ing the telescope, after unscrewing Band v. The scale, E, is fixed to a support, and its weight is balanced by a counterpoise, P.

The apparatus is very light and convenient. The use of it is not limited to the angular motions of a mirror around a vertical axis. In order to adapt it for observations of angular motions around a horizontal axis, it suffices to remove the scale and its support, and to fix the latter upon the horizontal cylinder situated a little above the bearings of the telescope.

Fig. 5 represents a telescope constructed by Dr. Edelmann, of Munich, and designed for use in laboratories of physics. The apparatus is mounted upon a large wooden tripod, thus giving it great mobility.

A parallelogram, *h, i, k, l*, serves for slight lateral movements, the screw, *a*, for slight angular ones in a horizontal plane, and the screw, *e*, for angular motions in a vertical plane.—*La Lumière Electrique*.

[ELECTRICAL REVIEW.]

REIS' TELEPHONES.

In your issue of to-day you reproduce an extremely interesting article from the SCIENTIFIC AMERICAN, in which the following passage occurs respecting the early instruments described in 1861 by Reis, and in 1876 by Bell:

"In both instruments is latent the capacity for transmitting speech, but by neither Bell nor Reis was this power distinctly claimed. . . . Both inventors appear on the field with devices adapted for the same end, but neither with perfect distinctness claims this function as a part of the invention."

Permit me to point out that these assertions are not justified, so far as Reis is concerned, by the facts. He most unmistakably stated that his instrument was intended to transmit speech. The transmission of music by it was merely an after-thought.

The very first sentence of the very first paper written by Reis on the telephone, in 1861, is as follows:

"The surprising results in the domain of telephony have often already suggested the question whether it may not also be possible to communicate the very tones of speech [die Tonsprache selbst] to a distance." There is no ambiguity here as to the intention. Then he states his first prime difficulty. "How could a single instrument reproduce at once the total actions of all the organs operated in human speech?" His solution of that cardinal problem was the construction of his first transmitter in the form of a model of the human ear, having at the back of the tympanum a metal microphone with spring contacts to imitate the human ear-bones. He then said, after describing the apparatus, "I give to my instrument the name 'Telephon.' . . . Until now it has not been possible to reproduce the tones of human speech with a distinctness to satisfy everybody. The consonants are for the most part tolerably distinctly reproduced, but the vowels not yet in an equal degree." These sentences are quoted from the paper contributed by Reis in December, 1861, to the annual report (Jahresbericht) of the Physical Society of Frankfurt-on-the-Main, of which Reis, Faraday, Brewster, and Wheatstone were members. They establish beyond all cavil the following points:

1. Reis intended to transmit "the very tones of human speech."
2. The "tones of human speech" included "consonants" and "vowels," in other words, what some people prefer to call articulate speech as distinguished from mere musical sounds.
3. The apparatus was more successful with consonants than with vowels, and though its articulation did not satisfy everybody, it is clear that it satisfied some. (This is not surprising, since the vowels are the least important part of speech, and may be altered without affecting intelligibility, as is the case in the differences between different dialects.)

All this was in 1861. In 1863 Reis published printed directions for using the improved instruments, and in these directions told the person listening to the receiver how to signal instructions to the speaker at the transmitting end: "One beat = sing; two beats = speak." These instructions were circulated in Germany, France, and England from 1863 to 1867.

I cannot understand how, in the face of these things, it is that even those who are desirous of giving Reis his just due so often fail in their endeavor. Nothing could be more appreciative, more fair, than the intentions of the writer of the article in the SCIENTIFIC AMERICAN. Yet he says Reis never claimed "this function [i. e., transmitting speech] as a part of his invention." Never claimed this function! Why, that was the starting point of the invention, its pith, its marrow.

That was the dream which for nine years had wrought in his brain before he had made a single experiment. That was the essence of the invention which he brought to the birth, and gave freely and without reservation to the scientific and commercial world, the invention which he lived for, and wrapped himself up in, until his all too early death. I say nothing here of the abundant evidence of contemporary documents and contemporary witnesses. I leave Reis' own written and published words to speak for themselves. *Littera scripta manet*. Not even the ability of the smartest Yankee lawyer can destroy the evidence to be found on the shelves of every great European library. Mr. Dickerson will find that out some day, perhaps when all the world (except himself) has ceased to ignore the very stubborn fact that Philipp Reis did actually write these unmistakable sentences in 1861, about his invention for transmitting speech.

