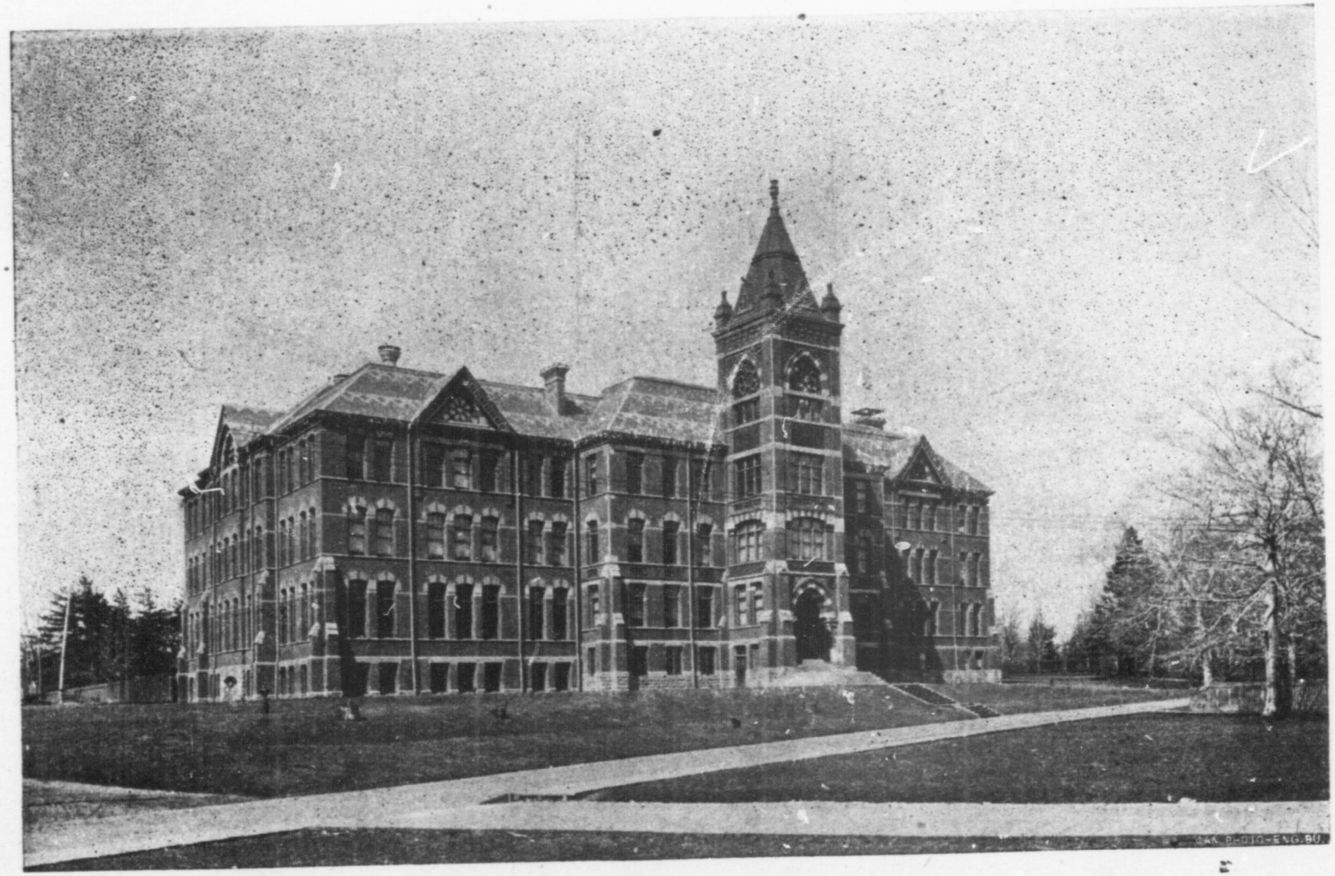

SCHOOL OF PRACTICAL SCIENCE, TORONTO.

University of Toronto Engineering Society
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PAPERS

READ BEFORE THE

ENGINEERING SOCIETY

OF THE

SCHOOL OF PRACTICAL SCIENCE

TORONTO

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CONTENTS

PREFACE

The proceedings of the Engineering Society of the School of Practical Science, now in its eleventh year, are herewith presented.

The contents of this issue will be found of great interest. It will give many helpful suggestions to students as well as to men already at professional practice, and will convince our readers that the members of the society take a lively interest in the opportunities presented to them by its semi-monthly meetings.

The paper on "Roentgen Radiation," by Mr. McLennan, will show that our institution was not in the least behind others, but in most respects exceeded them in the success of their experiments with the noted "X" rays.

Special attention is called to the masterly work on the "Pendulum," conducted in the laboratories of the School by Mr. Scott. This won for him the 1851 Exhibition Science Scholarship awarded by the University of Toronto.

A marked feature of the issue is the original work on Cements, and on Brick Piers. Several new inventions are also presented.

The present edition consists of 1,500 copies.

All of which is respectfully submitted.

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ENGINEERING SOCIETY

OF

The School of Practical Science

TORONTO

PRESIDENT'S ADDRESS

GENTLEMEN,—It is with great pleasure that I welcome you to this the first regular meeting of the Engineering Society.

I have to thank you most sincerely for the honor you have conferred upon me, the highest in the gift of the Society. It has been said by each president in turn that he feels entirely incapable of carrying on the work of his able predecessors, and I feel as keenly as they my incapacity to fulfil the onerous duties which devolve upon me. When I look back at the names of those who have in turn filled this chair, I cannot but feel my inability to attain to their standard of excellence. Among those who have led this Society none have surpassed Mr. Shields and Mr. Blackwood, to whom I looked up as a freshman and a sophomore. It is in the footsteps of such men as these that I have to follow. The task is a difficult one, yet be assured that I shall strive to perform it to the best of my ability. And I feel almost secure in promising this when I consider the personnel of the committee you have selected, composed as it is of energetic, enthusiastic students, who will doubtless do everything in their power for the advancement of the Society's interests. It is with great regret that we have been forced to accept the resignation of two of the best men on the committee, R. G. Black, fourth-year representative, and

C. L. Lawrie, treasurer. These gentlemen are now filling lucrative positions, and, while regretting our loss, we are pleased at their good fortune.

But, gentlemen, you must remember that the committee, no matter how capable, cannot make this Society a success without your hearty co-operation. It is not infallible, and when we act contrary to your wishes do not fail to draw our attention to the fact. It may be an expression of confidence to allow our actions to go unchallenged; but, on the contrary, such want of criticism may indicate on your part a lack of interest in the Society.

