down.  You see that little piece of iron shoot out with considerable

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force.  It becomes a sort of magnetic popgun.  This is an experiment which has been twice discovered.  I found it first described by Count Du Moncel, in the pages of *La Lumiere Electrique*, under the name of the “pistolet electromagnetique;” and Mr. Shelford Bidwell invented it independently.  I am indebted to him for the use of this apparatus.  He gave an account of it to the Physical Society, in 1885, but the reporter missed it, I suppose, as there is no record in the society’s proceedings.

## ELECTROMAGNETS FOR USE WITH ALTERNATING CURRENTS.

When you are designing electromagnets for use with alternating currents, it is necessary to make a change in one respect, namely, you must so laminate the iron that internal eddy currents shall not occur; indeed, for all rapid-acting electromagnetic apparatus it is a good rule that the iron must not be solid.  It is not usual with telegraphic instruments to laminate them by making up the core of bundles of iron plates or wires, but they are often made with tubular cores, that is to say, the cylindrical iron core is drilled with a hole down the middle, and the tube so formed is slit with a saw cut to prevent the circulation of currents in the substance of the tube.  Now when electromagnets are to be employed with rapidly alternating currents, such as are used for electric lighting, the frequency of the alternations being usually about 100 periods per second, slitting the cores is insufficient to guard against eddy currents; nothing short of completely laminating the cores is a satisfactory remedy.  I have here, thanks to the Brush Electric Engineering Company, an electromagnet of the special form that is used in the Brush arc lamp when required for the purpose of working in an alternating current circuit.  It has two bobbins that are screwed up against the top of an iron box at the head of the lamp.  The iron slab serves as a kind of yoke to carry the magnetism across the top.  There are no fixed cores In the bobbins, which are entered by the ends of a pair of yoked plungers.  Now in the ordinary Brush lamp for use with a steady current, the plungers are simply two round pieces of iron tapped into a common yoke; but for alternate current working this construction must not be used, and instead a U-shaped double plunger is used, made up of laminated iron, riveted together.  Of course it is no novelty to use a laminated core; that device, first used by Joule, and then by Cowper, has been repatented rather too often during the past fifty years to be considered as a recent invention.

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The alternate rapid reversals of the magnetism in the magnetic field of an electromagnet, when excited by alternating electric currents, sets up eddy currents in every piece of undivided metal within range. All frames, bobbin tubes, bobbin ends, and the like, must be most carefully slit, otherwise they will overheat. If a domestic flat iron is placed on the top of the poles of a properly laminated electromagnet, supplied with alternating currents, the flat iron is speedily heated up by the eddy currents that are generated internally within it. The eddy currents set up by induction in neighboring masses of metal, especially in good conducting metals such as copper, give rise to many curious phenomena. For example, a copper disk or copper ring placed over the pole of a straight electromagnet so excited is violently repelled. These remarkable phenomena have been recently investigated by Professor Elihu Thomson, with whose beautiful and elaborate researches we have lately been made conversant in the pages of the technical journals. He rightly attributes many of the repulsion phenomena to the lag in phase of the alternating currents thus induced in the conducting metal. The electromagnetic inertia, or self-inductive property of the electric circuit, causes the currents to rise and fall later in time than the electromotive forces by which they are occasioned. In all such cases the impedance which the circuit offers is made up of two things—resistance and inductance. Both these causes tend to diminish the amount of current that flows, and the inductance also tends to delay the flow.

## ELECTROMAGNETS FOR QUICKEST ACTION.

I have already mentioned Hughes' researches on the form of electromagnet best adapted for rapid signaling. I have also incidentally mentioned the fact that where rapidly varying currents are employed, the strength of the electric current that a given battery can yield is determined not so much by the resistance of the electric circuit as by its electric inertia. It is not a very easy task to explain precisely what happens to an electric circuit when the current is turned on suddenly. The current does not suddenly rise to its full value, being retarded by inertia. The ordinary law of Ohm in its simple form no longer applies; one needs to apply that other law which bears the name of the law of Helmholtz, the use of which is to give us an expression, not for the final value of the current, but for its value at any short time, t, after the current has been turned on. The strength of the current after a lapse of a short time, t, cannot be calculated by the simple process of taking the electromotive force and dividing it by the resistance, as you would calculate steady currents.

In symbols, Helmholtz's law is:

$$i_{t} = E/R \left( 1 — e^{-(R/L)t} \right)$$

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In this formula $i_{t}$ means the strength of the current after the lapse of a short time t; E is the electromotive force; R, the resistance of the whole circuit; L, its coefficient of self-induction; and *e* the number 2.7183, which is the base of the Napierian logarithms. Let us look at this formula; in its general form it resembles Ohm's law, but with a new factor, namely, the expression contained within the brackets. The factor is necessarily a fractional quantity, for it consists of unity less a certain negative exponential, which we will presently further consider. If the factor within brackets is a quantity less than unity, that signifies that $i_{t}$ will be less than E / R. Now the exponential of negative sign, and with negative fractional index, is rather a troublesome thing to deal with in a popular lecture. Our best way is to calculate some values, and then plot it out as a curve. When once you have got it into the form of a curve, you can begin to think about it, for the curve gives you a mental picture of the facts that the long formula expresses in the abstract. Accordingly we will take the following case. Let E = 2 volts; R = 1 ohm; and let us take a relatively large self-induction, so as to exaggerate the effect; say let L = 10 quads. This gives us the following:

| $t_{(sec.)}$ | $e^{+(R/L)t}$ | $i_{t}$ |
|---|---|---|
| 0 | 1 | 0 |
| 1 | 1.105 | 0.950 |
| 2 | 1.221 | 1.810 |
| 5 | 1.649 | 3.936 |
| 10 | 2.718 | 6.343 |
| 20 | 7.389 | 8.646 |
| 30 | 20.08 | 9.501 |
| 60 | 403.4 | 9.975 |
| 120 | 16200.0 | 9.999 |

In this case the value of the steady current as calculated by Ohm's law is 10 amperes, but Helmholtz's law shows us that with the great self-induction which we have assumed to be present, the current, even at the end of 30 seconds, has only risen up to within 5 percent. of its final value; and only at the end of two minutes has practically attained full strength. These values are set out in the highest curve in Fig. 54, in which, however, the further supposition is made that the number of spirals, S, in the coils of the electromagnet is 100, so that when the current attains its full value of 10 amperes, the full magnetizing power will be Si = 1000. It will be noticed that the curve rises from zero at first steeply and nearly in a straight line, then bends over, and then becomes nearly straight again, as it gradually rises to its limiting value. The first part of the curve—that relating to the strength of the current after *very small* interval of time—is the period within which the strength of the current is governed by inertia (i.e., the self-induction) rather than by resistance. In reality the current is not governed either by the self-induction or by the resistance alone, but by the ratio of the two. This ratio is sometimes called the “time constant” of the circuit, for it represents *the time* which the current takes in that circuit to rise to a definite fraction of its final value.

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E = 10 r = 1 R = 100 L = 10

```
Si
1000 +            _..-------------------------------
|        .                   _ _---------
|       .              .----
|      .             .-  2 IN SERIES
|     .            .-
|    -
|   .:         - :
|   .:        .   :
500 |    . :     __-     -:---------------------------
|  .  :  _.- -      :        2 IN PARALLEL
| .   :.  -        :
| .  / :  -         :
| . /  -           :
|. / - :            :
|./.   :            :
|/_____:______________:______________________________ t
10     20     40     60     80    100    120
```

FIG. 54.—CURVES OF RISE OF CURRENTS.

This definite fraction is the fraction (e — 1)/e; or in decimals, 0.634.  All curves of rise of current are alike in general shape, they differ only in scale, that is to say, they differ only in the height to which they will ultimately rise, and in the time they will take to attain this fraction of their final value.

*Example (1).*—Suppose E = 10; R = 200 ohms; L = 8.  The final value of the current will be 0.025 amp. or 25 milliamperes.  Then the time constant will be 8 / 400 = 1-50th sec.

*Example (2).*—The P.O.  Standard "A" relay has R = 400 ohms; L = 3.25.  It works with 0.5 milliampere current, and therefore will work with 5 Daniell cells through a line of 9,600 ohms.  Under these circumstances the time constant of the instrument on short circuit is 0.0081 sec.

It will be noted that the time constant of a circuit can be reduced either by diminishing the self-induction or by increasing the resistance.  In Fig. 54 the position of the time constant for the top curve is shown by the vertical dotted line at 10 seconds.  The current will take 10 seconds to rise to 0.634 of its final value.  This retardation of the rise of current is simply due to the presence of coils and electromagnets in the circuit; the current as it grows being retarded because it has to create magnetic fields in these coils, and so sets up opposing electromotive forces that prevent it from growing all at once to its full strength.  Many electricians, unacquainted with Helmholtz's law, have been in the habit of accounting for this by saying that there is a lag in the iron of the electromagnet cores. They tell you that an iron core cannot be magnetized suddenly, that it takes time to acquire its magnetism. They think it is one of the properties of iron. But we know that the only true time lag in the magnetization of iron, that which is properly termed "viscous hysteresis," does not amount to any great percentage of the whole amount of magnetization, takes comparatively a long time to show itself, and cannot therefore be the cause of the retardation which we are considering. There are also electricians who will tell you that when magnetization is suddenly evoked in an iron bar, there are induction currents set up in the iron which

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oppose and delay its magnetization. That they oppose the magnetization is perfectly true, but if you carefully laminate the iron so as to eliminate eddy currents, you will find, strangely enough, that the magnetism rises still more slowly to its final value. For by laminating the iron you have virtually increased the self-inductive action, and increased the time constant of the circuit, so that the currents rise more slowly than before. The lag is not in the iron, but in the magnetizing current, and the current being retarded, the magnetization is of course retarded also.

## CONNECTING COILS FOR QUICKEST ACTION.

Now let us apply these most important though rather intricate considerations to the practical problems of the quick working of the electromagnet. Take the case of an electromagnet forming some part of the receiving apparatus of a telegraph system in which it is desired to secure very rapid working. Suppose the two coils that are wound upon the horseshoe core are connected together in series. The coefficient of self-induction for these two is four times as great as that of either separately; coefficients of self-induction being proportional to the square of the number of turns of wire that surround a given core. Now if the two coils instead of being put in series are put in parallel, the coefficient of self-induction will be reduced to the same value as if there were only one coil, because half the line current (which is practically unaltered) will go through each coil. Hence the time constant of the circuit when the coils are in parallel will be a quarter of that which it is when the coils are in series; on the other hand, for a given line current, the final magnetizing power of the two coils in parallel is only half what it would be with the coil in series. The two lower curves in Fig. 54 illustrate this, from which it is at once plain that the magnetizing power for very brief currents is greater when the two coils are put in parallel with one another than when they are joined in series.

Now this circumstance has been known for some time to telegraph engineers. It has been patented several times over. It has formed the theme of scientific papers, which have been read both in France and in England. The explanation generally given of the advantage of uniting the coils in parallel is, I think, fallacious; namely that the “extra currents” (i.e., currents due to self-induction) set up in the two coils are induced in such directions as tend to help one another when the coils are in series, and to neutralize one another when they are in parallel. It is a fallacy, because in neither case do they neutralize one another. Whichever way the current flows to make the magnetism, it is opposed in the coils while the current is rising, and helped in the coils while the current is falling, by the so-called extra currents. If the current is rising in both coils at the same moment, then, whether the coils are in series or in parallel, the effect of self-induction is to retard the rise of the current. The advantage of parallel grouping is simply that it reduces the time constant.

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## BATTERY GROUPING FOR QUICKEST ACTION.

One may consider the question of grouping the battery cells from the same point of view. How does the need for rapid working, and the question of time constant, affect the best mode of grouping the battery cells? The amateur's rule, which tells you to so arrange your battery that its internal resistance should be equal to the external resistance, gives you a result wholly wrong for rapid working. The supposed best arrangement will not give you (at the expense even of economy) the best result that might be got out of the given number of cells. Let us take an example and calculate it out, and place the results graphically before our eyes in the form of curves. Suppose the line and electromagnet have together a resistance of 6 ohms, and that we have 24 small Daniell cells, each of electromotive force say 1 volt, and of internal resistance 4 ohms. Also let the coefficient of self-induction of the electromagnet and circuit be 6 quadrants. When all the cells are in series, the resistance of the battery will be 96 ohms, the total resistance of the circuit 102 ohms, and the full value of the current 0.235 ampere. When all the cells are in parallel, the resistance of the battery will be 0.133 ohm, the total resistance 6.133 ohms, and the full value of the current 0.162 ampere. According to the amateur rule of grouping cells so that internal resistance equals external, we must arrange the cells in 4 parallels, each having 6 cells in series, so that the internal resistance of the battery will be 6 ohms, total resistance of circuit 12 ohms, full value of current 0.5 ampere. Now the corresponding time constants of the circuit in the three cases (calculated by dividing the coefficient of self-induction by the total resistance) will be respectively—in series, 0.06 sec.; in parallel, 0.5 sec.; grouped for maximum steady current, 0.96 sec. From these data we may now draw the three curves, as in Fig. 55, wherein the abscissae are the values of time in seconds and the ordinates the current. The faint vertical dotted lines mark the time constants in the three cases. It will be seen that when rapid working is required the magnetizing current will rise, during short intervals of time, more rapidly if all the cells are put in series than it will do if the cells are grouped according to the amateur rule.

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 |                                          .
4|                       MAXIMUM           .
 |                     OUTPUT \        .
 |                                .
3|                              .
 |                     .  :       ALL IN SERIES
 |        _------------------:------------------------------
2|      .-           —         :
 |   -          -          :
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|   -:          -                :
1| / :        —                      :              ALL IN PARALLEL
|.   :    .                      :              __________--------
|-   :__        :         ----------
+------------------------------:--------------------------------
0      1      2      3      4      5      6      7      8      9     10
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FIG. 55.—CURVES OF RISE OF CURRENT WITH DIFFERENT GROUPINGS OF BATTERY.