I have spoken of Reis and what he described as his invention in 1861 and 1863. Let me extract from Bell's specification (U. S.) of 1876 the passages which relate—as we are now told—to the transmission of speech:

"There are many other uses to which these instruments may be put, such as . . . the telegraphic transmission of noises or sounds of any kind."
"One of the ways in which the armature may be set in vibration has been stated to be by wind. Another mode is . . . by the human voice or by means of a musical instrument."

"I claim" . . . "The method of and apparatus for transmitting vocal or other sounds telegraphically." The language of Bell's specification is more general than that of Reis, but it is at the same time far less significant or descriptive. Plenty of matter about "musical notes," and "dot and dash," and "signals"

of Morse code, and "pitch," and "amplitude," and "multiple telegraphy," but never a word about "speech," or "consonants," or "vowels," no instructions how to signal to your friend when to "speak." No; the nearest approach to anything of that sort is: "noises or sounds of any kind," "vocal or other sounds." But, weak though this may be, the use of such general and feeble terms does not prove that Bell did not contemplate at that time the possible application of his "new and useful improvements in telegraphy" to the transmission of speech. It merely proves that Bell, on the 14th of February, 1876, was still far behind the point attained by Reis on the 1st of December, 1861. He was only fourteen years three months and thirteen days too late; and even then not abreast of that which Reis had accomplished without either attorneys or capitalists to back up his efforts.

SILVANUS P. THOMPSON.

September 5, 1885.

CAUSE OF THE ELECTRIFICATION OF CLOUDS.

WHEN the potential of an insulating medium increases as we recede from a conductor in its midst, this latter is negatively electrified; and this is the case with the air in fine weather, so that Peltier's hypothesis that the ground is normally covered with a layer of negative electricity can be shown to be a fact. Even if some portions of the earth's surface are positively electrified, as is indeed the case, they are so limited in extent as to leave the balance of the earth's charge of negative sign.

A cloud arising near the earth's surface may be in electrical communication with it, and so become negatively electrified, and on the dispersion of this cloud by evaporation the surrounding air would take up this negative charge. A cloud is a sufficiently good conductor to allow of the potential at all points in it being practically the same. If, then, a cloud is formed in an originally clear sky, this cloud will be electrified positively below, and negatively above. If, now, by the action of the wind the two portions of the cloud become separated, we have two clouds charged with opposite signs. Not only can this electrification by induction be shown to take place, but it may be further shown that the quantity thus produced is sufficient to account for electrical storms; for as the clouds rise their potential increases enormously, probably by one C.G.S. unit for each meter of elevation, so that the mere displacement of clouds may give rise to differences of potential corresponding to several thousand units.

The author, therefore, reduces the electrification of clouds to the existence of a negative charge of electricity on the earth's surface, which he considers to have always existed since its formation.—*H. Pellat, in Journal de Physique*.

CONSTRUCTION OF COPIES OF THE LEGAL OHM.

AFTER the Conference of 1884, at which the legal ohm was settled, the author was requested by the Minister of Posts and Telegraphs to make copies of the unit of resistance. This he was able to do with great accuracy, having placed at his disposal all the resources of the International Committee of Weights and Measures.

To calculate the resistance of a column of mercury, it is necessary to know (1st) the internal shape of the glass tube, (2d) its capacity, (3d) its length; we can then use the formula,

$$R = r - \left\{ \left(\frac{1}{C + x_1 - x_a} + \frac{1}{C + x_2 - x_b} + \dots \right) C^2 + \frac{(n-m)^2}{n-m+x_n-x_m} \right\},$$

where *r* is the resistance of a column of unit length and unit section; *l* is length at zero of one division of the graduated tube; *v* the capacity of one division; *a, b, c*, etc., the several divisions; *C* the constant value *b-a-c-b*, etc.; *x_a, x_b* the corrections to be made for the individual divisions.