Let each one make himself thoroughly conversant with the details connected with the organization and working of the Society, and if such a course be pursued our meetings will never lack interest and enthusiasm.

A hearty welcome is extended to the many new members who are with us to-day. Last year the proportion of first-year men attending the meetings was large; we hope the present first-year will follow their excellent example. For the benefit of those who are present for the first time, it might be mentioned that the Society meets every other Wednesday, at 3 p.m., to discuss matters of general interest in engineering and kindred subjects, and to hear papers read based on topics which may be useful to us in our profession. Ample opportunity is also given for free discussion and interchange of ideas. These meetings are virtually part of the regular course of the School—minus an examination. The draughting rooms are closed, no lectures are given, every student is expected to wend his way to our gatherings and listen with such interest or patience as he can command to the instructive or heavy paper which may be before the meeting. And though the paper may not always be on your special line, or entirely within your grasp, still there is always the educative value of intercourse with one's fellow-students.

The students of the School of Science have the name of being loyal to their colors, and always exhibiting a true college spirit. This, gentlemen, is most desirable, and we trust we shall always possess the *esprit de corps* which has been so useful to us in the past. But in order to work well together we must know each other, and these meetings are almost the only medium for bringing the four years together.

In addition to the benefits which are to be derived from the Society's meetings, there are also the advantages of an excellent library, where is to be found, in ever-increasing number, such books as engineering students or engineers require. In the library are also kept the leading engineering papers and magazines of the day. We hope that some arrangement will soon be made so that these will be accessible at any time, and not at certain hours as at present.

The Society has been steadily growing in influence and interest since its inauguration, under the able guidance of our worthy principal, Professor Galbraith. Every year there has been an advance over the preceding, the past year being no exception. A larger number of papers were read, and the general interest taken in the meetings showed how much the work of the Society was appreciated. Of the eighteen papers read eight were contributed by undergraduates, three from the second-year and five from the third. These eight papers, though they may not be of surpassing value to the literature of the day, were, however, of great value not only to the Society, but also to those who have written them.

The authors of the papers derive the greatest benefit; each one chooses a subject in which he has some especial interest; and long before the paper is finished he finds many points in which his knowledge is insufficient, and so investigates further to obtain the necessary information. When the paper is ready to be presented he has become very much better acquainted with his subject, and is able to lay before his hearers in a clear and concise manner the information thus gained. Allow me to say that there is nothing to hinder any of you from writing on some subject which has come under your notice. You will be amply repaid for your labor by the knowledge you yourselves gain, and by the interest your fellow-students take in your efforts.

There is yet another inducement, one which is or should be of secondary importance, but which nevertheless is of great value to the Society. I refer to the action of the Council in giving marks for each paper read, the maximum being 100; all marks over 50 are considered in granting honors. But you may say you do not intend to write for honors at the examinations; nevertheless we shall be glad to hear from you in the line of original contributions, for, as has been said before, the marks given are of but little value in comparison with the information and experience gained in writing the paper. Of the eight undergraduate writers last year, only three were honor men.

Gentlemen, some of you may not feel inclined to write a paper, but burn with a desire to address the Society. For that and other reasons, I would strongly advocate the introduction of debates. We are attending the School of Practical Science; it sometimes seems as if we are so intensely practical, so matter of fact and prosaic, that it is impossible for us to adequately express our ideas without the use of draughting boards, rheostats, transits, or telescopes. It may not be expected of the student of the School of Practical Science to soar aloft on the wings of oratory, as do his brethren in the non-kindred course of Political Science; yet it is to be expected that men are to be found here who can maintain the honor of the School on any platform. We send two men each year to debate against 'Varsity; why should they go untrained, comparatively speaking, any more than that men not fleet of foot should be sent to carry our colors in a "team race"?

We should take every opportunity of gaining ease and confidence in public speaking, for it is only by constant practice that we shall be able to speak fluently and intelligibly. In our professional life we will come in contact with men of knowledge and experience who will frequently judge us by our power of expression. Naturally, when practising here we may have to put up with many a jesting remark or good-natured joke, but such should only serve to spur us on to renewed and more perfect effort.

Before we parted last spring, pamphlet No. 8 was distributed, thanks to the untiring efforts of the editor, Mr. Angus. The pamphlet was a large one. The plan of sending out, in advance, copies of the papers to be read worked admirably, and contributed largely to the success of the meetings. I trust that this plan may be continued, for by it the writers of papers may bring their views on any subject promptly to the notice of the profession, and not six months or a year after the paper has been written.

The pamphlet was a large one; we looked at it admiringly, and, as we noted its size and scanned its pages, each of us felt proud of our Engineering Society. The sky was clear, not a cloud in sight, nothing, in fact, to mar the serenity of all concerned. And so, at first, thought the committee to whom you intrusted the management of your affairs. But soon a cloud appeared no bigger than a man's hand; someone hinted at money! This vague hint troubled us little; we looked back on our track, we saw the pamphlet, large and entertaining, and felt confident. But the cloud grew as we looked, and the pamphlet, large and entertaining, grew, as we looked, larger, but less entertaining. The cloud spread, soon the whole heavens were overcast. We scanned the horizon; away in the distance appeared havens of safety, but the storm was fast coming up. We faced it bravely, but shuddered when it broke, for there burst on our devoted heads a thunderbolt—an account for $580.08. Gentlemen, we did not despair, but set to work with a will. One hundred dollars was the sum total of the cash in the treasury; $225 followed from advertisements, collected by the persevering efforts of the treasurer; and finally $50 came from the government, making in all $375. We breathed more easily, but felt somewhat heavy-laden, being still $200 short.

Naturally the question arose, Where was the money to come from? We could count on this year's membership fees of $100 or more, but against that had to be placed the running expenses of the Society, postage, printing, library, etc., which last year amounted to $110. We endeavored to obtain cheap postal rates for the distribution of the pamphlet, but failed. We interviewed a minister of the government, and when he understood the state of affairs he very kindly granted us $50 nominally in payment of pamphlets already distributed among members of parliament. On May 7th a letter was issued to our graduates asking for subscriptions; to this we received but few replies. Two months and a half later, July 25th, we issued another letter, stating fully our position and asking aid. Many of the graduates responded liberally, and none more so than those who are now with us as Lecturers or Fellows. Up to date we have received $65, and now we take the opportunity of thanking the graduates who so generously came to our assistance.