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When they are all put in series, so that the battery has a much greater resistance than the rest of the circuit, the current rises much more rapidly, because of the smallness of the time constant, although it never attains the same ultimate maximum as when grouped in the other way.  That is to say, if there is self-induction as well as resistance in the circuit, the amateur rule does not tell you the best way of arranging the battery.  There is another mode of regarding the matter which is helpful.  Self-induction, while the current is growing, acts as if there were a sort of spurious addition to the resistance of the circuit; and while the current is dying away it acts of course in the other way, as if there were a subtraction from the resistance.  Therefore you ought to arrange the battery so that the internal resistance is equal to the real resistance of the circuit, plus the spurious resistance during that time.  But how much is the spurious resistance during that time?  It is a resistance proportional to the time that has elapsed since the current was turned on.  So then it comes to a question of the length of time for which you want to work it.  What fraction of a second do you require your signal to be given in?  What is the rate of the vibrator of your electric bell?  Suppose you have settled that point, and that the short time during which the current is required to rise is called t; then the apparent resistance at time t after the current is turned on is given by the formula:

$$R_{t} = R \times e^{(R/L)t} + ( e^{(R/L)t} - 1 )$$

## TIME CONSTANTS OF ELECTROMAGNETS.

I may here refer to some determinations made by M. Vaschy,[1] respecting the coefficients of self-induction of the electromagnets of a number of pieces of telegraphic apparatus.  Of these I must only quote one result, which is very significant.  It relates to the electromagnet of a Morse receiver of the pattern habitually used on the French telegraph lines.

| | L, in quadrants. |
|---|---|
| Bobbins, separately, without iron cores. | 0.233 and 0.265 |
| Bobbins, separately, with iron cores. | 1.65 and 1.71 |
| Bobbins, with cores joined by yoke, coils in series | 6.37 |
| Bobbins, with armature resting on poles. | 10.68 |

[Footnote 1:  “Bulletin de la Societe Internationale des Electriciens,” 1886.]

It is interesting to note how the perfecting of the magnetic circuit increases the self-induction.

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Thanks to the kindness of Mr. Preece, I have been furnished with some most valuable information about the coefficients of self-induction, and the resistance of the standard pattern of relays, and other instruments which are used in the British postal telegraph service, from which data one is able to say exactly what the time constants of those instruments will be on a given circuit, and how long in their case the current will take to rise to any given fraction of its final value.  Here let me refer to a very capital paper by Mr. Preece in an old number of the "Journal of the Society of Telegraph Engineers," a paper "On Shunts," in which he treats this question, not as perfectly as it could now be treated with the fuller knowledge we have in 1890 about the coefficients of self-induction, but in a very useful and practical way.  He showed most completely that the more perfect the magnetic circuit is—though of course you are getting more magnetism from your current—the more is that current retarded.  Mr. Preece'e mode of experiment was extremely simple.  He observed the throw of the galvanometer when the circuit which contained the battery and the electromagnet was opened by a key which at the same moment connected the electromagnet wires to the galvanometer.  The throw of the galvanometer was assumed to represent the extra current which flowed out.  Fig. 56 represents a few of the results of Mr. Preece's paper.

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FIG. 56.—ELECTROMAGNETS OF RELAY, AND THEIR EFFECTS.

Take from an ordinary relay a coil, with its iron core, half the electromagnet, so to speak, without any yoke or armature. Connect it up as described, and observe the throw given to the galvanometer. The amount of throw obtained from the single coil was taken as unity, and all others were compared with it. If you join up two such coils as they are usually joined, in series, but without any iron yoke across the cores, the throw was 17. Putting the

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iron yoke across the cores, to constitute a horseshoe form, 496 was the throw; that is to say, the tendency of this electromagnet to retard the current was 496 times as great as that of the simple coil. But when an armature was put over the top, the effect ran up to 2,238. By the mere device of putting the coils in parallel, instead of in series, the 2,238 came down to 502, a little less than the quarter value which would have been expected. Lastly, when the armature and yoke were both of them split in the middle, as is done in fact in all the standard patterns of the British postal telegraph relays, the throw of the galvanometer was brought down from 502 to 26. Relays so constructed will work excessively rapidly. Mr. Preece states that with the old pattern of relay having so much self-induction as to give a galvanometer throw of 1,688, the speed of signaling was only from 50 to 60 words per minute, whereas, with the standard relays constructed on the new plan, the speed of signaling is from 400 to 450 words per minute. It is a very interesting and beautiful result to arrive at from the experimental study of these magnetic circuits.

SHORT CORES *versus* LONG CORES.

In considering the forms that are best for rapid action, it ought to be mentioned that the effects of hysteresis in retarding changes in the magnetization of iron cores are much more noticeable in the case of nearly closed magnetic circuits than in short pieces. Electromagnets with iron armatures in contact across their poles will retain, after the current has been cut off, a very large part of their magnetism, even if the cores be of the softest of iron. But so soon as the armature is wrenched off, the magnetism disappears. An air gap in a magnetic circuit always tends to hasten demagnetizing. A magnetic circuit composed of a long air path and a short iron path demagnetizes itself much more rapidly than one composed of a short air path and a long iron path. In long pieces of iron the mutual action of the various parts tends to keep in them any magnetization that they may possess; hence they are less readily demagnetized. In short pieces, where these mutual actions are feeble or almost absent, the magnetization is less stable, and disappears almost instantly on the cessation of the magnetizing force. Short bits and small spheres of iron have no magnetic memory. Hence the cause of the commonly received opinion among telegraph engineers that for rapid work electromagnets must have short cores. As we have seen, the only reason for employing long cores is to afford the requisite length for winding the wire which is necessary for carrying the needful circulation of current to force the magnetism across the air gaps. If, for the sake of rapidity of action, length has to be sacrificed, then the coils must be heaped up more thickly on the short cores. The electromagnets in American patterns of telegraphic apparatus usually have shorter cores, and a relatively greater thickness of winding upon them, than those of European patterns.

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## ELECTRIC ERYGMASCOPE.

The erygmascope is the name of an electric lighting apparatus designed for the examination of the strata of earth traversed by boring apparatus.

It consists of a very powerful incandescent lamp inclosed in a metallic cylinder.  One of the two semi-cylindrical sides constitutes the reflector, and the other, which is of thick glass, allows of the passage of the luminous rays, which thus illuminate with great brilliancy the strata of earth traversed by the instrument.  The base, which is inclined at an angle of 45 deg., is an elliptical mirror, and the top, of straight section, is open in order to permit the observer standing at the mouth of the well, and provided with a powerful spyglass, to see in the mirror the image of the earth.  The lamp is so mounted that its upwardly emitted rays are intercepted.

The whole apparatus is suspended from a long cable, formed of two conducting wires, which winds around a windlass with metallic journals which are electrically insulated. These journals communicate, through the intermedium of two friction springs, with the conductors on the one hand and, on the other, with the poles of an automatic and portable battery.

[Illustration:  THE TROUVE ERYGMASCOPE.]

This permits of lowering and raising the apparatus at will, without derangement, and without its being necessary to interrupt the light and the observation.—*Revue Industrielle.*

* * * * *

## A NEW ELECTRIC BALLISTIC TARGET.

The electrical target usually employed in determining velocities of projectiles consists of a wooden frame on which is strung a copper wire so as to make a continuous circuit arranged in parallel vertical lines about one inch or one and one half inches apart.

It frequently happens that a projectile will pass through this target without breaking the circuit, either by squeezing between the wires or because, when last repaired, the target was short-circuited unnoticed, so that the cutting of the wires did not break the circuit. The repair of this target takes considerable time.

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Plan.

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|===|  ==========       A                 A
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Besides these objections to this target, another and more serious one is the irregularity in the manner of breaking the circuit. It has been proved that times required for a flat headed and an ogival headed projectile to rupture the current are very different.

To remedy these defects a new and very ingenious target has been devised and used with great success at the United States Military Academy at West Point. The top of the target is a wooden strip, F, on the upper side of which are screwed strips of copper, A A, about 1/2 in. wide, and 1/8 in. thick. The connection between two adjoining strips is made by a copper cartridge, C, which is dropped in a hole in the frame bored to receive it. This cartridge is the one used in the Springfield rifle. Inside the cartridge is a spiral spring, S, which, acting on the bottom of the hole and the head of the cartridge, tends to make the latter spring up, and so break the circuit.

To the hook, H, which is attached to the cartridge, is suspended, by means of a string, the lead weight, W, thus drawing down the cartridge and making the circuit between A and A'. All the weights being suspended the current comes in through the post, P, passes along the copper strips and out of the corresponding post on the other end.

On firing the projectile cuts a string, and the spring at once causes the cartridge to spring up, thus breaking the circuit.

It is not possible for the projectile to squeeze between the strings and not break the current, for in so doing the cartridge is tipped slightly, which is sufficient, as it breaks the current on one side.

This target is used in connection with the Boulenge chronograph. Two targets are established at a known distance apart, say 50 ft., and the time required for the projectile to pass over this distance is determined by finding the difference in the time of cutting of the two targets, by finding the difference in the time of falling of the two rods, caused by the demagnetization of two electromagnets in the same circuit with the targets.

By means of a disjunctor both rods are dropped at the same time, the shorter one releasing a knife blade which makes a cut on the longer one. Now both rods are hung from the magnets again and the gun is fired.

The projectile passes through the first target, breaks the circuit, demagnetizes the magnet of the longer rod, and it begins to fall. On passing through the second target, the projectile causes the shorter rod to fall. This releases the knife blade, and a second cut is made. The time corresponding to the distance between these cuts is the time the longer rod was falling before the second rod began to fall or the time between the cutting of the two targets by the projectile.

The distance between the cuts is measured, and the time corresponding to it can easily be found. Then the velocity of the projectile is equal to 50/t.

To repair this target, strings are prepared in advance of suitable length and looped at both ends, so that by placing the hook of the cartridge in one loop and that of the weight in the other the repair is quickly made.

This target has been used on the West Point proving ground to determine velocities over distances of 100 ft. interval to distances of only 9 ft. interval, and has given most satisfactory results.

* * * * *

[Continued from SUPPLEMENT, No. 786, page 12566.]

## THE OUTLOOK FOR APPLIED ENTOMOLOGY.

[Footnote: Address of Dr. C.V. Riley at the annual meeting of the Association of Economic Entomologists, Champaign, Ills., November 11 to 14, 1890.]

LEGISLATION.

The amount of legislation in different countries that has of late years been deemed necessary or sufficiently important, in view of injurious insects, is a striking evidence of the increased attention paid to applied entomology; and while modern legislation of this kind has been, on the whole, far more intelligent than similar efforts in years gone by, many of the laws passed have nevertheless been unwise, futile, and impracticable, and even unnecessarily oppressive to other interests.

The chief danger here is the intervention of politics or political methods. Expert counsel should guide our legislators and the steps taken should be thorough in order to be effective. We have had of late years in Germany very good evidence of the excellent results flowing from thorough methods, and the recent legislation in Massachusetts against the gypsy moth (*Ocneria dispar*), which at one time threatened to become farcical, has, fortunately, proved more than usually successful; the commission appointed to deal with the subject having worked with energy and followed competent advice.

## PUBLICATION.

On the question of publication of the results of our labors it is perhaps premature to dwell at length. Each of the experiment stations is publishing its own bulletins and reports quite independently of the others, but after a uniform plan recommended by the association with which we meet here; and with but one exception that has come to my notice, another important recommendation of the same association—that these publications shall be void of all personal matter—has been kept in mind. The National Bureau of Experiment Stations at Washington is doing what it can with the means at command to further the general work by issuing the Experiment Station Record, devoted chiefly to digests of the State station bulletins. There is a serious question in my mind as to the utility of State digests by the national department of results already published extensively by the different States and distributed under government frank to all similar institutions and to whomsoever is interested enough to ask for them.

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Such digests may or may not be intelligently made, and, even under the most favorable circumstances, will hardly serve any other purpose than helping to the reference to the original articles, and this could undoubtedly be done more satisfactorily to the stations and to the people at large by general and classified indices to all the State documents, made as full as possible and issued at stated intervals. Only a small proportion of the bulletins have been so far noticed by digest in this record, with no particular rule, so far as I can see, in the selection. In point of fact, those will be most apt to be noticed whose authors can find time to themselves send or make for the purpose their own abstracts. This is, perhaps, inevitable under present arrangements. Complete and satisfactory digests of all, if intelligent and critical, imply a far greater force than is at present at Prof. Atwater's command.

Under these circumstances, it would seem wiser to devote all the energies of the bureau to digests of the similar literature of other countries, which would be of immense advantage to our people and to the different station workers. Judging from the recommendations and resolutions of the general association, this is the view very generally held, but except in chemistry and special industries like that of beet sugar, very little of that kind of work has yet been attempted.

What is true of the station publications in general is equally true of special publications. As entomologist of the department, I have been urged to bring together, at stated intervals, digests of the entomological publications of the different stations. Such digests to be of any value, however, should also be critical, and it were a thankless task for any one to be critic or censor even of that which needs correction or criticism. Moreover, to do this work intelligently would require increase of the divisional force, which at present is more advantageously employed, for, as already intimated, I should have great doubts of the utility of these digests.

I believe, however, that the division should strive for such increase of means as would justify the periodic publication, either independently or as a part of the department record, of general and classified indices to the entomological matter of the station bulletins, and should work more and more toward giving results from other parts of the world. This could, perhaps, best be done by titles of subject and of author so spaced and printed on stout paper that they could be cut and used in the ordinary card catalogue. The recipient could cut and systematically place the titles as fast as received.

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As to the character of the matter of the entomological bulletins, it will inevitably be influenced by the needs and demands of the people of the respective States, and while originality should be kept in mind, there must needs be in the earlier years of the work much restatement of what is already well known. That some results have been published of work which reflects no particular credit upon our calling is a mere incident of the new positions created. Yet we may expect marked improvement from year to year in this direction, and without being invidious, I would cite those of Prof. Gillette's on his spraying experiments and on the plum curculio and plum gouger, as models of what such bulletins should be.

Although the resolution offered at our last meeting by Prof. Cook, to the effect that purely descriptive matter should be excluded from the station bulletins, met with no favor, but was laid on the table, by the general association, I am in full sympathy with this position and am strongly of the opinion that in the ordinary bulletins such purely technical and descriptive matter should be reduced to the necessary minimum consistent with clearness of statement and accuracy, and that if it is desired, on the part of the station entomologists, to issue technical and descriptive papers, a separate series of bulletins were better instituted for this class of matter.

Finally, for results which it is desired to promptly get before the people, the agricultural press is at our disposal, and so far as the entomological work of the department of agriculture is concerned, the periodical bulletin, *Insect Life*, was established for this purpose. Its columns are open to all station workers, and I would here appeal to the members of the association to help make it, as far as possible, national, by sending brief notes and digests of their work as it progresses. Hitherto we have been unable to make as much effort in this direction as we desired, but in future it is our hope to make the bulletin, as far as possible, a national medium through which the results of work done in all parts of the country may quickly be put on record and distributed, not only to all parts of our own country, but to all parts of the world.

The rapid growth and development of the national department and the multiplication of its divisions have necessitated special modes of publication and rendered the annual report almost an anachronism so far as it pretends to be what it at one time was—a pretty complete report of the scientific and other work of the department. The attempts which I have made through the proper authorities to get Congress to order more pretentious monographic works in quarto volume similar to those issued by other departments of the government have not met with encouragement, and in this direction many of the stations will, let us hope, be able to do better.