In order to introduce the mercurial column into the circuit, the connections were made by means of large flasks full of mercury, into which the tube entered through lateral tubulures. This method of connection introduces an additional resistance which must be taken into account. It is obtained by adding to the length of the tube a quantity equal to 0.82 time its diameter. The coefficient 0.82, which had been deduced by Lord Rayleigh from theoretical considerations, has been experimentally verified by Mascart and De Nerville.

The author made use of four tubes, two of green glass, two of flint glass. They were all about 1.2 m. long, with a sectional area of 1 square mm., and were divided into millimeters for a length of 1.05 m. They had been prepared fourteen months before they were actually used.

The calibration of the bore of the tubes was most carefully carried out in the same manner as is done in the case of delicate thermometers, viz., by accurately measuring the length of a thread of mercury at various points of the tube.

To determine the capacity of one division of the tube, it was filled with a column of mercury 950 to 1,000 mm. long, which was in like manner measured in twenty different positions, allowance being made for errors in the size of bore as determined by the preceding operation, and also for the meniscus. The length having been thus most carefully ascertained, the mercury was emptied into a small glass vessel, and weighed with all the nicety possible with the very delicate instruments of the Committee of Weights and Measures. Adopting the value 13.5956 for the specific gravity of mercury, it was then easy to calculate the volume comprised between any two divisions of the tube. It was found that in the measurements of lengths it was sufficient to divide the tube into twenty-one sections, as this gave the true resistance within 1/10000. Starting from the zero division, the exact length of each tube was determined which should have a resistance of one legal ohm, and by successive trials the tube was reduced to this length. The whole of the measurements having been made at the prevailing temperature of the room, the coefficient of expansion of the glass was obtained, and the corrections made to bring the values to zero.

The geometrical determinations having been completed, the resistances of the four tubes were compared

electrically among themselves. The bridge was made up of three German silver wire resistances, each nearly one ohm in value, the fourth branch containing the tube under test. The apparatus was completed by a stretched wire with a sliding contact. A displacement of 1 mm. on the bridge wire caused a deflection of about 300 mm. on the scale of the galvanometer. The value of the bridge wire had been accurately determined beforehand, and it was found that 1 mm. was equivalent to 0.0000907 ohm. The comparisons were made between each pair of tubes by two observers, the author and Mr. De Nerville.

Admitting that the mean value 0.999994 ohm as deduced from the geometrical measurements is exact, the resistance of the four tubes comes out—

	Benoit.	De Nerville.
I.	1.000016	1.000020
II.	0.999996	0.999996
III.	0.999962	0.999959
IV.	1.000002	1.000004

More easily portable copies of these four standards have also been made in glass tubes coiled into a spiral; the ends being reduced until the resistance of the spiral was made accurately equal to one legal ohm.

With reference to the point of the purity of the mercury employed, Mr. Benoit remarks that after several tests it was found that mercury purified repeatedly by the action of hot nitric acid, then dried under a layer of concentrated sulphuric acid, and finally passed over caustic potash, was always directly comparable, from whatever source it might have been derived.—*Benoit, in Journal de Physique*.

THE LAWS OF FRICTION.

By MARCEL DEPREZ.*

THE purely mechanical part of one of the receiving machines designed for the experiment upon the electric transmission of power, which will take place before long between Creil and Paris, having been completed a few days ago, I was obliged to go and receive it, that is to say, to ascertain whether it was capable of revolving at the velocity of six hundred revolutions per minute for a whole day without any heating resulting therefrom. This condition having been fulfilled, I desired to measure approximately the work absorbed by passive resistances, which are in great part represented by the friction of the axles in their bearings. For this purpose I had a belt attached to the machine, and suspended therefrom weights that were increased until the machine began to move. It was ascertained in this way that a weight of 341 pounds, acting tangentially upon a pulley 17 inches in diameter, was scarcely sufficient to keep the machine in motion when it had been set running at a slight velocity. The diameter of the journals being 4.5 inches, and the weight of the movable pieces being 8,360 pounds, it is easy to satisfy one's self that the coefficient of friction is nearly equal to 0.15, and that the resistant work due to such friction rises to 1,595 foot-pounds when the axle of the machine is making a revolution. The normal velocity having to be 400 revolutions per minute, it results from these figures that friction will absorb about 11,000 foot-pounds per second.

This result approaches those that are daily being ascertained in the railway industry, where the coefficient of friction in the axle-journals lowers for the velocity of freight trains to 0.015.