But, gentlemen, there is still a deficit. We have done all in our power for the Society, and, as a last resource, have fallen back on our undergraduate members. The Society has heretofore been dealing, philanthropically, in draughting material, giving the students the best of imported paper at cost price. The price has been slightly increased, so as to yield us a revenue. With this added source of income, a reduction of running expenses along with a few more subscriptions, we hope that by the end of the college year our accounts will show a slight surplus.

But what for the future? Evidently there must be a change in our financial methods. One of our graduates remarked: "'Out of debt, out of danger,' is a good and safe maxim, and one I recommend your financial committee to adopt. Anyone can do wonders with money, but the sharp business element is that which enables one to do much with the money in hand." The advice is good; we would do well to follow it. Our ordinary running expenses cannot be curtailed to any great extent. The expenditure on the pamphlet is where the change must be made; funds must come in from new sources if it is to be kept up to the size of No. 8. But is this a necessity? Must we publish all the papers read? Or, should we make a selection, as was done in former years? Then there is the question of advance proofs; that is an additional expense, but, if at all possible, I would strongly advocate the continuance of this plan, which worked so successfully last year.

It has been suggested that graduates and undergraduates should each pay forty or fifty cents for the pamphlet. I think, however, that the latter should receive the pamphlet free, as in the past. As to the graduates, I am of the opinion that they would be quite willing to pay a small amount for a publication which is well worth the money, especially when they know they are assisting the Society of their old *alma mater*. However, these and other points will doubtless be fully dealt with during the coming term.

Before leaving the question of finance, we wish to express our sincere thanks to the Alumni Association of the School, especially to the president, Mr. James McDougall, and to our genial friend the secretary, Mr. A.T. Laing, for their efforts on our behalf. Upon hearing the condition of our finances, they very kindly issued a letter to the members of the Alumni Association —who are also members of the Engineering Society—asking them to aid us in every way possible. We have no doubt that their appeal helped us very materially.

The last three years have been active ones in the history of the Society. Our regulations are not "like the laws of the Medes and Persians, which change not," for many an amendment, or amendment to the amendment, has been made to such and such an article of our constitution, until now some of the articles are exceedingly cumbersome. Would it not be advisable to have the constitution thoroughly revised by a small committee, the result of its deliberations to be submitted to the Society for its approval?

Notice of motion has already been given the Society of still another change in the constitution, viz., to transfer the office of editor-in-chief from the fourth-year to the graduates' representative. This is a motion of considerable importance, and requires serious consideration.

While still of the opinion that "member of the Engineering Society" and "student of the S.P.S." should be synonymous terms, and that the Society should be the mainstay of everything of interest to the student, yet, from financial considerations, it may be advisable that such a controversial subject should be held over for consideration at some future time. Yet, whether or not the terms are synonymous in theory, they are so in fact. We are a separate body in university life, and it should be the aim of everyone of us, as without doubt it is, to make the name of the S.P.S. both feared and respected in college and campus.

Many pleasant letters have been received from our graduates, and from professional men not graduates, speaking very highly of our latest publication, and such praise we take as no small testimony to its value.

May such expressions of appreciation spur us on to greater efforts in the future !

Several interesting papers have already been promised for the coming term. As to the subject-matter for a paper, the opinion of one of our graduates, now a busy engineer, fully covers the ground: "Make the papers essentially practical, avoid too much theorizing, get at what a man will meet in practice, and the rest will turn up in hundreds of magazines and scientific papers published as business concerns."

The librarian has on hand, for distribution, copies of nearly all our previous pamphlets. These volumes are full of information. Every student should examine them for himself; for he will find much that is of direct practical importance in them, papers bearing almost directly on work set down in the curriculum, such as "The Fundamental Principles of Mechanics," "Diagrams," "Hydraulic Cements," "Notes on Cements," "Hirn's Analysis," "Frieberg's Smelting Process," etc.

Before taking my seat, I wish to refer to the recent appointments to the staff of the School, namely: W. E. Boustead, Demonstrator in Metallurgy and Assaying; W. M. Minty, Fellow in Mechanical Engineering; and last, but not least, one who is well known to you all, and who formerly filled the president's chair with so much energy and ability, A. E. Blackwood, Fellow in Electrical Engineering.

On behalf of the Society, I congratulate these gentlemen on their appointment.

Gentlemen, I trust that we shall have a profitable and instructive year, and that we shall all be worthy of and live up to our college motto,

"*Scite et Strenui.*"

Toronto, Oct. 16th, 1895. G. M. CAMPBELL.

ILLUSTRATIVE CASUALTIES IN THE WORKSHOP

By Cromwell Gurney, '96.

In nearly every shop the details of all accidents, particularly those resulting fatally, are suppressed as far as possible, and this suppression results in the dangerous part being left unprotected, even after the casualty due to a want of protection has occurred. For this reason a brief outline of some common accidents that are happening every day may prove of use to men whose vocation calls them to a constant handling of all kinds of fast-running and so-called dangerous machinery Every accident resulting in injury to a human being should be reported to the factory inspector, but in a large establishment the foreman in whose shop the accident occurs usually leaves it to the superintendent, while he, the superintendent, leaves it to the foreman, and in this way it is never reported at all.

There are more casualties reported as occasioned by circular saws than from any other one type of machinery, and, for this reason, too much prominence cannot be given to the danger of carelessness in handling them.

Sometimes it happens that no one is to blame when a fatality occurs, as when a circular saw breaks, as shingle saws and veneer saws are liable to do, being necessarily very thin and running at a very high rate of speed. All circular saws should be made of the best crucible or finest silver steel, and should be carefully and uniformly tempered throughout, requiring great skill and watchfulness on the part of the temperer. Great care is also required in hammering out these saws, as often the process forces the strain to one part, causing a slight bulge, which may crack when some unusual strain is put upon the saw. The crack relieves the strain caused by the bulge, and by boring a small hole at the terminus of the split it will go no farther, the saw being safer than before the fracture occurred. In using a rip saw a wedge should always be inserted behind the saw, in order to keep the cut open, that the wood may not bind the saw. A terrible example of this neglect came under the writer's observation. A clumsy hand was pushing a large piece of lumber upon a saw so fast that the machine almost stopped. At this moment the damp wood bound the saw, with the effect that the heavy lumber was shot, end on, into the man's chest, mutilating him horribly. Most of the accidents, however, are not due to imperfect saws, but to carelessness in the employee, who, as a rule, loses his fingers as a penalty.