## CO-OPERATION.

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Every other subject that might be considered on this occasion must be subordinate to the one great question of co-operation. With the large increase of actual workers in our favorite field, distributed all over the country, the necessity for some co-operation and co-ordination must be apparent to every one. Just how this should be brought about or in what direction we may work toward it, will be for this association in its deliberations to decide. Nor will I venture to anticipate the deliberations and conclusions of the special committee appointed to take the matter into consideration, beyond the statement that there are many directions in which we can adopt plans for mutual benefit. Take, for instance, the introduction and dissemination of parasites. How much greater will be the chance of success in any particular case if we have all the different station entomologists interested in some specific plan to be carried out in co-operation with the national department, which ought to have better facilities of introducing specimens to foreign countries or to different sections of our own country than any of the State stations.

Let us suppose that the fruit growers of one section of the country, comprising several States in area, need the benefit in their warfare against any particularly injurious insect of such natural enemy or enemies as are known to help the fruit growers of some other section. There will certainly be much greater chances of success in the carrying out of any scheme of introduction if all the workers in the one section may be called upon through some central or national body to help in the introduction and disposition of the desired material into the other section. Or, take the case of the boll worm investigation already alluded to. The chances of success would be much greater if the entomologists in all the States interested were to give some attention to such lepidopterous larvae as are found to be affected with contagious diseases and to follow out some specific plan of cultivating and transmitting them to the party or parties with

whom the actual trials are intrusted. The argument applies with still greater force to any international efforts. I need hardly multiply instances.
There is, it is true, nothing to prevent any individual station entomologist from requesting co-operation of the other stations, nor is there anything to prevent the national department from doing likewise; but in all organization results are more apt to flow from the power to direct rather than from mere liberty to request or to plead. The station entomologist may be engrossed in some line of research which he deems of more importance to the people of his State, and may resent being called upon to divert his energies; and with no central or national power to decide upon plans of co-operation for the common weal, we are left to voluntary methods, mutually devised, and it is here that this association can, it seems to me, most fully justify its organization. And this brings me to the question of

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## THE DEPARTMENT AND THE STATIONS.

Immediately connected with the question of co-operation is the relation of the National Department of Agriculture and the State experiment stations. The relation, instead of being vital and authoritative, is, in reality, a subordinate one. Many persons interested in the advancement of agriculture foresaw the advantage of having experiment stations attached to the State agricultural colleges founded under the Morrill act of 1862; but I think that in the minds of most persons the establishment of these stations implied some such connection with the national department as that outlined in an address on Agricultural Advancement in the United States, which I had the honor to deliver in 1879 before the National Agricultural Congress, at Rochester, and in which the following language was used:

"In the light of the past history of the German experimental stations and their work, or of that in our own State of Connecticut, the expediency of purchasing an experimental farm of large dimensions in the vicinity of Washington is very questionable. There can be no doubt, however, of the value of a good experimental station there that shall have its branches in every State of the Union. The results to flow from such stations will not depend upon the number of acres at command, and it will be far wiser and more economical for the commissioner to make each agricultural college that accepted the government endowment auxiliary to the national bureau, so that the experimental farm that is now, or should be, connected with each of these institutions might be at its service and under the general management of the superintendent of the main station. There is reason to believe that the directors of these colleges would cheerfully have them constituted as experimental stations under the direction of the department, and thus help to make it really national—the head of a vast system that should ramify through all parts of the land...."With the different State agricultural colleges, and the State agricultural societies, or boards, we have every advantage for building up a national bureau of agriculture worthy of the country and its vast productive interests, and on a thoroughly economical basis, such as that of Prussia, for instance."

In short, the view in mind was something in the nature of that which has since been adopted by our neighbors of the North, where there is a central or national station or farm at Ottawa and sub-stations or branch farms at Nappan, Nova Scotia, Brandon, Manitoba, Indian Head, N.W.T., and Agassiz, British Columbia, all under the able direction of Mr. William Saunders, one of our esteemed fellow workers. It was my privilege to be a good deal with Mr. Saunders when he was in Europe studying the experience of other countries in this matter, and the policy finally adopted in Canada as a result of his labors is an eminently wise one, preventing some of the difficulties and dangers which beset our plan, whether as between State and nation or college and station.

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Under the present laws and with the vast influence which the Association of Agricultural Colleges and Experiment Stations will wield, both in Congress and in the different States, there is great danger of transposition, in this agricultural body politic, of those parts which in the animal body are denominated head and tail, and the old saw to the effect that "the dog wags the tail because the tail cannot wag the dog," will find another application.
So far as the law goes, the national department, which should hold a truly national position toward State agricultural institutions depending on federal support, can do little except by suggestion, whether in the line of directing plans or in any way co-ordinating or controlling the work of the different stations throughout the country. The men who influenced and shaped the legislation which resulted in the Hatch bill were careful that the department's function should be to indicate, not to dictate; to advise and assist, not to govern or regulate. We have, therefore, to depend on such relationships and such plans of co-operation as will appear advantageous to all concerned, and these can best be brought about through such associations as are now in convention here.

Without such plans there is great danger of such waste of energy and means and duplication of results as will bring the work into popular disfavor and invite disintegration, for already there is a growing feeling that agricultural experiment is and will be subordinated to the ordinary college work in the disposition of the federal appropriations.

What is true of the national department as a whole in its connection with the State stations is true in a greater or less degree of the different divisions of the department in connection with the different specialists of the stations. With the multiplicity of workers in any given direction in the different States, the necessity for national work lessens.
A favorite scheme of mine in the past, for instance

(and one I am glad to say fully indorsed by Prof. Willits), was to endeavor to have a permanent agent located in every section of the country that was sufficiently distinctive in its agricultural resources and climate, or, as a yet further elaboration of the same plan, one in each of the more important agricultural States. The necessity for such State agents has been lessened, if not obviated, by the Hatch bill, and the subsequent modifications looking to permanent appropriations to the State stations or colleges, which give no central power at Washington. The question then arises, What function shall the national department perform? Its influence and field for usefulness have been lessened rather than augmented in the lines of actual investigation in very many directions. Many a State is already far better equipped both as to valuable surrounding land, laboratory and library facilities, more liberal salaries, and greater freedom from red tape, administrative routine, and restrictions as to expenditures, than

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we are at Washington; and, except as a directing agent and a useful servant, I cannot see where the future growth of the department's influence is to be outside of those federal functions which are executive. Just what that directing influence is to be is the question of the hour, not only in the broader but in the special sense. The same question, in a narrower sense, had arisen in the case of the few States which employed State entomologists. In the event, for instance, of an outbreak of some injurious insect, or in the event of any particular economic entomological question within the limits of the State having such an officer, the United States entomologist would naturally feel that any effort on his part would be unnecessary, or might even be looked upon as an interference. He would feel that there was always danger of mere duplication of observation or experiment, except where appealed to for aid or co-operation. This is, perhaps, true only of insects which are local or sectional, and is rather a narrow view of the matter, but it is one brought home from experience, and is certainly to be considered in our future plans.
The favor with which the museum work of the national division was viewed by you at the meeting last November and the amount of material sent on for determination would indicate that the building up of a grand national reference collection will be most useful to the station workers. But to do this satisfactorily we need your co-operation, and I appeal to all entomologists to aid in this effort by sending duplicates of their types to Washington, and thus more fully insuring against ultimate loss thereof.

## STATUS OF OUR SOCIETY.

This train of thought brings up the question of the status of our society with the station entomologists as represented by the committee of the general association. Those of us who had desired a national association for the various purposes for which such associations are formed, felt, I believe, if I may speak for them, that the creation of the different experimental stations rendered such an organization feasible. Your organization at Toronto and the constitution adopted and amended at the meeting at Washington all indicate that the chief object was the advancement of our chosen work and that the strength of the association would come from the experiment station entomologists. There was then no other organization of the kind, nor any intimation that such a one would be founded. Some of us therefore were surprised to learn from the circular sent out by Prof. Forbes, its chairman, that the committee appointed by the association of agricultural colleges and experiment stations, and through which we had hoped to communicate and co-operate with that association, was not in the proper sense a committee, but a section which has prepared (and in fact was required by the executive committee and the rules of the superior body to prepare) a programme

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of papers and discussions for the meeting to be held at the same time and place with our own. I cannot but feel that this is in some respects a misfortune, and it will devolve upon you to decide upon several questions of importance that will materially affect our future existence. That there is not room for two national organizations having the same objects in view and meeting at the same time and place goes, I think, without saying; and if the committee of the general association is to be anything more than a committee in the proper sense of the word, or if it is to assume with or without formal constitution the functions of our own association, then our own must necessarily be crippled, and to do any good at all must meet at a different time and a different place. A committee or section, or whatever it may be called, of the general association with which we meet, would preclude active membership of any but those who come within the constitution of that body. Our Canadian friends and many others who have identified themselves with applied entomology, and do not belong to any of our State or government institutions, would be debarred from active representation, however liberal the association may have been in inviting such to participate, without power to vote in its deliberations. Our own association has, or should have, no such limitations. Some of us who are entitled to membership in both bodies may feel indifferent as to the course finally decided upon, and that it will not make any difference whether we have an outside and independent organization, as that of the association of official chemists, or whether we do, as did the botanists and horticulturists, waive independence in favor of more direct connection with the general association, provided there is some way whereby the committees of the general association are given sufficient latitude and time to properly present their papers and deliberate; but there are others who feel more sensitive as to their action and are more immediately influenced by the feelings of the main body. I hope that whatever action be taken at this meeting, the general good and the promotion of economic entomology will be kept in mind and that no sectional or personal feeling will be allowed to influence our deliberations.

## SUGGESTION AND COMMENT.

You will, I know, pardon me if, before concluding these remarks, I venture to make a few comments which, though not altogether agreeable, are made in all sincerity and in the hope of doing good. The question as to how far purely technical and especially descriptive and monographic work should be done by the different stations or by the national department is one which I have already alluded to and upon which we shall probably hold differing opinions, and which will be settled according to the views of the authorities at the different stations. Individually, I have ever felt that one ostensibly engaged in applied entomology

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and paid by the State or national government to the end that he may benefit the agricultural community can be true to his trust only by largely overcoming the pleasure of entomological work having no practical bearing. I would, therefore, draw the line at descriptive work except where it is incidental to the economic work and for the purpose of giving accuracy to the popular and economic statements. This would make our work essentially biological, for all biologic investigation would be justified, not only because the life habits of any insect, once ascertained, throw light on those of species which are closely related to it, but because we can never know when a species at present harmless may subsequently prove harmful, and have to be classed among the species injurious to agriculture.

On the question of credit to their original sources of results already on record, it is hardly necessary for me to advise, because good sense and the consensus of opinion will in the end justify or condemn a writer according as he prove just and conscientious in this regard.

There is one principle that should guide every careful writer, *viz*., that in any publications whatever, where facts or opinions are put forth, it should always be made clear as to which are based upon the author's personal experience and which are compiled or stated upon the authority of others. We should have no patience with a very common tendency to set forth facts, even those relating to the most common and best known species, without the indications to which I have referred. The tendency belittles our calling and is generally misleading and confusing, especially for bibliographic work, and cannot be too strongly deprecated.

On this point there will hardly be any difference of opinion, but I will allude to another question of credit upon which there prevails a good deal of loose opinion and custom. It is the habit of using illustrations of other authors without any indication of their original source.

This is an equally vicious custom and one to be condemned, though I know that some have fallen into the habit, without appreciation of its evil effect. It is, in my judgment, almost as blameworthy as to use the language or the facts of another without citing the authority.

Every member of this association who has due appreciation of the time and labor and special knowledge required to produce a good and true illustration of the transformations and chief characteristics of an insect will appreciate this criticism. However pardonable in fugitive newspaper articles in respect of cuts which, from repeated use, have become common or which have no individuality, the habit inevitably gives a certain spurious character to more serious and official publications, for assumption of originality, whether intended or not, goes with uncredited matter whether of text or figure. Nor is mere acknowledgment of loan or purchase to the publisher, institution or individual who may own the block or stone what I refer to. But that acknowledgment to the author of the figure or the work in which it first appears which is part of conscientious writing, and often a valuable index as to the reliability of the figure.

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It were supererogation to point out to a body of this kind the value of the most careful and thorough work in connection with life histories and habits, often involving as it does much microscopic study of structure. The officers of our institutions who control the funds, and more or less fully our conduct, are apt to be somewhat impatient and inappreciative of the time given to anatomic work, and where it is given for the purpose of describing species and of synopsizing or monographing higher groups, without reference to agriculture, I am firmly of the belief that it diverts one from economic work, but where pursued for a definite economic purpose it cannot be too careful or too thorough and I know of no instances better calculated to appeal to and modify the views of those inclined to belittle such structural study than Phylloxera and Icerya. On the careful comparison of the European and American specimens of *Phylloxera vastatrix*, involving the most minute structures and details, depended originally those important economic questions which have resulted in legislation by many different nations and the regeneration of the affected vineyards of Europe, of our own Pacific coast, and of other parts of the world by the use of American resistant stocks. In the case of *Icerya purchasi* the possibilities of success in checking it by its natural enemies hung at one time upon a question of specific difference between it and the *Icerya sacchari* of Signoret—a question of minute structure which the descriptions left unsettled and which could only be settled by the most careful structural study and the comparison of the types, involving a trip to Europe.

## CONCLUSION.

I have thus touched, gentlemen, upon a few of the many subjects that crowd upon the mind for consideration on an occasion like this—a few gleanings from a field which is passing rich in promise and possibility. It is a field that some of us have cultivated for many years and yet have only scratched the surface, and if I have ventured to suggest or admonish, it is with the feeling that my own labors in this field are ere long about to end and that I may not have another occasion.

At no time in the history of the world has there, I trow, been gathered together such a body of devoted and capable workers in applied entomology. It marks an era in our calling and, looking back at the progress of the past fifteen years, we may well ponder the possibilities of the next fifteen. They will be fruitful of grand results in proportion as we persistently and combinedly pursue the yet unsolved problems and are not tempted to the immediate presentation of separate facts, which are so innumerable and so easily observed that their very wealth becomes an element of weakness. Epoch-making discoveries result only from this power of following up unswervingly any given problem, or any fixed ideal. The kerosene emulsion, the Cyclone

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nozzle, the history of *Phylloxera vastatrix*, of *Phorodon humuli*, of *Vedalia cardinalis*, are illustrations in point, and while we may not expect frequent results as striking or of as wide application as these, there is no end of important problems yet to be solved and from the solution of which we may look for similar beneficial results. Applied entomology is often considered a sordid pursuit, but it only becomes so when the object is sordid. When pursued with unselfish enthusiasm born of the love of investigation and the delight in benefiting our fellow men, it is inspiring, and there are few pursuits more deservedly so, considering the vast losses to our farmers from insect injury and the pressing need that the distressed husbandman has for every aid that can be given him. Our work is elevating in its sympathies for the struggles and suffering of others. Our standard should be high—the pursuit of knowledge for the advancement of agriculture. No official entomologist should lower it by sordid aims.