Wishing to submit my results to a decisive control, and having no dynamometer at my disposal, I employed the following method: I gave the machine an increasing velocity, and, at the moment the speed reached about 600 revolutions per minute, I gave orders to allow the belt to fall that set it in motion. Then, a revolution counter having been applied to the shaft of the machine, I noted very accurately, every thirty seconds, the number of revolutions that it was making. I thus obtained the annexed table.

Time Marked by the Chronometer.	Number of Revolutions Marked by the Counter.	Mean Velocity for Thirty Seconds.	Second Differences.	Time Marked by the Chronometer.	Number of Revolutions Marked by the Counter.	Mean Velocity for Thirty Seconds.	Second Differences.
Seconds.				Seconds.			
0	0			600	3,486	107	5
30	275	275	15	630	3,588	102	5
60	535	260	14	660	3,685	97	5
90	781	246	14	690	3,777	92	4
120	1,013	232	13	720	3,865	88	4
150	1,232	219	13	750	3,949	84	4
180	1,439	207	12	780	4,029	80	4
210	1,635	196	11	810	4,105	76	3
240	1,821	186	10	840	4,178	73	3
270	1,998	177	9	870	4,248	70	3
300	2,167	169	8	900	4,315	67	3
330	2,328	161	8	930	4,379	64	3
360	2,482	154	7	960	4,440	61	4
390	2,629	147	7	990	4,497	57	4
420	2,769	140	7	1,020	4,550	53	5
450	2,902	133	7	1,050	4,598	48	6
480	3,029	127	6	1,080	4,640	42	9
510	3,150	121	6	1,110	4,673	33	9
540	3,267	117	5	1,140	4,697	24	
570	3,379	112	5	1,154	4,702	Stop.	

* In *La Lumière Electrique*.

This table makes known the number of revolutions as a function of the time, and it is easy to deduce from it the angular acceleration, and, consequently, the retarding element due to friction. Let us designate by:

P, the total weight of the masses in motion,
 ρ , their radius of gyration,
 ω , the angular speed at the epoch, t ,
 K , the resistant work developed by friction for an entire day,
 r , the radius of the journals,
 f , the coefficient of friction,

and we shall have the following relations:

$$K = -P\rho^2 \frac{d^2\omega}{dt^2} \quad f = \frac{\rho^2 d^2\omega}{gr dt^2}$$

In the machine under consideration, the masses in motion were reduced to, say, two homogeneous cast-iron disks 3.6 feet in diameter, weighing together 7,700 pounds. The steel axle weighed 660 pounds, but its motion of inertia was trifling with respect to that of the disk. The radius of the journals was 2.25 inches.

By reason of the slowness with which the velocity decreased, we may, without committing sensible error, replace the differentials by the finite differences, so that in definite we may say that the coefficient of friction is proportional to the second differences inscribed in the fourth column of the table. Now, these second differences (and this is the main fact that results from this experiment) vary from 15 to 3. Thus, then, in the experiment cited, the coefficient of friction diminished with great regularity at the same time with the velocity, and that its value varied in the ratio of 5 to 1, while the velocity fell from 550 to 145 revolutions per minute. From 145 to 120 revolutions per minute it remained sensibly constant, in order to increase anew very rapidly in measure as the velocity tended toward zero. As to the absolute value of this coefficient, it is 0.025 for a velocity of 550 revolutions per minute, and 0.005 for one of 145 revolutions.

However astonishing this figure may at first sight appear, it seems to me to be beyond dispute, being given the extreme regularity with which the readings made with the revolution counter succeeded one another, and the fact that the machine revolved for 19 minutes and 14 seconds before stopping, thus making 4,700 revolutions in order to exhaust the initial live force. The mean coefficient of friction, calculated by taking these last figures as a basis, would be equal to 0.013.

THE PRESERVATION OF TIMBER.*

REPORT OF THE COMMITTEE OF THE AMERICAN SOCIETY OF CIVIL ENGINEERS ON THE PRESERVATION OF TIMBER, PRESENTED AND ACCEPTED AT THE ANNUAL CONVENTION, JUNE 25, 1885.

The committee on the "Preservation of Timber" was appointed early in 1880.