Generally speaking, the circular saw is always dangerous when in motion, and care is needed on the part of the attendant when operating any style of saw machine; but there are adjustments or adaptations of saws much more dangerous than others. Those running through a slot in the table are perhaps the most harmless as regards cutting accidents, while those overhanging their frames, and projecting out, are the most dangerous. Of the many guards devised none are free from objection, for one reason or another. Sometimes a hammer or wrench, left on the table, will be jarred until it comes into contact with the teeth of the running saw. As the teeth are unable to bite through the metal of the wrench or hammer, the saw itself breaks, sending its fragments with fearful velocity, as may be judged by one case, where a large piece of a saw was buried completely out of sight in a neighboring post.

As to covering or guarding all saws, it is impracticable, without very much interfering with the quantity of work turned out.

The floor about the neighborhood of a naked saw should be kept altogether free from obstructions which would be likely to cause stumbling, as to stumble against a running saw means horrible mutilation.

In using both emery wheels and saws, the operator should stand slightly out of the plane of the wheel, which puts him out of range, so to speak, if any accident should happen.

Next to the saw in degree of danger is any machine with more or less exposed gears. Machines used for reaming and tapping steam fittings, such as elbows, are a perfect maze of gears, and consequently are considered very dangerous. According to law such machines are supposed to have their gears protected, but this is impossible in many cases, or, perhaps, the upper half of a gear wheel is shielded, while the lower is exposed, which gives the operator a false sense of security. While operating this class of machinery every part of the garments should be buttoned up snug, and no loose or ragged sleeves worn which might catch and draw the hand or arm in.

No gear machine should be lubricated while in motion, as it is by this means that many a catastrophe occurs, as happened in the case of a man who was drawn into the underside of the cogs on the crown wheel of a water wheel. It was supposed that he was oiling the bearings.

Another source of a great many disasters is carelessness in handling running belts. Some advantage may be derived by a review of several typical examples of accidents caused by belts.

One peculiar accident, resulting only in a badly frightened youth, might be here mentioned. The particular youth in question desired to see how close he could stand to a fourteen-inch driving belt. The belt, however, owing to a violent fluctuation of load, gave a quick fling, and the overlap where the joint was made caught in some portion of his apparel, with the effect that he was whisked off his feet, and rapidly borne toward a three-inch opening in a brick wall, through which the belt disappeared. When he reached the hole in the wall, like a flash of lightning, every rag of clothing he possessed except his boots was wrenched off, and he dropped helplessly to the floor, very much shaken, but otherwise unhurt.

Great care should be exercised in putting a belt on a running pulley, as this operation causes many accidents. Every year there are the usual number of broken and dislocated arms reported from this cause. The reason of this is apparent at once to anyone who has felt the tremendous and sudden wrench when a fast-running belt starts unexpectedly while it is being put on.

The most significant fact in all belt accidents is that the victims are not, as a rule, green hands, but old and skilled mechanics and engineers, whose familiarity with machinery has made them careless.

A belt should never be held upon a pulley with the foot, as there is great danger of the boot getting caught in the joint of the belt. Neither should one be held on by a piece of wood held loosely in the hand, as the stick wrenched from the hand might strike the face or head of the holder.

The iron foundry is correctly given the credit, or perhaps it should be discredit, of being one of the most dangerous departments of modern manufacturing establishments. On a sultry July day, when the thermometer near the cupola registered 140° Fahr., the writer has seen four men, one after another, receive burns more or less dangerous. The extreme heat creates a recklessness in the men which sometimes results disastrously. Nearly all these accidents occur from a careless man slopping the iron from his ladle on to his mate's foot, or perhaps the carelessness in the victim himself, who steps backward against a ladle of molten metal. If the melted iron strikes a smooth surface, it rolls off and does no harm ; but, as a rule, the moulders wear lace boots, and the metal lodges in the laces and burns to the bone before the boot can be removed.

In a moulding room, all obstructions which might cause the men to stumble while carrying the iron must be removed, as a bad stumble by one of the men carrying a 300-lb. ladle means a painful accident to both his mate and himself.

Some time ago a peculiar incident happened in a large American foundry. The brickwork through which the tap-hole was made completely gave way, allowing the whole fluid contents of the cupola to come seething out. The hot iron, striking some water in the sand, flew to great distances, setting fire to the roof of the building in a dozen places. It was only by incredible activity on the part of the employees that the building was saved. In this case but one life was lost; the melter was standing with his back to the cupola, and so had no chance to make an effort to escape. Nearly every man in the whole shop was more or less burned by the flying metal, while of the unfortunate melter there remained but a few charred bones.

It is only of late years that people have wakened up to the real danger of spontaneous combustion. If a piece of waste that has been used for wiping up linseed oil is left in the sun for a very short time, it will nearly always ignite of itself, so that any carelessness in leaving oily waste about is very dangerous. Another source of spontaneous combustion is from a painter who, having used a pair of overalls in an atmosphere of benzine and turpentine, rolls them up tightly and puts them into a drawer. In nine cases out of ten, if the above painter came back to look into the drawer within five or six hours, he would find his overalls smoking.

In some large factories the oil room is in a cement vault, and hence is quite safe; but where a wooden shed is used, the floor becomes so saturated with oil that it would ignite at a comparatively low temperature. In such a case some kind of a chemical fire extinguisher should be close at hand.

The explosive qualities of naphtha are sufficiently well understood to need but slight notice here. However, it might be mentioned that if a warm piece of waste, saturated with naphtha, be carried in the hand through the air, it will leave a trail of naphtha vapor behind it, along which fire will travel till it comes to the saturated waste itself. This same thing applies to gasoline also. In some works benzine is used to thin the varnish. Then, if the varnished article is baked in an oven to which the air has access, there might be an explosion. If the temperature of the oven is unduly raised, and then allowed to drop again, the gases in the oven contract, drawing into the oven a supply of air, which, mingling with the vapors of benzine, forms a terribly explosive mixture if ignited. In the neighborhood of a hot oven this ignition is very likely to occur.

Occasionally we hear of a death from asphyxiation, caused by inhaling the hydrocarbon vapors issuing from the tail pipes of petroleum refining stills. However, of late years, these vapors have been trapped and carried into the air by means of a ventilating pipe. So virulent are the above fumes that a single full breath, where they are present, has been known to produce unconsciousness. The fumes of naphtha being two and one-half times heavier than air sink rapidly when liberated, passing under doors and down stairways to the basement, where if they reach a burning gas jet an explosion occurs.