During the recent political campaign the farmer must have been sorely puzzled to know whether his interests needed protection or not. On the abstract question of tariff protection to his products we, as entomologists, may no more agree than do the politicians or than does the farmer himself. But ours is a case of protection from injurious insects, and upon that there can nowhere be division of opinion. It is our duty to see that he gets it with as little tax for the means as possible.

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## POTASH SALTS.

[Footnote: By John B. Smith, entomologist. Potash as an insecticide is not entirely new, but has never been brought out with the prominence I think it deserves.—*N.J. Ag. Col. Exp. St., Bulletin 75*.]

My attention was attracted to potash salts as an insecticide, by the casual remark of an intelligent farmer, that washing his young pear trees with a muriate of potash solution cleared them of scales. The value of this substance for insecticide purposes, should its powers be sufficient, struck me at once, and I began investigation. It was unluckily too late in the season for field experiments of the nature desired; but it is the uniform testimony of farmers who have used either the muriate or the kainit in the cornfields, that they have there no trouble with grubs or cut worms. Mr. E.B. Voorhees, the senior chemist of the station, assures me that on his father's farm the fields were badly infested, and replanting cornhills killed by grubs or wire worms was a recognized part of the programme. Since using the potash salts, however, they have had absolutely no trouble, and even their previously worst-infested fields show no further trace of injury. The same testimony comes from others, and I feel safe in recommending these salts, preferably kainit, to those who are troubled with cut worms or wire worms in corn.

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## EXPERIMENTS.

A lot of wire worms (*Iulus* sp.) brought in from potato hills were put into a tin can with about three inches of soil and some potato cuttings, and the soil was thoroughly moistened with kainit, one ounce to one pint of water. Next morning all the specimens were dead. A check lot in another can, moistened with water only, were healthy and lived for some days afterward.

A number of cabbage maggots placed on the soil impregnated with the solution died within twelve hours.

To test its actual killing power, used the solution, one ounce kainit to one pint water, to spray a rose bush badly infested with plant lice. Effect, all the lice dead ten hours later; the younger forms were dropping within an hour.

Sprayed several heads of wheat with the solution, and within three hours all the aphides infesting them were dead.

Some experiments on hairy caterpillars resulted unsatisfactorily, the hair serving as a perfect protection against the spray, even from the atomizer.

To test its effect on the foliage, sprayed some tender shoots of rose and grape leaves, blossoms, and clusters of young fruit. No bad effect observable 24 hours later. There was on some of the leaves a fine glaze of salt crystals, and a decided salt taste was manifest on all.

Muriate of potash of the same strength was tested as follows: Sprayed on some greenhouse camellias badly infested by mealy bugs, it killed nearly all within three hours, and six hours later not a living insect was found. The plants were entirely uninjured by the application.

Thoroughly sprayed some rose bushes badly infested with aphides, and carried off some of the worst branches. On these the lice were dead next morning; but on the bushes the effect was not so satisfactory, most of the winged forms and many mature wingless specimens were unaffected, while the terminal shoots and very young leaves were drooping as though frosted. All, however, recovered later.

The same experiment repeated on other, hardier roses, resulted similarly so far as the effect on the aphides was concerned, but there was no injury to the plant.

Used this same mixture on the caterpillars of *Orgyia leucostigma* with unsatisfactory effect, and with the same results used it on a number of other larvae. Used on the rose leaf roller, *Cacaecia rosaceana*, it was promptly effective.

Tested for injury to plants, it injured the foliage and flowers of wisteria, the younger leaves of maple and grape, and the finer kinds of roses.

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From these few experiments kainit seems preferable to the muriate, as acting more effectively on insects and not injuriously on plants. For general use on plants it is not to be recommended. It is otherwise on underground species, where the soil will be penetrated by the salts and where the moisture evaporates but slowly, and the salt has a longer and better chance to act. The best method of application would be a broadcasting in fertilizing quantity before or during a rain, so as to carry the material into the soil at once. In cornfields infested with grubs or wire worms, the application should be made before planting. Where it is to be used to reach root lice, it should be used when the injury is beginning. When strawberry beds are infested by the white grub, the application should be made when cultivating or before setting out.

The potash salts have a high value as fertilizers, and any application made will act as a stimulant as well as insecticide, thus enabling the plants to overcome the insect injury as well as destroying the insect.

In speaking on this subject in Salem county, I learned from farmers present that those using potash were not troubled with the corn root louse to any extent, and also that young peach trees have been successfully grown in old lice-infested orchards, where previously all died, by first treating the soil with kainit of potash.

* * * * *

A meteorological station has been built on Mont Blanc, at an elevation of 13,300 feet, under the direction of M. Vallot. It required six weeks to deliver the materials. The instruments are self-registering and are to be visited in summer every fifteen days if possible, the instruments being left to register between the visits. In the winter the observatory will be entirely inaccessible. This is the highest scientific station in Europe, but is 847 feet lower than the Pike's Peak station in Colorado.

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## THE EXPENSE MARGIN IN LIFE INSURANCE.

The principle of mutuality requires that the burden of expense in life insurance should be borne by all the members equally; but, even with the most careful adjustment, the allowance usually made is considerably in excess of what is needed in the regular companies doing business on the "level premium" plan.

It is customary in these companies to add to the net premium a percentage thereof to cover the expense account. This practice, though in harmony with the "commission system," is so clearly defective and so far removed from the spirit of life insurance mathematics, that it scarcely deserves even this passing notice.

It is generally understood that these corporations combine the functions of the savings bank and life insurance company, and it is only by separating the two in our minds as far as possible that we can obtain a clear conception of the laws that should govern the apportionment of the expenses among the great variety of policies.

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While it is a comparatively simple matter to state the amount of either the insurance or savings bank element in a single policy, it is by no means easy, as things go, to classify the company's actual expenses on this basis.

Fortunately, we can pretty accurately determine what these amounts should be in any particular case.

In the first place, there are institutions in our midst devoted solely to receiving and conserving small sums of money; doing, in fact, exactly what our insurance companies are undertaking to do with the reserve and contributions thereto. These savings banks are required by law to make returns to the State commissioner, from whose official report we can get a very good idea of the expense attendant on doing this business.

Confining ourselves to the city banks, where the conditions more nearly resemble those of the insurance companies, we find in thirty-eight combined institutions for saving in the State of Massachusetts a deposit in 1888 of $192,174,566, taken care of at an aggregate cost of $455,387, or about 24-100 of one per cent.

The same ratio carried out for all the savings banks in Massachusetts gives a trifle over 25-100 of one per cent.; we may, therefore, consider 1/4 of one per cent. as expressing pretty nearly the cost of receiving, paying out, and investing the savings of the people.

We must remember in this connection that in the popular estimation, the savings bank is an important factor in the public welfare, and in the towns and smaller cities there are often found public spirited men willing to give their services to encourage this mode of saving; but public sentiment has not yet given to life insurance the place which it is destined, sooner or later, to occupy by the side of the savings bank. Hence the services of able managers can only be obtained by a liberal outlay of the corporate funds. A satisfactory adjustment of the matter of expenses will, perhaps, do more than anything else to bring about this recognition on the part of the public.

In the case of the savings bank it is safe to say that for double the present outlay a liberal salary could be paid to all the officers. Following the analogy, we are led to infer that if this be the case in savings banks, then 1/2 of one per cent. of the reserve should be an ample allowance for the special labor required in the purely banking portion of the business.

In this we have the concurrence of the late Elizur Wright. In an essay on this subject he says:
“The expenses of the five largest savings banks in Boston, in 1869, did not exceed 4-10 of one per cent. on $28,000,000 deposited in them. They certainly had twice as many transactions, in proportion to the deposits, as any life insurance company could have with the same amount of reserve, so that 1/2 of one per cent. on the reserve seems to be ample for all working expenses save those of maintaining the agencies and collecting the premiums.”

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This need hardly be looked upon as an admission that it costs twice as much to care for the funds of a life insurance company as for those of a savings bank. A liberal expense allowance must be made at the outset, seeing that an error in this particular cannot easily be rectified after the policy is issued. The dividend, or, to speak more correctly, the annual return of surplus, will correct any overpayment on this account.

There is another expense which seems inevitable. This is the government tax on insurance companies, amounting in the aggregate to nearly 1/3 of one per cent. on the reserve.

When we consider that these institutions are intended to encourage thrift and to relieve the community from the care of numberless widows and orphans, it seems a clear violation of the principles of political economy to levy a tax on this business; still, whatever our opinion may be as to the justice or injustice of the imposition, the tax is maintained and must be provided for. Consequently a further allowance of 1/2 of one per cent. must be added to the net premium to cover the same, making a total of 1 per cent. of the reserve for banking expenses and taxes. Considering this point as settled for the time being, let us proceed to investigate the insurance expenses.

Here, again, we are fortunate in being able to refer to the official reports of a class of corporations doing nearly, if not quite pure insurance.

The assessment societies, outside of the fraternal and benevolent, reporting in 1889 to the insurance commissioner of Massachusetts, show outstanding risks amounting to $733,515,366. Losses to the amount of $7,270,238 were paid during the year at a cost for transacting the business of $2,403,053, which includes among other items “agency expenses and commissions,” which amount to about $1,203,000, or 17 per cent. of the cost value of the insurance actually done. It would seem as if an allowance of 20 per cent. would be a liberal one in the case of the regular companies, which surely have as good facilities for doing business as the assessment societies.

As far as insurance is concerned, there is less difference between regular and co-operative companies than is generally supposed. Regular companies assess each policy in advance for a year's insurance at a time, while co-operative societies furnish insurance only from one assessment to another. The difficulty in the way of collecting the assessment in the latter case would seem to be greater than in the former, owing to the more permanent nature of the regular insurance contract.

In compensating agents the assessment companies naturally pay in proportion to the insurance obtained, inasmuch as there is no other basis to go upon, but regular companies usually pay the agent a percentage of the premium *which includes a considerable trust fund* over and above the assessment for actual insurance. It is easily seen that by the last method the agent's compensation increases in proportion to the amount of savings bank business forced upon the company.

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To realize how far we are from anything like a scientific, not to say common sense basis for insurance expenses, we have but to examine the following list, which gives the ratios between the expenditures for general expenses in 1889, and those for the extension of the business. For every $100 used in a general way, the different companies spend for commissions and agency expenses: $37, $66, $67, $78, $91, $106, $110, $113, $120, $140, $157, $161, $173, $175, $186, $189, $200, $202, $222, $264, $311, $346.

It will doubtless be said that I am taking a very advanced position when I say that in the ideal life insurance scheme there is no place for the commission system. Solicitors will be a necessity only so long as they are in the field, but fifty years of life insurance has taught our community its true value and, thanks to the modern press, the institution it is no more likely to fall into desuetude than is Christianity or the moral law.

For the convenience of bringing the company to the individual, the latter should be willing to pay a fee. The man who renders another a service or puts his superior knowledge at another's disposal should look to the party benefited for his remuneration. Any compensation given for such service to a go-between by a mutual company is paid by all, and the question arises, Is the advantage to the company of sufficient importance to warrant the imposition of this tax upon all its members promiscuously? The following, from the Massachusetts Insurance Commissioner's Report for 1885, leaves no doubt as to the convictions of the writer on this important matter:
"The expensiveness of the life insurance policy is not because the level net premium is too high, for the premium is absolutely just, and the policy holder gets full value; but the complaint justly applies to the excessive expense charge. A person who wants insurance, life or fire or other, should be able to buy it at first cost without paying tribute of profits to middlemen. To that complexion the matter will finally be brought by the force of intelligent opinion, whatever resistance may be opposed by persons whose thrift lies in the perpetuation of the expensive system now in fashion."

It requires but a slight degree of prophetic vision to predict that in a very few years the companies in self defense will be obliged to change their method of compensating agents.

Several companies have already begun the reform by grading commissions; granting a percentage proportional to the amount of insurance likely to be done on the policy. Other companies have simply reduced the amount of the commission rate, thus virtually withdrawing from active competition.

This will, in a certain degree, explain the wide variation in the figures given above, where it is noticed that, in five companies out of twenty-two, the total agency expenditures amount to less than the general expenses, while in six cases the companies spend more than double as much on the former as on the latter. In either class we find representatives of the five largest companies in the country.

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On applying the foregoing ratios to the business of the existing companies we find that, calling the theoretical expenses $100, the actual expenditures for 1889 were as follows: $112.67, $118.34, $150.40, $194.48, $208.16, $208.53, $228.66, $235.89, $248.44, $250.79, $258.33, $258.57, $265.14, $267.19, $267.92, $274.47, $294.17, $314.96, $335.70, $377.94, $616.70.

In this discouraging exhibit there is one ray of comfort. The combined assets of the two companies heading the list amount to over $100,000,000. There is no question as to their financial standing, and both show a large increase in membership over the previous year. I may also say here that it is a difficult matter to get at the actual “cost of insurance” in the various companies. Many of them, on their own acknowledgment, do not compute the advance cost of carrying their “amount at risk,” and others, for reasons of their own, do not care to state the figures. In cases where the correct figures were not obtainable, I have assumed the cost to have been 1-1/3 per cent. of the mean amount at risk.

If we should, in our comparison, omit the actual agency expenses and commissions, the ratios would stand as follows:

Where I would allow $100 the companies actually used: $43.17, $55.90, $65.21, $77.21, $82.39, $88.34, $91.99. $91.98. $92.19, $94.65, $97.15. $99.55. $99.11. $102.86, $109.35, $125.05, $133.03, $141.92, $195.90, $207.06, $287.72.

As might be supposed, the first two ratios are those companies before alluded to. These companies might have doubled their advertising account and expended $300,000 between them on agents’ salaries, and still have kept within my allowance.

Admitting, for the present at least, the reasonableness of the proposed allowance for the expenses of the banking and insurance departments of the business, we have before us the problem how to equitably adjust the burden among the great variety of policies.

In the first place, *there should be no policy in the company that does not contribute its proportionate share of the expense allowance during every year of its life*. I make a special point of this, for at present the policies which have become paid up, either by the payment of a single premium at the outset or by the completion of a stipulated number of payments, contribute practically nothing to the expense account after the premium payments cease.

The following plan, I think, complies with all the requirements of the problem. By the proposed method every policy, at all stages of its existence, contributes its exact share to the expense fund, whatever its plan of payment may be.