Even before the gathering of statistics on forestry for the census of that year, it had become evident to engineers, from the increasing price and growing difficulty in procuring good timber, lumber, and railroad ties in many parts of the United States, that several of the sources of such supplies were being rapidly exhausted, and that as a measure of both private and national economy it would soon become necessary for us to resort to the artificial preparation of wood against decay, as had been successfully done for years in Europe.

The principal questions before the committee were to determine which methods of preserving timber against decay were most successful, and which were best adapted to the needs and current practice in this country.

Designing to base its report only upon well ascertained facts, the committee undertook to gather as fully as practicable the records of the experiments that had thus far been made in the United States, in order to compare them with each other, and with the results in Europe.

During the first year the committee accomplished but little. It had to overcome the public indifference which then existed on the subject, and the popular impression that probably no good would result from the investigation, as it was doubted whether there were any successful processes applicable to this country. It also had to ascertain where the much-scattered information desired was to be found.

This it was determined to do by circulars and correspondence, in order to find who possessed the data of past experiments, and who would be willing to impart them. For this purpose the committee issued some 2,000 circulars, with postal cards inclosed, on which a form of answer was printed. It corresponded with some 350 persons, gathered and studied some 55 pamphlets, examined 104 patents, and thus obtained the results of some 147 experiments, which are hereinafter tabulated and commented upon.

When this information began to come in, the committee found it very confusing. Not only were there the greatest possible differences in the opinions which were expressed, but the facts seemed to contradict each other. Apparently the same processes seemed to give different results, and these again differed from the European experience.

The committee, therefore, made a preliminary report to the Washington Convention in 1882, and asked for more time to ascertain how each experiment had been carried out, and to sift the evidence.

This was very tedious, both because there were few written records of these experiments and we had to rely upon personal recollections, and because the various persons who possessed such remembrance (and sometimes it required three or four to recollect the details of one experiment) had changed their business connection, and were scattered all over the country.

The committee here wishes to express its grateful thanks for the universal kindness and care with which its inquiries have been answered by the various gentlemen whose names will be found quoted as "authority" in the records of experiments (last column). Whatever value this report may prove to possess is due to their assistance.

NECESSITY FOR PRESERVING TIMBER.

The data on forestry gathered for the census of 1880 have made it apparent that the time has now arrived

when railway and other interests must largely resort to the artificial preparation of wood, in many parts of the United States.

As something may be accomplished by economy in the use of timber and by preventing waste, the Washington Convention instructed the committee to include the latter subject in its report. This was intrusted to Mr. Francis Collingwood, who, from past researches and present inclination, was best fitted for the task, and who has furnished a monograph on the subject, which will be found in Appendix No. 1, and to which particular attention is invited.

From this it appears that our Northern white pine forests, which less than a generation ago were considered as inexhaustible, now bid fair to yield but eleven years more of supply at the existing rate, and that there is a growing scarcity of wood suitable for railroad ties.

These facts must soon produce advancing prices. The farther, but still accessible, supplies are now being drawn upon, and as soon as they are exhausted, prices will go up with a jump. Several such periodical advances have already taken place, and in the older States, lumber, timber, and ties are ruling at about double the rates prevailing thirty or forty years ago, with a prospect that in less than half that time they will approximate European prices.

Timber has hitherto been so abundant and cheap in the United States that there has been but little inducement for economy in preserving it artificially against decay. It was known that the Europeans, who have been experimenting on the subject for more than a century, had accomplished decided success; but inasmuch as to be effective the work must be well done, it was found here to be relatively too costly.

So long as wood was cheap, the cost of efficient preparation, including interest on plant and price of antiseptics, was so great in proportion to the ruling timber prices twenty or thirty years ago that timber preservation did not pay. It was cheaper to let it rot in the good old way, and many corporations and individuals, after trying experiments which were more or less successful, abandoned the work as not conferring benefits commensurate with the cost.

Fresh attention was attracted to the subject by the publication of Messrs. Colburn and Holley's report upon European railways in 1858. In this very valuable treatise a chapter was devoted to sleepers, and to their preparation against decay. This brought about the erection of some wood-preparing works by the larger railways, and a plentiful crop of so-called inventions to cheapen the processes.