An interesting and instructive accident happened about three years ago in this city. A man in a cleaning and dyeing works was cleaning a silk dress by immersing it in a vessel containing benzine. The vapors at once took fire, and the man was so badly burned that he died within ten days. There was no fire in the room, nor, so far as could be learned, within sixty feet of him. Could it not be that an electric spark, produced by the friction of the silk, as it was immersed, ignited the vapors? This theory would seem to be supported by the fact that if a piece of waste, saturated with benzine, be held in the neighborhood of a fast-running belt, the static discharge will almost invariably ignite the benzine.

Several fires have been caused by the fact that the water pipes are at a different potential difference to the gas pipes, or *vice versa*, as sometimes occurs owing to stray currents from the electric street car service. A case of this kind occurred where there were two pipes about one inch apart, and both paralleling an elevator shaft. The potential difference across these two pipes must have been considerable, for, whenever the elevator went up, the jarring of the two pipes caused a brilliant spark to leap from one to the other. As one of the pipes was used for conveying illuminating gas, and was partly eaten into by the spark when discovered, the danger from fire must be apparent, for had the spark damaged the pipe sufficiently to allow the gas to escape it would have been instantly ignited by the same spark, and in all probability the building would have been burned.

Do not these foregoing examples seem to indicate that foresight is a quality that should be conspicuously present in all good mechanics and engineers?

LIGHTNING ARRESTERS

H. P. Elliott, '96.

The problem of obtaining an efficient protection against damage by lightning has been before the operators of electrical machinery ever since it came into use. From the first it was noticed that, during thunderstorms, charges of static electricity travelled along the wires which were used to convey current from power stations, telegraph instruments, and other electric apparatus.

A coil of wire offers more resistance to a static charge or varying current than it does to a direct current. This is due to what is known as self-induction. It is known by theory, and borne out by experience, that when a static charge passes through a coil, E.M.F.'s, which tend to oppose its flow, are formed in the same way as the induced currents in a coil of wire placed near another coil which is traversed by an alternating current.

Most electrical apparatus contain coils of wire which offer high resistance to the passage of static charges, so that they find less resistance in piercing the insulation of the wire than following the successive convolutions.

Although this of itself may do no serious damage as far as burning or shattering the machines, it short-circuits the coil, and renders it useless, and in the case where it conveys a heavy dynamo current very great damage may be done.

The telegraph instruments were the first to suffer from this. Sometimes, however, instead of springing through the coil, and short-circuiting it, the electricity would spring off the line to water-pipes and other metallic objects connected to the earth. When this took place, it was noticed that the instruments were not damaged. The idea of the first lightning arrester was evolved from this. A point, or several points, were connected with the live wire just before it entered the instruments, and these points were placed near others connected with the earth, so that it was supposed the charge would pass to the earth through these rather than through the instruments. This was generally found to be the case, but very often the lightning ignored the path to earth, and damaged the instruments, as before. This spark gap is the main principle of all lightning arresters of to-day, and on telegraph lines and circuits of a similar nature they are of essentially the same form as the primitive ones.

When a lightning arrester is placed on a circuit used for supplying arc lights, incandescent lights, or current for anything that requires a high potential, the arrester must consist of more than the simple spark gap. You all know that in a circuit like this the potential is many volts above that of the ground. I mean by this that, if the circuit is connected to the ground, current will flow back to the generator through numerous leaks in the wire, or perhaps through another ground connection, or the frame of the machine itself. The amount of this current depends on the resistance it has to encounter, and in many cases may amount to a short circuit on the dynamo. It is found that when the static charge leaps to earth over the spark gap, the current from the dynamo will follow it; arcing between the points, and forming a dangerous ground or short circuit, which blows the fuses and interrupts the operation of the machine for the time being.

Naturally, the first improvement sought for was something to prevent the dynamo current from following the lightning discharge. The first device used was a fuse placed somewhere in the ground connection. This fuse would be blown before the main circuit fuse was affected, the circuit thus remaining unbroken. This answered the purpose very well, but as the fuse had to be replaced after every discharge further improvement was necessary. A New York firm manufactures arresters upon this principle, but they are each provided with eleven fuses, arranged so that when one is blown another is immediately placed in circuit.

There are a great many arresters that depend upon a mechanical device to break the arc. Most of them have an electro-magnet, which is energized by the current when it cuts across the spark gap; the magnet attracting an armature which breaks the circuit. It is easy to see that these will get out of order unless given constant attention; and, in any case, they are gradually destroyed by the heavy ground, no matter how quickly they may stop it. If they get out of order there is generally a brilliant display of fireworks for about three seconds, and the arrester is reduced to a lump of copper.

A very great improvement upon the mechanical device was made by utilizing a well-known property of a magnet—that of repelling an arc.

The spark gap is formed of two metal wings, which are close together at one point, and slope away from each other till there is a space of several inches between them. The wings are placed between the poles of a powerful magnet, so that the arc is blown from the narrow space outwards, until it breaks. These arresters work very well. I had an opportunity of seeing them in operation in a power house equipped by the Canadian General Electric Company. They required little attention but when either the wires were set too close together, or if any dirt collected between them, they would sometimes not break the arc.

It might be well, before going further, to say something about the way lightning gets on the wires, and how it travels along them. Probably the greatest authority on the subject of protection from lightning is Mr. Alexander J. Wurtz, of the Westinghouse Company, whose thorough technical education has enabled him to far outstrip all other investigators. I have noticed, while in a power house, that although the arresters usually discharge when there is a big flash of lightning, they often do so when no flash is seen, or when it is too far away to have struck the wires directly. The arc lamps were often what the men called "struck by lightning," but, upon examination, they bore no traces such as we would expect to find had they been struck. They would have one or more of the coils burned out, just as when the coil is short-circuited. A transformer, said to have been struck, merely had one of the coils burned out, and neither the iron case nor the pole had any marks. Whenever there was an extra large flash of lightning during the storm, and the arrester discharged, the men employed in the power house would say the lines were struck at such and such a point; but I never saw a part where the wires appeared to have been injured, or the insulation burned off.