Let us, as an illustration, examine the case of a ten year endowment policy, taken out at age 30, and consider it under three aspects, first, as paid for in advance by a single payment, second, as paid by five annual payments, and third, as paid for annually throughout the term. I have used this short term endowment policy simply for convenience, the rule applying equally to policies of longer term or to the ordinary life policy, which is, in fact, an endowment policy payable at death or age 100.[1]

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[Footnote 1: The expense allowance on a plain life policy for $1,000, taken at age 33, would be about $5.29; net premium (com. ex. 4 per cent.), $18.04; total office premium, $23.33; present rate $24.10.]

Taking the case of the single premium endowment policy for $1,000, we find that the following sums are required, each year to provide for the care of the reserve and to pay the government fees (1 per cent. of reserve):
1st year $6.9982 | 6th year $8.4136 2d " 7.2560 | 7th " 8.7381 3d " 7.5258 | 8th " 9.0781 4th " 7.8082 | 9th " 9.4346 5th " 8.1039 | 10th " 9.8086
The insurance expenses should be covered by the 20 per cent. allowance given below:
1st year $ .4422 | 6th year $ .2566 2d " .4100 | 7th " .2076 3d " .3762 | 8th " .1556 4th " .3402 | 9th " .0988 5th " .2996 | 10th " .0344
Consequently the total contribution required from this policy each year is:
1st year $7.4404 | 6th year $8.6702 2d " 7.6660 | 7th " 8.9457 3d " 7.9020 | 8th " 9.2337 4th " 8.1484 | 9th " 9.5334 5th " 8.4034 | 10th " 9.8430
The present value of all these contributions is found to be, at 4 per cent. interest, $71.6394; in other words, this sum paid at the outset, provides a fund from which we may deduct the current expenses of each year in advance, and by accumulating the balance at the assumed rate of interest from year to year, we shall have enough to pay the anticipated expenses, leaving nothing over.

In the above case the sums in hand at the beginning of the year are as follows:
1st year $71.3694 | 6th year $42.6981 2d " 66.7669 | 7th " 35.3890 3d " 61.4650 | 8th " 27.5009 4th " 55.7055 | 9th " 18.9979 5th " 49.4594 | 10th " 9.8430
We find a somewhat different condition existing during the first years of a 5-year endowment policy.
As there is more insurance and less banking, the requirements are as follows:

| 1 P. Ct. on Reserve. | 20 P. Ct. on Cost. | Total. | Initial Fund. |
|---|---|---|---|

| | | | | |
|---|---|---|---|---|
| 1st year | $1.5038 | $1.2572 | $2.7610 | $12.9769 |
| 2d " | 3.0406 | 1.0216 | 4.0622 | 23.6015 |
| 3d " | 4.6503 | .7852 | 5.4355 | 33.2979 |
| 4th " | 6.3367 | .5378 | 6.8745 | 41.9538 |
| 5th " | 8.1039 | .2996 | 8.4035 | 49.4594 |
| 6th " | 8.4136 | .2566 | 8.6702 | 42.6981 |
| 7th " | 8.7381 | .2076 | 8.9257 | 35.3890 |
| 8th " | 9.0781 | .1556 | 9.2337 | 27.5009 |
| 9th " | 9.4346 | .0988 | 9.5334 | 18.9979 |
| 10th " | 9.8086 | .0344 | 9.8430 | 9.8430 |

As the premium payments extend over only five years, the expense contributions must all be paid during that time and are most conveniently made by a uniform addition to the net premium.

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The present value of the amounts in column 3 is $60.0819, and the equivalent annuity for five years is $12.9769. This amount, received for five consecutive years, will put the company in funds to pay current expenses and leave a reserve of $42.6981 at the beginning of the sixth year, which, as we have seen in the analysis of the single-premium policy, is the sum required for future expenses on the paid up basis.

In like manner we find that the 10-year annuity equivalent to the present value of the annual contributions in the case of an annual-payment policy is $5.534, thus:

| | 1 P. Ct. on Reserve. | 20 P. Ct. on Cost. | Total. | Initial Fund. |
|---|---|---|---|---|
| 1st year | $.8234 | $1.3514 | $2.1748 | $ 5.5340 |
| 2d " | 1.6473 | 1.2478 | 2.8951 | 9.0275 |
| 3d " | 2.5096 | 1.1388 | 3.6484 | 11.9116 |
| 4th " | 3.4124 | 1.0210 | 4.4334 | 14.1277 |
| 5th " | 4.3572 | .8916 | 5.2488 | 15.6161 |
| 6th " | 5.3479 | .7534 | 6.1013 | 16.3160 |
| 7th " | 6.3853 | .5966 | 6.9819 | 16.1572 |
| 8th " | 7.4726 | .4270 | 7.8996 | 15.0763 |
| 9th " | 8.6127 | .2418 | 8.8545 | 12.9977 |
| 10th " | 9.8086 | .0344 | 9.8430 | 9.8430 |

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The present value of the ten yearly expense items given in the “total” column above is $46.6812, which is equal to a ten-year annuity of $5.534. The several premiums stand now as follows:

ENDOWMENT: $1,000, AGE 30, PAYABLE AT DEATH OR 40

Net Prem.[2] Margin.

Total.

| | | | |
|---|---|---|---|
| At single premium. | $687.228 | $71.6394 | $758.8674 |
| At five premiums. | 150.615 | 12.9769 | 163.5939 |
| At annual premiums. | 84.172 | 5.5340 | 89.7060 |

[Footnote 2: Thirty American offices. Discount from middle of year, Vx-1/2 or (M x 1.01961) / Dx.]

By the actuaries' rate we have, with the customary loading for expense:

Single premium: $721.66 (loaded, $34.36). Five premiums, $188.70 (loaded $37.78). Annual premium, $105.65 (loaded $21.11).

Admitting the correctness of the new method, we must conclude that the present single premium is not sufficiently loaded to cover its own expenses, while the annual payment policy pays more than its just share.
A prominent and thoroughly informed life insurance president says in this connection: "Many of the policies, particularly the short term endowments, are charged with too high a percentage of expenses to prove a good investment at maturity or profitable to the insured in case of surrender." This is not to be wondered at when the applicant for a 10-year endowment policy sees at a glance that he must pay, in the gross, more than is returned unless he should die in the interim, in which case a plain "life" or "term" policy would have answered the purpose. Under the new system of assessing expenses one form is as desirable as another, from the standpoint of the insured or the company.

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The new premium for the 10-year endowment policy, $89.71, commends itself at once to the applicant, who can easily see that his total outlay must fall short of the amount ultimately to be realized, of course, disregarding interest and probable dividends in both cases.

In discounting the future expense contributions I have not taken the chances of dying into account. Hence the expense reserve in any instance applies only to that individual case, and, in the event of death or surrender before the maturity of the policy, the amount of the expense fund not used would naturally revert to the insured.

The scheme of expense assessment outlined above will doubtless be pronounced impracticable by the majority of insurance men.

Such a far reaching reform is too much to hope for, at least in the immediate future.

No well informed life insurance expert will deny that there are opportunities for improvement in the business, but to graft new methods on old companies is a hopeless undertaking.

It is well, however, to have new methods well matured in advance of the public demand, and I feel convinced that the ideas here set forth are in the line of the reform which, before long, must be instituted by the companies if they would retain the confidence and patronage of the community.

Doubtless many insurance presidents could tell of suggestions which have impressed them favorably and which they would gladly have adopted were it not for the injustice done thereby to older members and the changes necessary to bring existing contracts into conformity with the new system. Similar objections may be urged against the ideas here advanced, and

I must confess I hardly see a way by which the present suggestions can be utilized by existing companies. We can only hope that sooner or later some of the new theories may be practically tested. Meanwhile the companies at present in the field are doing a great work for the good of humanity, even though their methods may be, in some particulars, more practical than scientific.

Winchester, Mass. FRANK J. WILLS.

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# THE FLOOD AT KARLSBAD.

During the flood which occurred in Germany and Bohemia, the last week of November, Karlsbad was especially unfortunate; it suffered such an inundation as had never before been known in the “Sprudelstadt.” On the evening of November 23, the Tepl was very much swollen by the rain, which had continued for several days, but it was supposed that there was no danger of a flood, as the bed of the river had been put in proper condition. During the forenoon of November 24, the water suddenly began to rise with such astonishing rapidity that within half an hour all the lower streets were like turbulent rivers and the Alte and Neue Wiese were transformed into a lake. The stores on the Alte Wiese were under water to the roofs, and the

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proprietors, who were trying to save their goods, were surprised by the water and had to take refuge in the trees. They were rescued by having ropes thrown to them, and during this work a catastrophe occurred which was a great misfortune to all classes of citizens. The beloved burgermeister of Karlsbad, Dr. Rudolf Knoll, who had just recovered from a severe illness, was, with others, directing the work from the balcony of one of the houses, when a rope by which a man was being drawn through the water broke, and the man was carried off by the waves. The fright and excitement of the scene gave the burgermeister a shock which caused his instant death, but the man who was in danger was brought safely out of the water.

The water was 9 ft. in Marienbaderstrasse, the Marktplatz, Muhlbadgasse, the Sprudelgasse, Kreuzgasse, Kaiserstrasse, and Egerstrasse, and flooded the quay, causing great destruction. All places of business were flooded, the doors and iron shutters were pushed in by the force of the water and the goods were carried away or ruined.

The house called “Zum Kaffeebaum” was undermined and part of it fell to the ground; the same fate was feared for other buildings. The Sophien and Curhaus bridges were carried away. Other bridges were greatly damaged, and the masonry along the banks of the river was partially destroyed. The Sprudelgasse was completely washed out, and the condition of the Muhlbadgasse was almost as bad. The fire department with its apparatus had great difficulty in saving the inhabitants and guests, as there were very few boats or pontoons at their command, and the soldiers (Pionniere) from Prague and the firemen from the neighboring towns did not arrive until evening. Fortunately the water began to fall in the night, and the next day it had gone down so that it left its terrible work visible. The Sprudel and the mineral springs were not injured, but, on the other hand, the water pipes of the bathing establishments and the gas pipes were completely destroyed.—*Illustrirte Zeitung*.

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## THEATRICAL WATER PLAYS.

In one of the plays at Hengler's Circus in London a water scene is introduced, for which purpose the main ring is flooded with water in a manner which is both striking and interesting.

[Illustration:  FLOODING A CIRCUS RING.]

The ring is entirely lined with stout macintosh sheeting, and into this, from two large conduits. 23,000 gallons of water are poured, the tank being filled to a depth of some 2 ft. in the remarkably short time of 35 seconds. A steamboat and other small craft are then launched and the adventures of the heroine then proceed. She falls overboard, we believe, but is saved after desperate and amusing struggles.  Our engravings, which are from the *Graphic*, illustrate the mode of filling the ring with water, and the steamboat launch.

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[Illustration:  A THEATRICAL STEAMBOAT.]

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## SCIENCE IN THE THEATER.

In the pretty little hall of the Boulevard des Italiens, at Paris, a striking exhibition of simulated hypnotism is given every evening.

This entertainment, which has met with much success, was devised by Mr. Melies, director of the establishment, which was founded many years ago by the celebrated prestidigitator whose popular name (Robert Houdin) it still bears.  This performance carries instruction with it, for it shows how easily the most surprising phenomena of the pathologic state can be imitated. To this effect, several exhibitions are given every evening.

Mr. Harmington, a convinced disciple of Mesmer, asks for a subject, and finds one in the hall.  A young artist named Marius presents himself.  Mr. Harmington makes him perform all sorts of extravagant acts, accompanied with a continuous round of pantomimes that are rendered the more striking by the supposed state of somnipathy of the subject.  At the moment at which Marius is finishing his most extraordinary exercises, a policeman suddenly breaks in upon the stage in order to execute the recent orders relative to hypnotism.  But he himself is subjugated by Mr. Harmington and thrown down by the vibrations of which the encephalus of this terrible magnetizer is the center.  When the curtain falls, the representative of authority is struggling against the catalepsy that is overcoming him.

All the phenomena of induced sleep are successively simulated with much naturalness by Mr. Jules David, who plays the part of Marius in this pleasing little performance.

At a certain moment, after skillfully simulated passes made by the magnetizer, Mr. David suddenly becomes as rigid as a stick of wood, and falls in pivoting on his heels (Fig. 1). Did not Mr. Harmington run to his assistance, he would inevitably crack his skull upon the floor, but the magnetizer stands just behind him in order to receive him in his arms. Then he lifts him, and places him upon two chairs just as he would do with a simple board. He places the head of the subject upon the seat of one of the chairs and the heels upon that of the other. Mr. David then remains in a state of perfect immobility. Not a muscle is seen to relax, and not a motion betrays the persistence of life in him. The simulation is perfect.

[Illustration: FIG. 1.—CATALEPTIC RIGIDITY.]

In order to complete the astonishment of the spectators, Mr. Harmington seats himself triumphantly upon the abdomen of the subject and slowly raises his feet and holds them suspended in the air to show that it is the subject only that supports him, without the need of any other point of support than the two chairs (Fig. 2).

[Illustration: FIG. 2.—EXPERIMENT ON THE SAME SUBJECT.]

Usually, there are plenty of persons ingenuous enough to think that Mr. David is actually in a cataleptic sleep, one of the characters of which is cadaveric rigidity.

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As Mr. David's neck is entirely bare, it is not possible to suppose that the simulator of catalepsy wears an iron corset concealed beneath his clothing. He has performed a feat of strength and skill rendered easy by the exercise that he has given to the muscles occupying the *colliciae* of his vertebral column. This part of the muscular system is greatly developed in the weakest and least hardy persons. In fact, in order that man may keep a vertical position and execute an infinite multitude of motions in which stability is involved, nature has had to give him a large number of different organs. The muscles of the back are arranged upon several superposed layers, the vertebral column is doubly recurved in order that it may have more strength, and, finally, rachidion nerves issue from each vertebra in order to regulate the contraction of each muscular fasciculus according to the requirements of equilibrium. The trick is so easy that we have seen youths belonging to the Ligue d'Education Physique immediately imitate Mr. David after seeing him operate but once.

For the sake of those who would like to perform it, we shall add that Mr. David takes care to bend his body in the form of an arch in such a way that the convexity shall be beneath. As Mr. Harmington never fails to place himself in the center of the line that joins Mr. David's head and heels, his weight is divided into two parts, that is to say, 88 pounds on each side of the point of support. The result is that the stress necessary is less than that of a strong man of the Halle lifting a bag of wheat to his shoulder or of an athlete supporting a human pyramid. The force of contraction of the muscular fibers brought into play in this experiment is much greater than is commonly believed. In his lectures on physiology, Milne-Edwards cites some facts that prove that it may exceed 600 pounds per square inch of section.

[Illustration: FIG. 3.—THE PERFORATE

ARM.]