The chief efforts of this crop of inventors were either to discover some cheaper antiseptics, or to use a less quantity than had been found to be requisite in Europe. They also proposed to reduce the time of application. These efforts to produce effects with cheaper agents, or to apply the homeopathic principle of small doses to the preservation of timber, did not meet with success. The inventors patented sundry preparations and methods, took such jobs as they could obtain, sold whatever State and county rights they could, and retired from the field when the insufficiency or danger of their process became apparent. We will hereafter describe some of these processes; and it may fairly be expected, should the publication of this report lead to renewed attention to the subject of wood preservation, that there will be a new crop of inventions based on the method of "how not to do it."

It will hereafter be seen that some of the processes which have been successful abroad led to indifferent or unsatisfactory results in this country. The committee has endeavored to ascertain the causes of this, and found them in the unobservance of some of the conditions which have been found necessary to success in Europe. These relate to the time occupied in treatment, to the methods pursued, to the strength and quality of the solutions employed, and especially to the fact that the Europeans chiefly operate upon seasoned timber, while we have generally operated upon green or freshly cut timber, which requires a different treatment.

These various points will be touched upon in describing the principal experiments which have been conducted in this country, and will be found fairly to account for whatever differences have been found in the results—differences so great as to have led many managers of important interests to doubt whether any method of wood preparation against decay could be said to be successful.

PRESERVING PROCESSES.

Although experiments in wood preserving now date back some centuries, and the list of substances experimented with seems nearly endless,* there are but few antiseptics which have stood the test of time, or have been worked commercially.

Special activity seems to have set in with the Railway Era, and the consequent demand for timber which had to be exposed to the weather.

Hundreds of patents were then taken out in England, and the committee has collected 104 issued in the United States. Few of them, however, have proved permanently successful.

Mr. W. P. Moir, in a paper read in 1869 before the Philosophical Society of Glasgow, gives the following list of the names of the patentees and the most valuable compounds employed:

Kyan.....	1832		Chloride of mercury.
Margary.....	1837		Sulphate of copper.
Bethell.....	1838		
Bethell.....	1848		Creosote or pitch oil.
Burnett.....	1838		
Burnett.....	1840		Chloride of zinc.
Boucherie....	1839		Pyrolignite of iron.
Boucherie....	1846		Sulphate of copper.
Payne.....	1841		Sulphate of iron.
Payne.....	1846		Carbonate of soda.

* In a treatise on the preservation of timber, by Wm. Chapman, civil engineer, published in London in 1817, will be found records of experiments with, and mode of application of, the following substances: Sulphate of iron, sulphate of copper, nitrate of silver, corrosive sublimate, sulphate of zinc, carbonate of soda, caustic soda, charring, arsenic, barytes, salt, quick-lime, alumina, selenite, soap, resin, mineral coal, charcoal powder, coal-tar oil, vegetable oils, fish oils, essential oils, vapors of oils, etc., which had been applied to the preservation of timber for ships, jetties, bridges, buildings, etc., etc. This it will be seen covers almost all the ground since traveled over by patentees, who are constantly even now experimenting with and patenting substances which had been investigated and discarded by Mr. Chapman and his predecessors.

Concerning the modes of application, Mr. Moir says: "The methods employed practically in working these patents were three in number, namely, steeping, vital suction, and pressing in close vessels; Kyan and Margary employed the first mentioned method; Boucherie employed the second; and Payne, Burnett, and Bethell employed the third, which was also latterly adopted by Boucherie. The first and third methods required that the timber should be seasoned and free from sap."

These original patents have now all expired, and the use of the antiseptics which have proved most effective is open to the public. Whatever valuable patents have since been taken out refer chiefly to the modes of application, such as steaming in closed cylinders, to season the wood and remove the sap, and various devices to prevent the antiseptic from washing out upon exposure. Whether it will pay to use them each party will determine for himself; but it may be said in general terms that we now know the antiseptics which are most effective, and that the best modes of application are also pretty well known.

It will much simplify matters, for those who have not made a study of the subject, to group, so far as possible, the records of experience under the names of the best known processes, and to include therein various modifications which had sufficient of novelty to form the basis for a new patent. We have, therefore, classified the various experiments in the United States, concerning which we have been enabled to gather information, under the following heads:

1. Kyanizing—or use of corrosive sublimate.
2. Burnettizing—or use of chloride of zinc.
3. Creosoting—or use of creosote oil.
4. Boucherie—or use of sulphate of copper.
5. Miscellaneous—or use of various substances.