These facts seem to show that the lines are seldom or never struck directly, and Mr. Wurtz gives the following theory as the one that is most probably correct. The atmosphere, especially during thunderstorms, is found to be at a potential different from the earth, and increasing as we go higher, so that at a height of 1,000 feet a potential of 10,000 volts has been measured. It is not probable, however, that the atmosphere surrounding the lines is under sufficient strain to cause the wires, which are electrically a part of it, to discharge to the earth over the stop gaps. The lightning discharge, whether from one cloud to another or to the earth, is of an oscillatory nature. There are, in fact, as many as ten or more discharges until equilibrium is restored. These discharges will have an inductive effect on the wires and the atmosphere surrounding them, causing waves of high potential to travel along them to and fro, so that now one point and now another is under tension and tends to discharge. These waves also combine and form greater ones, and if they meet a resistance, such as a choke coil, field-magnet coil, or transformer, will pile up at these points, so that the tension is enormously increased. If the wave, on piling up in this manner, cannot flash off to something connected with the earth, it may he reflected back, but is very likely to pass through the coil by the path of least resistance, which is, as was explained, through the insulation.

From this it will be seen that the lightning arresters will not always protect the apparatus. A wave passing along the line may not be great enough to cross the spark gap, but may increase in size and cut through the insulation at some other point on the line. The arrester, in other words, does not arrest, but merely offers an opportunity for the charge to pass off. The spark gap, then, must be arranged so that it will be at a point where the tension is high. There are two ways of doing this: one is to have the gap just before the coil; the other, to have arresters placed at intervals all along the lines. The former method has been used for a long time. The arresters were, in most cases, a continuation of choke coil and spark gap, and were placed either as near as possible to the place where the wires entered the power house, or on the machines themselves. The necessity of having arresters distributed along the lines has only recently become known.

The ideal lightning arrester must be very simple in construction, with no moving parts to get out of order, and must last throughout a great number of discharges; for, when out upon the lines, it cannot receive much attention. It must be cheap, so that a large number may be used. The spark gap must have a very low inductive resistance; but must prove an effectual barrier to the dynamo current, so that it may discharge very easily and before damage is done elsewhere. An arrester fulfilling these conditions would be very far in advance of any of those described, and such a one *has* been constructed. I think you will agree with me in saying that it approaches very nearly to the ideal.

Mr. Wurtz, in studying the behavior of lightning, and trying to get an efficient arrester, found that certain metals, zinc, bismuth, antimony, cadmium, and mercury, would not, under certain conditions, sustain an arc due to an alternating current. He uses in his (lightning) arrester seven cylinders made from an alloy of the above metals. Each cylinder is $\frac{3}{4}$ in. in diameter by $1\frac{1}{2}$ in. long, and the seven are fastened, side by side, on an insulating block, with a space of $\frac{1}{64}$ in. between each. The outside cylinders are connected to the circuit wires, and the centre one to the ground; the discharge passing readily between them. These arresters can only be used for alternating currents, and the above form is suitable for 1,000-volt circuits. For 2,000 volts two arresters can be coupled together, and so on, up to 10,000 volts or more.

For use on direct current circuits, Mr. Wurtz has designed an arrester which works on a different principle, but is equally efficient. He noticed that a static discharge would take place much more readily over a surface such as smooth glass, wood, or marble than through air; also that if the electrodes were joined by a lead pencil mark or a charred groove on the surface, the distance across which the charge would spring would be much increased. It is known that the electric arc is caused by the current being conducted by the vapors of the electrodes, and that if these vapors could be suppressed there would be no arc. Combining these two principles, the following (method) was evolved: The two electrodes are placed three-eighths of an inch apart on the surface of a block of wood, on which several shallow grooves are burned, joining them. Another block of wood is placed over these, and screwed tightly down. By this means a very low resistance is offered to the discharge, although the gap is wide; moreover, the electrodes being covered, there is no chance for the vapors to form a conducting path from one to the other.

After a discharge has taken place on the metallic arrester, there is only a slight pit or burn on the cylinders, *so they will last for practically an indefinite time.* The arrester for direct current circuits gave some trouble at first, as a cavity was soon worn in the wood by the static discharges; but this has been remedied by cutting a slot between the electrodes at right angles to them, so that the path of discharge is opened to the air without the electrodes being uncovered. These arresters now show hardly any signs of use after seven or eight hundred discharges.

The manner in which the arresters are installed is a very important point, if they are to be of any service. The old way was to have an arrester on each line coming into the power house. When they failed to protect the dynamos, the blame fell on the construction of the arrester. The fact is, the arresters will not stop the lightning, but will, as I have said, gave it an opportunity to pass off. Choke coils should always be used in conjunction with spark gaps, and, when there are long stretches of line with no apparatus on them, it is sufficient protection to use a series of choke coils at each end, with spark gap arresters intervening. In the case of protecting transformers and other translating devices, they may be utilized as choke coils themselves.

The ground connection, above all, should be well made. It is usual to bury a copper plate or piece of iron casting in permanently damp earth, and the wire leading from it must be firmly soldered to it, and run as straight as possible. Most makers of arresters recommend that the ground plate be buried in broken charcoal. It is very important that the ground wire be straight, and not wound into any fancy coils, which increase its resistance, and may cause the current to leap off to some other object. I once noticed a peculiar thing in a power house which, I think, was caused by a bend in the ground wire. The arrester was placed on the back of the switch board, about two feet from the floor and three feet from the wall behind. The ground wire had to run under the floor to the wall, where it joined another wire which carried all the grounds to a copper plate buried in the earth. After a very strong discharge one night, a number of charred lines were to be seen on the floor, apparently running from the ground wire, as if the current had leaped from it.

PLANIMETERS

J. W. Bain, '96.

The evaluation of the area of an irregular figure is a problem of frequent occurrence in certain departments of engineering, and it is because of the value of the planimeter in such determinations that this paper has been written.

The planimeter is an instrument for measuring areas, regardless of the character of the bounding perimeter. When we are dealing with simple geometrical figures, we can find the area, after a little calculation, from the measurement of certain lines ; but when a figure whose boundary is an irregular curve comes under our notice resort must be had to the following process. A number of right lines are drawn to form triangles or rectangles, in such a manner that their combined area approximates, as closely as possible, to the true area of the figure. Simple and easy as this process is theoretically, in practice it becomes tedious, demanding the closest attention and considerable skill for the attainment of a moderate degree of accuracy. If only an approximate result is required, the planimeter may be used with great advantage, and both time and mental labor saved.