The experiment on cadaveric rigidity is followed by others in insensibility. Mr. David, without wincing, allows a poignard to be thrust into his arm, which Mr. Harmington has previously “cataleptized” (Fig. 3). This trick is performed by means of a blade divided into two parts that are connected by a semicircle. This process is well known to prestidigitators, but it might be executed in a genuine manner. In fact, on replacing the poignard by one of the gold needles used by physicians for acupuncture, it would be possible to dispense with prestidigitation. Under such conditions it is possible to transpierce a person’s arm. The pain is supportable, and consists in the sensation of a prick produced in the passage of the needle through the skin. As for the muscular flesh, that is of itself perfectly insensible. The needle, upon the necessary antiseptic precautions being taken, may traverse the veins and arteries with impunity, provided that it is not allowed to remain long enough to bring about the formation of a clot of coagulated blood (Fig. 4).

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[Illustration: FIG. 4.—AN ARM TRANSPIERCED BY A NEEDLE.]

We think it of interest to add that it is necessary that the experiment be performed by a practitioner if one desires to demonstrate upon himself a very curious physiological fact that has been known from the remotest antiquity. It has been employed for several thousand years in Chinese medicine, for opening a passage for the bad spirits that produce diseases. For some years past a much more serious use has been made of it in European medicine for introducing electric currents into the interior of the organism. In this case the perimeter of the needle is insulated, and the electricity flows into the organism through the point. We have several times had these operations performed upon ourselves, and this permits us to assert that the above mentioned facts are absolutely true.—*La Nature*.

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## NEWER PHYSIOLOGY AND PATHOLOGY.

By Prof. SAMUEL BELL, M.D.

Physiology has for many decades been a science founded on experiment, and pathology has been rapidly pressing forward in the same direction. To read the accounts of how certain conclusions have been arrived at in the laboratory, by ingenious devices and by skillful manipulations, is as fascinating as any tale of adventure.

When the microscope began its work, how discouraging was the vastness and complexity of the discoveries which it brought to light; how many years has it been diligently used, and how uncertain are we still about many of its revelations! But what a happy conjecture of man, and as proper environment takes place we may reach better results! Let me give an illustration:

Some thirty years ago, Virchow began his studies and lectures upon cellular pathology. The enthusiasm which he awakened spread over the whole medical world. The wonderful attention to detail, the broad philosophy which signalized his observations, were alike remarkable. His class room was packed with students from every country, who thought it no hardship to struggle for a seat at eight o'clock in the morning.
With his blackboard behind him and specimens of pathology before him, and microscopes coursing upon railway tracks around the tables which filled the room, he was the embodiment of the teacher; his highest honor was as discoverer. The life and importance of the cell, both in health and disease, it has been his work to discover and to teach. The point of view from which he has classified tumors is founded on this basis, and remains the accepted method. The light which he cast upon the nature of inflammation has not yet been obscured, and while other phenomena appear, the multiplication of cells and nuclei and the formation of connective tissue in the process of inflammation will always call to mind his labors.

To one of Virchow's pupils, Prof. Recklinghausen, we chiefly owe our knowledge of the phenomena of diapedesis as a part of the inflammatory activity. How incredible it seems that masses of living matter can make their way through the walls of blood vessels which do not rupture and which have no visible apertures!

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Virchow fixed his attention upon the forms and activities of the cells, their multiplication and degradation, and how they build up tissues, both healthy and morbid.

To another matter with which, both literally and metaphorically, the air is filled, we must also make allusion. The existence of micro-organisms in countless numbers is no new fact, but the influence they may exert over living tissues has only lately become the subject of earnest attention. So long as they were not known to have any practical bearing upon human welfare, they interested almost nobody, but when, however, it was shown that putrefaction of meat is due to the agency of the *bacterium termo*, and the decomposition of albumen to the *bacillus subtilis*; when anthrax in cattle and sheep was found to depend on the *bacillus anthracis*, and that in human beings it caused malignant pustules; when suppuration of wounds was found to be associated with micrococci; and when it was announced that by a process of inoculation cattle could be protected against anthrax, and that by carbolic spray and other well known precautions the suppuration of wounds could be prevented—all the world lent its ears and investigation at once began.

Because labors in bacteriology promised to be fruitful in practical results, the workers speedily became innumerable, and we are accumulating a wondrous store of facts. How long now is the list of diseases in which germs make their appearance—in pneumonia, in endocarditis, in erysipelas, in pyaemia, in tuberculosis, and so on and so on. One of the most striking illustrations is the gonococcus of gonorrhoea, whose presence in and around gives to the pus cells their virulent properties, and when transferred to the eye works such lamentable mischief. Without their existence the inoculation of pus in the healthy eye is harmless; pus bearing the gonococci excites the most intense inflammation. Similar suppurative action in the cornea is often caused by infection of cocci. The proof of causation may be found in the fact that the most effective cure now practiced for such suppuration is to sterilize them by the actual cautery. Rosenbach says that he knows six distinct microbes which are capable of exciting suppuration in man. Their activity may be productive of a poison, or putrefactive alkaloid, which is absorbed.

There are at present two prominent theories in regard to the infections which produce disease. The first is based upon chemical processes, the second upon the multiplication of living organisms. The chemical theory maintains that after the infectious element has been received into the body it acts as a ferment, and gives rise to certain morbid processes, upon the principle of catalysis. The theory of organisms, or the germ theory, maintains that the infectious elements are living organisms, which, being received into the system, are reproduced indefinitely, and excite morbid processes which are characteristic of

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certain types of disease. This latter theory so readily explains many of the facts connected with the development and reproduction of infectious diseases, that it has been unqualifiedly adopted by a large number of investigators. The proofs of this theory had not, however, advanced beyond the demonstrations of the presence of certain forms of bacteria in the pathological changes of a very limited number of infectious diseases, until February, 1882, when Koch announced his discovery of the tubercle bacillus, since which time nearly every disease has its supposed microbe, and the race is, indeed, swift in which the would-be discoverers press forward with new germs for public favor.

The term bacteria or microbe refers to particles of matter, microscopic in size, which belong to the vegetable kingdom, where they are known as fungi. If we examine a drop of stagnant water under the microscope, amplifying say four hundred diameters, we see it loaded with minute bodies, some mere points, others slightly elongated into rods, all actively in motion and in various positions, a countless confusion. If evaporation now takes place, all is still. If we now apply moisture, the dried-up granules will show activity, as though they had not been disturbed.

All these different organisms have become familiar to us under the generic term bacteria, which is a very unfortunate application, as it really applies to only a single class of fungi. Cohn calls them schizomycetes, and makes the following classifications:
1. *Sphero-bacteria*, or microbes. 2. *Micro-bacteria*, or bacteria. 3. *Desmo-bacteria*, or bacilli. 4. *Spiroteria*, or spirillae.
The *spiro-bacteria*, or micrococci, are the simplest of the fungi, and appear as minute organisms of spherical form. They multiply by fission, a single coccus forming two, these two producing four, and so on. They present a variety of appearances under the microscope. From single isolated specimens (which under the highest magnifying power present

nothing beyond minute points) you will observe them in pairs, again in fours, or in clusters of hundreds (forming zooeglea) and still adhering together, forming chains. When a given specimen is about to divide, it is seen to elongate slightly, then a constriction is formed, which deepens until complete fission ensues.

Micrococci possess no visible structure. They consist of a minute droplet of protoplasm (mycroprotein) surrounded by a delicate cell membrane. Certain forms are embedded in a capsule (diameter 0.0008 to 0.0001 millimeter).

These little organisms, when observed in a fluid like blood, sputum, *etc*., are found to present very active movements, although provided with no organs of locomotion.

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This Brownian motion is possessed by almost every minute particle of matter, organic and inorganic, and is not due to any inherent power of the individual. They are almost omnipresent, abounding in the air, the earth, the water, are always found in millions where moist organic matter is undergoing decomposition, and are associated with the processes of fermentation—in fact, they are essential to it. The souring of milk succeeds the multiplication of these germs. Certain varieties are pigmented, and we observe colonies of chromogenic cocci multiplying upon slices of boiled potato, eggs, *etc*., presenting all the colors of the rainbow. All of these germs are not the cause of disease. Certain species, however (termed pathogenic), are always associated with certain diseased conditions.

The *bacteria-termo*—micro-bacteria—are slightly elongated, and inasmuch as they multiply by division, frequently appear coupled together, linked in pairs, and in chains. They are generally found in putrefying liquids, especially infusions of vegetable matter. They possess mobility to a remarkable degree. Observing a field of bacteria-termo under the microscope, they may be seen actively engaged in twining and twisting. A flagellum has been demonstrated as attached to one or both extremities. This is too minute to be generally resolved, even if it is a common appendage.

*Desmo-bacteria* (or bacilli) are rod-like organisms, occurring of various lengths and different thicknesses. In a slide of the bacillus of tuberculosis and anthrax, we notice at intervals dots which represent the spores from which, as the rods break up, future bacilli are developed.

Then we have *spiro-bacteria,* the spirilla and the spirochetae; the former having short open spirals, the latter long and closely wound spirals. The *spirillum, volutans* is often found in drinking water, and in common with some other specimens of this class is provided with flagellae, sometimes at both extremities, which furnish the means of rapid locomotion. The spiro-bacteria multiply by spores, although little is at present known of their life history. They frequently are attached together at their extremities, forming zigzag chains.

We have seen that bacteria differ greatly in appearance from the elongated dot of the bacterium proper, to the elongated rod or cylinder of the bacillus, and the long spirals of spiro-bacteria. It is unfortunate that they are not sufficiently constant in habit to always attach themselves to one or the other of these genera. The micrococcus has a habit of elongating at times until it is impossible to recognize him except as a bacterium; while bacilli, again, break up until their particles exactly resemble micrococci.

Bacteria cannot exist without water; certain forms require oxygen, while others thrive equally well without it; some thrive in solution of simple salts, while others require albuminoid material.

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Bacteriology, with its relation to the science of medicine, is of importance to every investigating physician; it covers our knowledge of the relation of these minute organisms to the aetiology of disease. What has been gained as to practical application in the treatment of disease? This question is not infrequently asked in a sneering manner. We can, in reply, say that the results are not all in the future. It is encouraging that results have been attained which have had a very important practical bearing, and that these complaints come generally from individuals least acquainted with scientific investigations in bacteriology.

In the study of the relation of a given bacterium to a certain disease, it becomes necessary to attend carefully to three different operations: First, the organism supposed to cause the disease must be found and isolated. Second, it must be cultivated through several generations in order that absolute purity may be secured. Lastly, the germ must be again introduced into a healthy living being. If the preceding steps be carried out, and the original disease be communicated by inoculation, and the germs be again found in the diseased body, we have no alternative; we must conclude that we have ascertained the cause of the disease. The importance of being familiar with the aetiology of the disease before we can expect to combat it with any well-grounded hope of success is evident.

If the sputum of a phthisical patient be submitted to the skilled microscopist, he is nearly always able to demonstrate bacilli, but this goes for very little. Because bacilli are found in phthisis, it is no more certain that they are the cause of phthisis than it is certain that cheese mites are the cause of cheese. Well, suppose we were to inject sputum from a phthisical person into the lower animal and tuberculosis follows, and then announce to the profession that we have demonstrated the relation of the cause and effect between bacilli and phthisis?  Why we would start such an uproar of objections as would speedily convince us that there was much work yet in the domain of bacteriology.

The scientific investigators would say you have injected with the sputum into the blood of your unfortunate patient, pus, morphological elements, and perhaps half a dozen other forms of bacteria, any one of which is just as likely to produce the disease as the bacillus you have selected.

The first important step is, first isolate your bacillus. If I were to take a glass plate, one side of which is coated with a thick solution of peptonized gelatin, and allow the water to collect, the gelatinous matter will become solid.  If now, with a wire dipped in some tuberculous matter, I draw a line along the gelatin, I have deposited at intervals along this line, specimens of tubercle bacilli.  If this plate be now kept at a proper temperature, after a few days, wherever the bacilli have been caught, a grayish spot will appear, which, easily seen with the naked eye, gradually spreads and becomes larger. These spots are colonies containing thousands of bacilli. Let us return to our gelatin plate.

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We find a spot which answers to the description of a colony of tubercle bacilli. We now take a minute particle from this colony on a wire and convey it to the surface of some hardened blood serum in a test tube. We plug the tube so that no air germs may drop in, and place it in an incubator at the proper temperature. After several days, if no contamination be present, a colony of bacilli will appear around the spot where we sowed the spores. Let us repeat the process.

Take a particle from this colony, and transfer it to another tube. This is our second culture. This must be repeated until we are satisfied that we have secured a *pure* culture. If this be carried to the twenty-fifth generation, we may be assured that there remains no pus, no ptomaines, nothing but the desired bacilli.

It is a proper material now for inoculation, and if we inoculate some of the lower animals, for instance the monkey, we produce a disease identical with phthisis pulmpnalis. Bacteria also afford peculiar chemical reactions. For example, nitric acid will discharge all the color from all bacilli artificially dyed with anilin, except those of tubercle and anthrax. One species is stained readily with a dye that leaves another unaltered. Thus we are enabled in the laboratory to determine whether the bacilli found in sputum, for example, are from tubercle or are the bacteria of decomposition.

From what I have said of the tubercle bacillus, it would seem thoroughly demonstrated that it is the cause of tubercle in these animals. But we must walk cautiously here. These are not human beings, who know that like results would follow their inoculation. The animals used by Koch are animals very subject to tubercle.

We must, from the very nature of our environment, be constantly inhaling these germs as we pass through the wards of our hospitals; yes, they are floating in the air of our streets and dwellings. It becomes necessary then for us to inquire: If bacteria cause disease, in what manner do they produce it? The healthy organism is always beset with a multitude of non-pathogenic bacteria. They occupy the natural cavities, especially the alimentary canal. They feed on the substances lying in their neighborhood, whether brought into the body or secreted by the tissues. In so doing they set up chemical changes in their substances. Where the organs are acting normally these fungi work no mischief. The products of decomposition thus set up are harmless, or are conveyed out of the body before they begin to be active.

If bacteria develop to an inordinate degree, if the contents of organs are not frequently discharged, fermentative processes may be set up, which result in disease. Bacteria must always multiply and exist at the expense of the body which they infest, and the more weakened the vital forces become, the more favorable is the soil for their development.

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Septicaemia is caused by the absorption of the products of putrefaction, induced before bacteria can multiply inside or outside the body. Bacteria must find a congenial soil. The so-called cholera bacillus must gain access to the intestinal tract before it finds conditions suitable to colonization. It does not seem to multiply in the stomach or in the blood, but once injected into the duodenum develops with astonishing rapidity, and the delicate epithelial cells of the villi become swollen, soften and break down, exposing the mucosa.