In referring to these experiments, we have endeavored to point out the causes of the success or the failure; and where full information seemed desirable, we give, in the appendix, the text of letters received by us over the names of their respective writers.

Many of these gentlemen took sufficient interest in the matter to hunt up and send to the committee specimens of the timber operated upon in these experiments. These specimens were exhibited at the National Exposition of Railway Appliances, held in Chicago in June, 1883, and are now deposited in the museum of the Civil Engineers' Club of the University of Illinois, at Champaign, Illinois.

KYANIZING.

As already stated, kyanizing was first introduced in 1832. It consists in steeping the timber in a solution of corrosive sublimate, probably the most powerful antiseptic known next to creosote.

The original proportions were one pound of the dry salt to four gallons of water, or a solution three per cent. strong by weight. Of this the timber absorbed six or seven pounds of salt per "load" of 50 cubic feet, or say ten to twelve pounds per 1,000 feet, board measure. As the price of this chemical was then \$1.25 per pound, it cost some \$12 or \$14 per 1,000 feet, board measure, and this led to a more sparing use of it. Ultimately the solution was reduced to one pound to ten gallons, or even fifteen gallons, of water (1.2 to 0.8 per cent. by weight), and with the latter the timber absorbed about two and a half pounds of the salt per 1,000 feet, board measure.

It was also applied by the "Boucherie" process, but the timber was found to absorb too much of the salt, and some of the solution necessarily ran to waste. The cost induced some of the contractors to further reduce the strength of the solution. To speak in plain terms, it led them to cheating, and kyanizing fell into contempt in England. Moreover, the process is very tedious, because of the length of time required to impregnate the timber, and there are now but two private establishments in this country (which will be noticed further on) which kyanize timber for their own use.

At this salt now costs fifty to sixty cents a pound, and as an absorption of four or five pounds per 1,000 feet, board measure, is considered sufficient, kyanizing now costs about \$6 per 1,000 feet, board measure. Where and when it will be profitable to use it will depend upon the price of the timber and its subsequent exposure; and in order to serve as a basis for forming conclusions on this subject, we have compiled the following table of sixteen experiments, which have been made in this country, and we add the comments which they seem to require.

COMMENTS ON KYANIZING EXPERIMENTS.

Concerning the first experiment in the list, General T. J. Cram says, in a report to the Chief of Engineers, made September 9, 1870:

"I have found our northern red cedar treated with the old Kyan process (an infusion of corrosive sublimate), after 22 years' exposure, lying on a slope of strong limy soil, to have gone to decay, especially the lower ends of the sticks; and kyanized white oak of Michigan, resting upon the same kind of dirt, dozed and rotted twenty years after the treatment.

"Chestnut railroad ties, grown upon the barrens of Maryland, kyanized and laid upon a limy soil, some miles north of Baltimore, in 1838, I saw tested 11 years afterward, and then perfectly sound and more solid than when laid; while those of the same kind of wood, untreated but laid at the same time, in the same kind of soil and exposure with the treated ones, lasted only 7 years before they required renewal.

"This experiment of kyanizing timber was the first, I believe, ever practiced in our country. Ties enough were treated for 1 mile of track, costing 12½ cents per cubic foot of timber. The process, however, was so unhealthy, salivating all the men, it had to be abandoned. It would be worth while to ascertain if those kyanized ties are yet sound. At that time the untreated ties cost only 14 to 16 cents."

Diligent inquiries among the present officers and some of the former officials of the Northern Central Railroad have failed to elicit any further information as to the final result of this experiment. It is very evident, however, why it was not followed up. Cross-ties usually contain from 2 to 3½ cubic feet of timber, which, at 12½ cents per cubic foot, would make the cost of treatment, at that time, from 25 to 44 cents per tie. This is wholly out of proportion with their original cost, which was 14 to 16 cents apiece. The same ties would now cost untreated about 50 cents apiece, and at the reduced cost of corrosive sublimate could be kyanized for 15 to 25 cents each.

* From the Transactions of the Society.