The first planimeter was probably invented by Hermann, of Munich, who worked it out with Læmmle, in 1814, and since then quite a large number of instruments have been designed on the same theory. Different though they may be in their mechanism, all these depend upon a certain principle, which may be enunciated as follows :

Let M (Fig. 1) be the plan of a disk rolling in contact with a straight guide PQ, which is parallel to OX, and at a distance from it equal to the radius of the disk, so that the plan of the centre of the latter always lies in OX. Let *m* be a roller upon the surface of the disk, graduated and connected with wheel-work and an index, so that the distance turned through over the surface of the disk can be read in revolutions, or parts of a revolution. The plan of the point of contact B of the roller with the disk is always made to coincide with that particular point on the curve which is in a line, drawn at right angles, to OX, through the centre, C, of

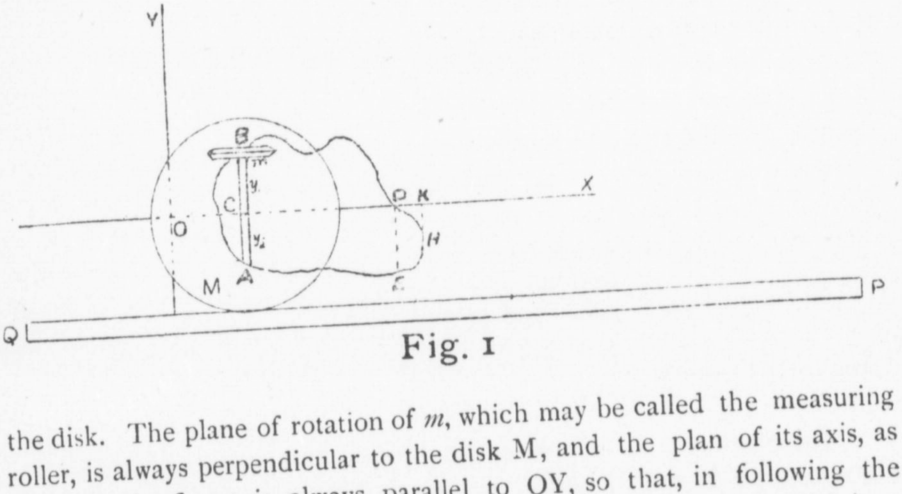

Fig. 1

the disk. The plane of rotation of *m*, which may be called the measuring roller, is always perpendicular to the disk M, and the plan of its axis, as shown in the figure, is always parallel to OY, so that, in following the curve, it slips backwards and forwards across the surface of the disk in a direction parallel to OY.

It is important that the action of the measuring roller should be understood. Any motion may be resolved into two components: one in the plane of the roller, and the other parallel to its axis; and it is evident that the former will cause rolling, and the latter slipping, in the roller.

Suppose the disk to roll along PQ for a distance Δx equal to the width of the element AB.

Then if y_1 = distance of B from OX,

R = radius of disk M,

r = radius of measuring roller *m*,

n_1 = consequent reading of measuring roller for this travel of disk.

Then $\frac{y_1}{R}\Delta x$ = linear distance turned through by a point on the disk at the distance y_1 from the centre.

$2\pi r n_1$ = linear distance turned through by a point on the circumference of *m*.

But since *m* rolls on M these distances are equal.

Therefore $$2\pi r n_1 = \frac{y_1}{R}\Delta x$$

or, $$n_1 = y_1 \Delta x \times \frac{1}{2\pi r R}$$

But $\frac{1}{2\pi r\mathrm{R}}$ is a constant which, by taking r and R in suitable ratio, may be made unity.

Then
$$n_1 = y_1 \Delta x$$
that is, the reading of the roller m measures that part of the area of the element above OX.

If the point of contact be made to follow round the curve continuously in one direction, then, when the portion of AB below OX is being measured, the disk is moving in the opposite direction along PQ; but, at the same time, the roller is turning in the opposite way, relatively to the disk, to that which it was doing before, since the point of contact is now below C. The final result of these two opposite motions is to cause the roller to turn as at first, and so add the result given for CA to what was given for CB. If the motion of disk Δx for the width of AB be now regarded as negative, and $-y_2$ = distance AC,

Also n_2 = reading of roller for this element of area,

Then, by reasoning similar to that already used,
$$n_2 = (-y_2)\,(\Delta x)$$
$$= y_2 \Delta x$$

Also
$$n = n_1 + n_2 = (y_1 + y_2)\,\Delta x = y\Delta x$$
= area of element AB.

This reasoning holds for any possible position of the rollers or of the axis OX, which may be altogether outside the figure, as it is practically, for the integration of the portion DHE. Then it will be found that DKH is subtracted and DKHE is added, so as to give the required actual area DHE.

Inasmuch as this reasoning is independent of the actual value of the width of the element, and as the vertical motion of the roller m has no effect, in theory, upon the distance rolled through by it, therefore in limit, when Δx becomes infinitely small, the actual value of the series of infinitely narrow strips which compose the figure ABDE is given by the final reading of the roller when the traverse of the boundary is completed.

This description, it is hoped, will enable the reader to understand the theory and action of the instruments of this class, which receive the general name of linear planimeters.

Passing on to the more important class of polar planimeters, we find that the first of any importance was invented by Prof. Amsler-Laffon, in 1856, and is known as the Amsler polar planimeter. This instrument is shown in Fig. 2, and consists of a radius bar a and a pole arm b jointed at C. The instrument is fixed at T by a needle point, on which is placed a weight W; the tracing point *p* is moved along the perimeter ABDE, and the roller *m*, which partly rolls and partly slips, gives the

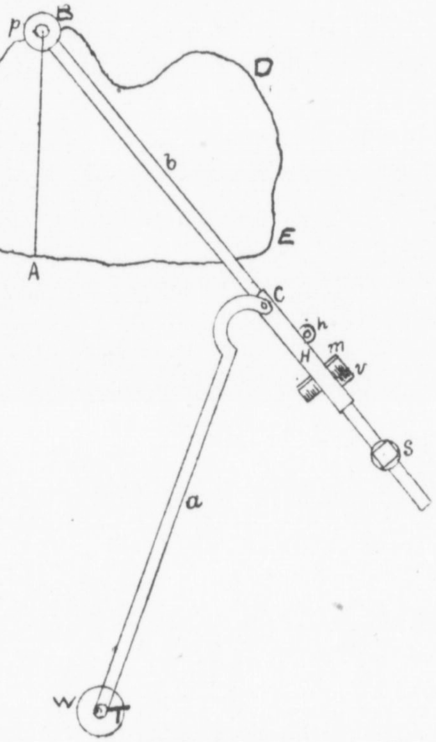

Fig. 2

area; by means of a dial *h* and a vernier *v* the result may be obtained to four places. The sleeve H can be placed in different positions along the pole arm *b* and fixed by a screw S, to give readings in different required units.