It has been shown that *bouillon* in which Loeffler's diphtheria bacillus has grown, and which has been passed through unglazed porcelain filters, shows the presence of a poison which is capable of producing the same results upon inoculation as the pure culture of the bacillus itself. Zarniko, working upon the same organism, obtained a number of positive results that led him to declare this bacillus is the cause of epidemic diphtheria, in spite of many assertions to the contrary. Chantmesse and Widal record the results of their work as to what will most easily and effectively destroy the bacillus of diphtheria.

The only three substances that actually checked and destroyed its vitality were phenic acid (5 per cent.), camphor (20 per cent.), olive oil (25 per cent.), in combination. For the last I substitute glycerine, because this allows the mixture to penetrate farther into the mucous membrane than oil, the latter favoring a tendency to pass over the surface. This mixture when heated separates into two layers, the upper one viscid and forming a sort of "glycerol," the lower clear. The latter will completely sterilize a thread dipped in a pure culture of the diphtheria bacillus. Corrosive sublimate was not examined because in strong enough doses it would be dangerous and in weaker ones it would be useless.

The facts obtained in regards to the streptococcus of erysipelas are reported as follows: That both chemical and experimental evidence teach the extreme ease of a renewed attack of the disease; that it is possible to kill guinea pigs by an intoxication when they are immune to an inoculation of the culture in ordinary quantities. And this latter fact should warn experimenters trying to obtain immunity in man by the inoculation of non-pathogenic bacteria, because the same results may be reached.

A new theory in regard to fevers and the relation of micro-organisms is suggested by Roussy, *viz*.: That it is a fermentation produced by a diastase or soluble ferment found in all micro-organisms and cells, and which they use in attacking and transforming matter, either inside their substance or without it.

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The resemblance of the malaria parasite to that of recurrent fever is noted in the work of Sacharoff. He states that there exists in the blood of those suffering from recurrent fever a haematozoon, which is most prominent after the fever has begun to fall, when it is of enormous proportions, twenty or more diameters of a red blood corpuscle, although smaller ones may still be found. The parasite consists of a delicate amoeboid body containing a multitude of dark, round, uniform, sharply outlined, movable granules. Besides these, the protoplasm contains a generally grayish homogeneous nucleus as large as one or two red blood corpuscles. The protoplasm sends out pseudopodia (with granules), which sometimes separate and appear as small delicate pieces of protoplasm. They vary in size, and are often swallowed by the red blood corpuscles in which they grow, and finally develop into the above mentioned amoeboid bodies.

Prof. J. Lewis Smith has made a great many autopsies of children dead from cholera infantum, and almost invariably found the stomach and liver in a comparatively healthy condition. Ganghen, who has given this subject considerable study, denies the existence of any specific germ in the summer diarrhea of infants, but claims to have found three different germs in the intestines of children suffering from cholera infantum, each producing a chemical poison which is capable of producing vomiting, purging, and even death. A great variety of germs are found in drinking water, and no doubt countless numbers are taken into the digestive tract, and the principal reason why pathological conditions do not occur more frequently is on account of the germicidal qualities of the gastric juice.

The comma bacillus of Koch, and the typhoid fever germ of Eberth, are especially destroyed in normal gastric juice. When the germs are very numerous, they run the gauntlet of the stomach (as the gastric juice is secreted only during digestion); and once in the alkaline intestinal canal they are capable of setting up disease, other conditions contributing—ill health, deranged digestion, *etc*.

Mittnam has made a study of bacteria beneath the nails, and reports, after examining persons following different occupations, having found numerous varieties of micro-organisms; which are interesting from a scientific standpoint relative to the importance of thoroughly cleansing the hands before undertaking any surgical procedure. He found, out of twenty-five experiments, 78 varieties of bacteria, of which 36 were classed as micrococci, 21 diplococci, 18 rods, 3 sarcinae, and 1 yeast. Cooks, barbers, waiters, *etc*., were examined.

The blood, defibrinated and freshly drawn, has marked germicidal action; for bacteria its action is decidedly deadly, even hours after it has been drawn from the body. Especially were anti-germic qualities noticed upon pathogenic bacteria. Buchner put the bacilli of anthrax in a quantity of blood, and in two hours the number was reduced from 4,800 to 56, and in three hours only 3 living bacteria remained. Other bacteria were experimented upon in blood with similar results, but the destruction of the organism from putrefaction was much less marked, and on some varieties the blood had little or no action.

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It is not the object of these remarks to even give a *resume* of the *status praesens* of bacteriology, but simply to stimulate thought in that direction. The claims of some of the ultra-bacteriologists may never be realized, but enough has been accomplished to revolutionize the treatment of certain diseases, and the observing student will do well to keep his eye on the microbe, as it seems from the latest investigations that its star is in the ascendant. And who can prognosticate but that in the next decade an entire revolution in the aetiology and treatment of many diseases may take place?

Detroit, Mich.

* * * * *

## THE COMPOSITION OF KOCH'S LYMPH.

WHAT PROFESSOR KOCH SAYS IT IS, AND WHAT IT CAN DO.

(By Cable to the *Medical Record*.)

BERLIN, January 15, 1891.

The curiosity to know the composition of the famous lymph has been gratified by the publication to-day of an article by Professor Koch on the subject. In the following, as will be seen, he reaffirms his original convictions and acknowledges the valuable assistance he has received from those who have used his fluid, and thus helped him in the accumulation of experience.

Professor Koch says: Two months ago I published the results of my experiments with the new remedy for tuberculosis, since which time many physicians who received the preparation have been enabled to become acquainted with its properties through their own experiments. So far as I have been able to review the statements published and the communications received by letter, my predictions have been fully and completely confirmed. The general consensus of opinion is that the remedy has a specific action upon tubercular tissues, and is, therefore, applicable as a very delicate and sure reagent for discovering latent and diagnosing doubtful tuberculous processes. Regarding the curative effects of the remedy, most reports agree that, despite the comparatively short duration of its application, many patients have shown more or less pronounced improvement. It has been affirmed that in not a few cases even a cure has been established. Standing quite by itself is the assertion that the remedy may not only be dangerous in cases which have advanced too far—a fact which may forthwith be conceded—but also that it actually promotes the tuberculous process, being therefore injurious.

During the past six weeks I myself have had opportunity to bring together further experiences touching the curative effects and diagnostic application of the remedy in the cases of about one hundred and fifty sufferers from tuberculosis of the most varied types in this city and in the Moabit Hospital.

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I can only say that everything I have latterly seen accords with my previous observations. There has been nothing to modify in what I before reported. As long as it was only a question of proving the accuracy of my indications, it was needless for any one to know what the remedy contained or whence it was derived. On the contrary, subsequent testing would necessarily be more unbiased, the less people knew of the remedy itself. Now, after sufficient confirmatory testing, the importance of the remedy is proved, my next task is to extend my study of the remedy beyond the field where it has hitherto been applied, and if possible to apply the principle underlying the discovery to other diseases.

This task naturally demands a full knowledge of the remedy. I therefore consider that the time has arrived when the requisite indications in this direction shall be made. This is done in what follows.

Before going into the remedy itself, I deem it necessary for the better understanding of its mode of operation to state briefly the way by which I arrived at the discovery. If a healthy guinea pig be inoculated with the pure cultivation of German Kultur of tubercle bacilli, the wound caused by the inoculation mostly closes over with a sticky matter, and appears in its early days to heal. Only after ten to fourteen days a hard nodule presents itself, which, soon breaking, forms an ulcerating sore, which continues until the animal dies. Quite a different condition of things occurs when a guinea pig already suffering from tuberculosis is inoculated. An animal successfully inoculated from four to six weeks before is best adapted for this purpose. In such an animal the small indentation assumes the same sticky covering at the beginning, but no nodules form. On the contrary, on the day following, or the second day after the inoculation, the place where the lymph is injected shows a strange change. It becomes hard and assumes a darker coloring, which is not confined to the inoculation spot, but spreads to the neighboring parts until it attains a diameter of from 0.05 to 1 cm.

In a few days it becomes more and more manifest that the skin thus changed is necrotic, finally falling off, leaving a flat ulceration which usually heals rapidly and permanently without any involvement of the adjacent lymphatic glands. Thus the injected tubercular bacilli quite differently affect the skin of a healthy guinea pig from one affected with tuberculosis. This effect is not exclusively produced with living tubercular bacilli, but is also observed with the dead bacilli, the result being the same whether, as I discovered by experiments at the outset, the bacilli are killed by a somewhat prolonged application of a low temperature or boiling heat or by means of certain chemicals. This peculiar fact I followed up in all directions, and this further result was obtained—that killed pure cultivations of tubercular bacilli, after rinsing in water, might be injected in great quantities under healthy guinea pig's skin without anything occurring beyond local suppuration. Such injections belong to the simplest and surest means of producing suppurations free from living bacteria.

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Tuberculous guinea pigs, on the other hand, are killed by the injection of very small quantities of such diluted cultivations. In fact, within six to forty-eight hours, according to the strength of the dose, an injection which is not sufficient to produce the death of the animal may cause extended necrosis to the skin in the vicinity of the place of injection. If the dilution is still further diluted until it is scarcely visibly clouded, the animals inoculated remain alive and a noticeable improvement in their condition soon supervenes. If the injections are continued at intervals of from one to two days, the ulcerating inoculation wound becomes smaller and finally scars over, which otherwise it never does; the size of the swollen lymphatic glands is reduced, the body becomes better nourished, and the morbid process ceases, unless it has gone too far, in which case the animal perishes from exhaustion. By this means the basis of a curative process against tuberculosis was established.

Against the practical application of such dilutions of dead tubercle bacilli there presented itself the fact that the tubercle bacilli are not absorbed at the inoculation points, nor do they disappear in another way, but for a long time remain unchanged, and engender greater or smaller suppurative foci. Anything, therefore, intended to exercise a healing effect on the tuberculous process must be a soluble substance which would be liberated to a certain extent by the fluids of the body floating around the tubercle bacilli, and be transferred in a fairly rapid manner to the juices of the body; while the substance producing suppuration apparently remains behind in the tubercular bacilli, or dissolves but very slowly. The only important point was, therefore, to induce outside the body the process going on inside, if possible, and to extract from the tubercular bacilli alone the curative substance. This demanded time and toil, until I finally succeeded, with the aid of a forty to fifty per cent. solution of glycerine, in obtaining an effective substance from the tubercular bacilli. With the fluid so obtained I made further experiments on animals, and finally on human beings. These fluids were given to other physicians to enable them to repeat the experiments.

The remedy which is used in the new treatment consists of a glycerine extract, derived from the pure cultivation of tubercle bacilli. Into the simple extract there naturally passes from the tubercular bacilli, besides the effective substance, all the other matter soluble in fifty per cent. glycerine.

Consequently, it contains a certain quantity of mineral salts, coloring substances, and other unknown extractive matters. Some of these substances can be removed from it tolerably easily. The effective substance is insoluble in absolute alcohol. It can be precipitated by it, though not, indeed, in a pure condition, but still combined with the other extractive matter. It is likewise insoluble in alcohol. The coloring

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matter may also be removed, rendering it possible to obtain from the extract a colorless, dry substance containing the effective principle in a much more concentrated form than the original glycerine solution. For application in practice this purification of the glycerine extract offers no advantage, because the substances so eliminated are unessential for the human organism. The process of purification would make the cost of the remedy unnecessarily high.

Regarding the constitution of the more effective substances, only surmises may for the present be expressed. It appears to me to be derivative from albuminous bodies, having a close affinity to them. It does not belong to the group of so-called toxalbumins, because it bears high temperatures, and in the dialyzer goes easily and quickly through the membrane. The proportion of the substance in the extract to all appearance is very small. It is estimated at fractions of one per cent., which, if correct, we should have to do with a matter whose effects upon organisms attacked with tuberculosis go far beyond what is known to us of the strongest drugs.

Regarding the manner in which the specific action of the remedy on tuberculous tissue is to be represented, various hypotheses may naturally be put forward. Without wishing to affirm that my view affords the best explanation, I represent the process myself in the following manner:

The tubercle bacilli produced when growing in living tissues, the same as in artificial cultivations, contain substances which variously and notably unfavorably influence living elements in their vicinity. Among these is a substance which in a certain degree of concentration kills or so alters living protoplasm that it passes into a condition that Weigert describes as coagulation necrosis. In tissue thus become necrotic the bacillus finds such unfavorable conditions of nourishment that it can grow no more and sometimes dies.

This explains the remarkable phenomenon that in organs newly attacked with tuberculosis, for instance in guinea pigs' spleen and liver, which then are covered with gray nodules, numbers of bacilli are found, whereas they are rare or wholly absent when the enormously enlarged spleen consists almost entirely of whitish substance in a condition of coagulation necrosis, such as is often found in cases of natural death in tuberculous guinea pigs. The single bacillus cannot, therefore, induce necrosis at a great distance, for as soon as necrosis attains a certain extension the growth of the bacillus subsides, and therewith the production of the necrotizing substance. A kind of reciprocal compensation thus occurs, causing the vegetation of isolated bacilli to remain so extraordinarily restricted, as, for instance, in lupus and scrofulous glands.

In such cases the necrosis generally extends only to a part of the cells, which then, with further growth, assume the peculiar form of riesen zelle, or giant cells. Thus, in this interpretation, follow first the explanation Weigert gives of the production of giant cells.

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If now one increased artificially in the vicinity of the bacillus the amount of necrotizing substance in the tissue, the necrosis would spread a greater distance. The conditions of nourishment for the bacillus would thereby become more unfavorable than usual.

In the first place the tissue which had become necrotic over a large extent would decay and detach itself, and where such were possible would carry off the inclosed bacilli and eject them outwardly, so far disturbing their vegetation that they would much more speedily be killed than under ordinary circumstances.

It is just in looking at such changes that the effect of the remedy appears to consist. It contains a certain quantity of necrotizing substance, a correspondingly large dose of which injures certain tissue elements even in a healthy person, and perhaps the white blood corpuscles or adjacent cells, thereby producing fever and a complication of symptoms, whereas with tuberculous patients a much smaller quantity suffices to induce at certain places, namely, where tubercle bacilli are vegetating and have already impregnated the adjacent region with the same necrotizing matter, more or less extensive necrosis of the cells, with the phenomena in the whole organism which result from and are connected with it.

For the present, at least, it is impossible to explain the specific influence which the remedy, in accurately defined doses, exercises upon tuberculous tissue, and the possibility of increasing the doses with such remarkable rapidity, and the remedial effects which have unquestionably been produced under not too favorable circumstances.

Of the consumptive patients whom he described as temporarily cured, two have been returned to the Moabit Hospital for further observation.

No bacilli have appeared in their sputum for the past three months, and their phthisical symptoms have gradually and completely disappeared.

* * * * *

## CAN WE SEPARATE ANIMALS FROM PLANTS?

By ANDREW WILSON.

One of the plainest points connected with the study of living things is the power we apparently possess of separating animals from plants. So self-evident appears this power that the popular notion scoffs at the idea of science modestly disclaiming its ability to separate the one group of living beings from the other. Is there any danger of confusing a bird with the tree amid the foliage of which it builds its nest, or of mistaking a cow for the grass it eats? These queries are, of course, answerable in one way only. Unfortunately (for the querists), however, they do not include or comprehend the whole difficulty. They merely assert, what is perfectly true, that we are able, without trouble, to mark off the higher animals from the higher plants. What science inquires is, whether we are able to separate *all* animals from *all* plants, and to fix a definite

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boundary line, so as to say that all the organisms on the one side of the line are assuredly animals, while all the others on the opposite side of the line may be declared to be truly plants. It is exactly this task which science declares to be among the impossibilities of knowledge. Away down in the depths of existence and among the groundlings of life the identity of living things becomes of a nature which is worse than confusing, and which renders it a futile task to attempt to separate the two worlds of life. The hopelessness of the task, indeed, has struck some observers so forcibly that they have proposed to constitute a third kingdom—the *Regnum Protisticum*—between the animal and the plant worlds, for the reception of the host of doubtful organisms. This third kingdom would resemble the casual ward of a workhouse, in that it would receive the waifs and strays of life which could not find a refuge anywhere else.

A very slight incursion into biological fields may serve to show forth the difficulties of naturalists when the task of separating animals from plants is mooted for discussion. To begin with, if we suppose our popular disbeliever to assert that animals and plants are always to be distinguished by shape and form, it is easy enough to show him that here, as elsewhere, appearances are deceptive. What are we to say of a sponge, or a sea anemone, of corals, of zoophytes growing rooted from oyster shells, of sea squirts, and of sea mats? These, each and all of them, are true animals, but they are so plant-like that, as a matter of fact, they are often mistaken by seaside visitors for plants. This last remark holds especially true of the zoophytes and the sea mats. Then, on the other hand, we can point to hundreds of lower plants, from the yeast plant onward, which show none of the ordinary features of plant life at all. They possess neither roots, stems, branches, leaves, nor flowers, so that on this first count of the indictment the naturalist gains the day.

Power of movement, to which the popular doubter is certain to appeal, is an equally baseless ground of separation. For all the animals I have above named are rooted and fixed, while many true plants of lower grade are never rooted at all. The yeast plant, the *Algae* that swarm in our ponds, and the diatoms that crowd the waters, exemplify plants that are as free as animals; and many of them, besides, in their young state especially (e.g., the seaweeds), swim about freely in the water as if they were roving animalcules. On the second count, also, science gains the day; power of motion is no legitimate ground at all for distinguishing one living being as an animal, while absence of movement is similarly no reason for assuming that the fixed organism must of necessity be a plant. Then comes the microscopic evidence. What can this wonder glass do in the way of drawing boundary lines betwixt the living worlds? The reply again is disappointing to the doubter; for the

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microscope teaches us that the tissues of animals and plants are built upon kindred lines. We meet with cells and fibers in both. The cell is in each case the primitive expression of the whole organism. Beyond cells and fibers we see the wonderful living substance, *protoplasm*, which is alike to our senses in the two kingdoms, although, indeed, differing much here and there in the results of its work. On purely microscopic grounds, we cannot separate animals from plants. There is no justification for rigidly assuming that this is a plant or that an animal on account of anything the microscope can disclose. A still more important point in connection with this protoplasm question consists in the fact that as we go backward to the beginnings of life, both in animals and plants, we seem to approach nearer and nearer to an identity of substance which baffles the microscope with all its powers of discernment. Every animal and every plant begins existence as a mere speck of this living jelly. The germ of each is a protoplasm particle, which, whatever traces of structure it may exhibit, is practically unrecognizable as being definitely animal or plant in respect of its nature. Later on, as we know, the egg or germ shows traces of structure in the case of the higher animals and plants; while even lowly forms of life exhibit more or less characteristic phases when they reach their adult stage. But, of life's beginnings, the microscope is as futile as a kind scientific touchstone for distinguishing animals from plants as is power of movement, or shape, or form.

A fourth point of appeal in the matter is found within the domain of the chemist. Chemistry, with its subtile powers of analysis, with its many-sided possibilities of discovering the composition of things, and with its ability to analyze for us even the light of the far distant stars, only complicates the difficulties of the biologist. For, while of old it was assumed that a particular element, nitrogen, was peculiar to animals, and that carbon was an element peculiar to plants, we now know that both elements are found in animals, just as both occur in plants. The chemistry of living things, moreover, when it did grow to become a staple part of science, revealed other and greater anomalies than these. It showed that certain substances which were supposed to be peculiar to plants, and to be made and manufactured by them alone, were also found in animals. Chlorophyl is the green coloring matter of plants, and is, of course, a typical product of the vegetable world; yet it is made by such animals as the hydra of the brooks and ponds, and by many animalcules and some worms. Starch is surely a typical plant product, yet it is undoubtedly manufactured, or at least stored up, by animals—a work illustrated by the liver of man himself, which occasionally produces sugar out of its starch.

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Again, there is a substance called *cellulose*, found well nigh universally in plants. Of this substance, which is akin to starch, the walls or envelopes of the cells of plant tissues are composed. Yet we find those curious animals, the sea squirts, found on rocks and stones at low-water mark, manufacturing cellulose to form part and parcel of the outer covering of their sac-like bodies. Here it is as if the animal, like a dishonest manufacturer, had infringed the patent rights of the plant. On the fourth count, then—that of chemical composition—the verdict is that nothing that chemistry can teach us may serve definitely, clearly, and exactly to set a boundary line or to erect a partition wall between the two worlds of life. There yet remains for us to consider a fifth head—that of the food.

In the matter of the feeding of the two great living worlds we might perchance light upon some adequate grounds for making up the one kingdom from the other. What the consideration of form, movement, chemical composition, and microscopic structure could not effect for us in this way, it might be supposed the investigation of the diet of animals and plants would render clear. Our hopes of distinguishing the one group from the other by reference to the food on which animals and plants subsist are, however, dashed to the ground; and the diet question leaves us, therefore, when it has been discussed, in the same quandary as before.

Nevertheless, it is an interesting story, this of the nutrition of animals and plants. A large amount of scientific information is to be gleaned from such a study, which may very well be commenced by our having regard to the matters on which a *green* plant feeds. I emphasize the word "green," because it so happens that when a plant has no chlorophyl (as green color is named in the plant world) its feeding is of diverse kind to that which a green plant exhibits. The mushroom or other fungus may be taken as an illustration of a plant which represents the non-green race, while every common plant, from a bit of grass to an oak tree, exemplifies the green-bearing order of the vegetable tribes.

Suppose we were to invite a green plant to dinner, the *menu* would have to be very differently arranged from that which would satisfy a human or other animal guest. The soup would be represented for the plant’s delectation by water, the fish by minerals, the joint by carbonic acid gas, and the dessert by ammonia. On these four items a green plant feeds, out of them it builds up its living frame. Note that its diet is of inorganic or non-living matter. It derives its sustenance from soil and air, yet out of these lifeless matters the green plant elaborates and manufactures its living matter, or protoplasm. It is a more wonderful organism than the animal, for while the latter can only make new protoplasm when living matter is included in its food supply, the green plant, by the exercise of its vital chemistry, can transform that which is not living into that which is life-possessing.

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The green plant in other words, raises non-living into living matter, while the animal can only transform living matters into its like. This is why the plant is called a constructive organism, while the animal is, contrariwise, named a destructive one. The result of the plant's existence is to build up, that of the animal's life is to break down its substance, as the result of its work, into non-living matter. The animal's body is, in fact, breaking down into the very things on which the green plant feeds. We ourselves are perpetually dissipating our substance in our acts of life and work into the carbonic acid, water, ammonia, and minerals on which plants feed. We "die daily" in as true a sense as that in which the apostle used the term. And out of the debris of the animal frame the green plant builds up leaf and flower, stein and branch, and all the other tokens of its beauty and its life.

If, then, an animal can only live upon living matter—that is to say on the bodies of other animals or of plants—with water, minerals and oxygen gas from the air thrown in to boot, we might be tempted to hold that in such distinctive ways and works we had at last found a means of separating animals from plants. Unfortunately, this view may be legitimately disputed and rendered null and void, on two grounds. First of all, the mushrooms and their friends and neighbors, all true plants, do not feed as do the green tribes. And secondly, many of the green plants themselves can be shown to have taken very kindly to an animal mode of diet.

A mushroom, thus, because it has no green color, lives upon water, oxygen, minerals, and organic matter. You can only grow mushrooms where there is plenty of animal matter in a state of decay, and as for the oxygen, they habitually inhale that gas as if they were animals. Non-green plants thus want a most characteristic action of their green neighbors.

For the latter in daylight take in the carbonic acid gas, which is composed of carbon and oxygen. Under the combined influence of the green color and the light, they split up the gas into its two elements, retaining the carbon for food and allowing the oxygen to escape to the atmosphere. Alas! however, in the dark our green plant becomes essentially like an animal as regards its gas food, for then it is an absorber of oxygen, while it gives off carbonic acid. If to take in carbonic acid and to give out oxygen be held to be a feature characteristic of a plant, it is one, as has been well said, which disappears with the daylight in green plants, and which is not witnessed at all in plants that have no green color.

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So far, we have seen that not even the food of plants and animals can separate the one kingdom of life from the other. The mushroom bars the way and the green plant’s curious behavior by night and by day respectively, in the matter of its gas food, once more assimilates animal life and plant life in a remarkable manner. Still more interesting is the fact, already noticed, that even among the green tribes there are to be found many and various lapses from the stated rules of their feeding. Thus what are we to say of the parasitic mistletoe, which, while it has grown leaves of its own, and can, therefore, obtain so much carbon food from the air on its own account, nevertheless drinks up the sap of the oak or apple which forms its host, and thus illustrates the spectacle of a green plant feeding like an animal, on living matter? Or, what may we think of such plants as the sundew, the Venus’ fly trap, the pitcher plants, the side saddle plants, the butterworts and bladderworts, and others of their kind, which not only capture insects, often by ingenious and complex lures, but also digest the animal food thus captured? A sundew thus spreads out its lure in the shape of its leaf studded with sensitive tentacles, each capped by a glistening drop of gummy secretion. Entangled in this secretion, the fly is further fixed to the leaf by the tentacles which bend over it and inclose it in their fold. Then is poured out upon the insect’s body a digestive acid fluid, and the substance of the dissolved and digested animal is duly absorbed by the plant. So also the Venus’ fly trap captures insects by means of its leaf, which closes upon the prey when certain sensitive hairs have given the signal that the animal has been trapped. Within the leaf the insect is duly digested as before, and its substance applied to the nutrition of the plant. Such plants, moreover, cannot flourish perfectly unless duly supplied with their animal food. Such illustrations of exceptions to the rule of green plant feeding simply have the effect of abolishing the distinctions which the diet question might be supposed to raise between animals and plants. We may return to the sundews and other insect catchers; meanwhile, I have said enough to show that to the question, “Can we separate animals from plants?” a very decided negative reply must be given. Life everywhere exhibits too many points of contact to admit of any boundary line being drawn between the two great groups which make up the sum total of organic existence.—*Illustrated London News*.

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## THE RECOVERY OF SILVER AND GOLD FROM PLATING AND GILDING SOLUTIONS.

In view of the rapid development and extension of the methods of electro-plating with silver and gold, and of the large amount of spent liquors containing silver or gold thus produced, it has long been desirable to find methods by which these metals can be recovered from the spent liquors. The processes hitherto adopted generally necessitate the tedious and unpleasant evaporation of the cyanide liquors, or else involve a series of chemical operations which are somewhat difficult to carry out, so that actually the used-up baths are sold to some firm which undertakes this recovery as a particular branch of its business.

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A process invented by Stockmuir and Fleischmann, and worked out by them in the chemical laboratory of the Bavarian Industrial Museum, is, however, exceedingly simple, and is employed in many establishments.

In order to remove silver from a potassium cyanide silver solution, it is only necessary to allow a clean piece of plate zinc to remain in the liquid for two days; even better results are obtained by the use of iron conjointly with the zinc. In the first case, the silver often adheres firmly to the zinc, while in the second it always separates out as a powder. It is then only necessary to wash the precipitated powder, which usually contains copper (since spent silver solutions always contain copper), dry it, and then dissolve it in hot concentrated sulphuric acid, water being added, and the dissolved silver precipitated by strips of copper. The silver thus obtained is perfectly pure. If the amount of copper present is only small, it can usually be removed by fusing the precipitated powder with a little niter and borax.

In this way a spent silver bath was found to contain per liter

| | |
|---|---|
| 1st experiment | 1.5706 grms. |
| 2d " | 1.5694 " |
| ------ | |
| Mean | 1.5700 " |

The presence of silver could not be qualitatively ascertained in the residual liquor.

Although sheet zinc, or zinc and iron sheets, serve so well for the precipitation of silver, they cannot be employed for the recovery of gold. The latter separates out in such a case very incompletely and as a firmly adhering lustrous film in the zinc. On the other hand, finely divided zinc, the so-called zinc dust, is an excellent substance to employ for precipitating gold quantitatively and in the form of powder from spent cyanide liquors. When zinc dust is added to a spent gold bath and the liquid periodically stirred or shaken, all the gold is precipitated in two or three days. The amount of zinc to be added naturally depends on the quantity of gold present. Freshly prepared gold baths for gilding in the cold contain on the average 3.5 grms. gold per liter, while those used for the hot process contain 10.75 grms. To precipitate all the gold in the original bath, 1.74 grms. or 0.37-0.5 grms. zinc dust would be necessary, and, of course, a much smaller quantity would be sufficient for the spent liquors. Since the precipitation takes place more rapidly when an excess of zinc dust is present, it is generally advisable to add 1/4 or at the most 1/2 kilo, of zinc dust to every 100 liters of solution.

The precipitated gold, which contains zinc dust and usually silver and copper, is washed, freed from zinc by hydrochloric acid, and then from silver and copper by nitric acid and thus obtained pure.

A spent bath treated in this way gave the following amounts of gold per liter:

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| | |
|---|---|
| 1st experiment | 0.2626 |
| 2d " | 0.2634 |
| Mean | 0.2630 grms. |

The presence of gold in the residual cyanide solution could not be qualitatively detected. The potassium cyanide of the solutions obtained by this process should be converted into ferrocyanide by heating with ferrous sulphate and milk of lime, since this substance is not poisonous and can therefore be got rid of without danger. It would, however, be more economical and, considering the large amount of cyanide present, more profitable to work it up into Prussian blue.

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