It is evident that when the plane of the wheel contains the point T the wheel *m* will not be turned as the pointer moves, since it is sliding in the direction of its axis; and the circle upon whose circumference the pointer moves under this condition is called the zero circle. The motion of the pointer *p* is made up of two motions, one the revolution of the radius bar *a* about the centre T, the other the revolution of the pole arm *b* about the centre C. In the latter motion, as the pointer *p* traverses the perimeter, the angle θ (Fig. 3) will have a series of values whose algebraic sum will be zero when the circuit is complete, or, in other words, the roller *m* will have made exactly as many revolutions in one direction as it has in the opposite direction. Hence this movement will have no effect upon the final record, and, theoretically, the arms *a* and *b* may be regarded as clamped firmly in one position.

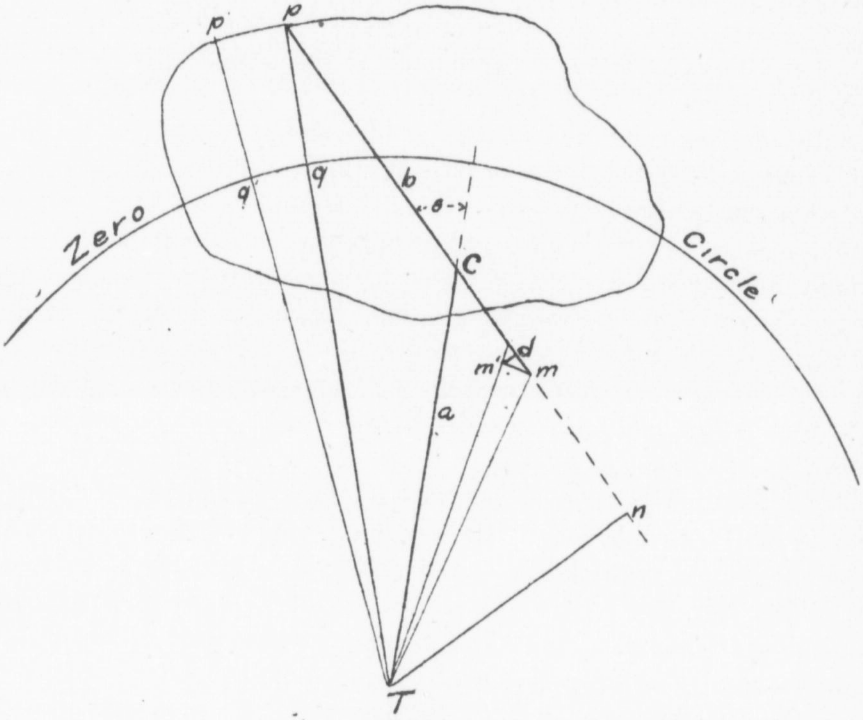

Fig. 3

Let the pointer traverse a small arc pp^{1}, which may be regarded as concentric with the zero circle, and let pp^{1} subtend a small angle w at the centre T. Then the roller m will move along the path mm^{1}, which also subtends an angle w at the centre T. Drop perpendiculars $m^{1}d$ and Tn from m^{1} and T on pn. Let the angle T$\mathrm{C}m = \theta$, $\mathrm{CT} = a$, $\mathrm{C}p = b$, $\mathrm{C}m = c$,

Then the area $pp^{1}q^{1}q = \frac{1}{2}w\ (a^{2} + b^{2} + 2\ ab \cos\ \theta\ ) - \frac{1}{2}\ w\ (a^{2} + b^{2} + 2\ cb) = wb\ (a \cos\theta\ - c)$, but b is a constant, and may be made equal to unity, hence area $pp^{1}q^{1}q = w\ (a \cos\ \theta\ - c)$.

Now, $mm^{1} = m\mathrm{T} \times w$ and $m^{1}d$, the record of the wheel is $= mm^{1} \times \cos\ (\pi\ - \mathrm{T}m\mathrm{C}) = w \times m\mathrm{T} \times \cos\ (\pi\ - \mathrm{T}m\mathrm{C})$. But $m\mathrm{T} \times\ \cos\ (\pi\ - \mathrm{T}m\mathrm{C}) = mn = a \cos\theta\ - \mathrm{c}\ \therefore m^{1}d = w(a \cos\theta\ - c)$.

Hence the wheel records the true area.

To show that proper summation is made on the wheel for the areas outside and inside the zero circle, Let A_0 = area generated by the line Tp, when the point p is outside the circle.

Let A_1 = area generated by the line Tp, when the point p is inside the circle.

Let S = area of sector between radii to points where the perimeter crosses the zero circle.

Let A = area of the figure.

Then $A_0 - S$ = outer area and $S - A_1$ = inner area.

The sum of these is $A = (A_0 - S) + (S - A_1) = A_0 - A_1$.

But since A_1 is recorded negatively on the wheel, while A_0 is recorded positively, the wheel record is $A_0 - (-A_1) = A_0 + A_1 = A$.

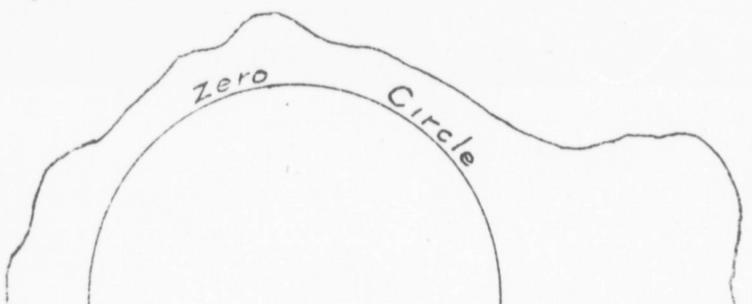

Fig. 4

We have until now discussed the case in which the point T falls without the figure, but, if we have a large area, we may be compelled to place the needle point T inside, as in Fig. 4. In the theory of the first case, the small area lying outside the zero circle was regarded as the difference between small sectors of the figure and of the zero circle; but in this latter case the sum of the sectors of the zero circle is equal to the area of that circle. Hence the record of the wheel is only the area lying between the zero circumference and the perimeter of the figures, and this leads us to an important rule. If the figure contains the needle point T, add the area of the zero circumference, which is $\pi\ (a^2 + b^2 + 2\ bc)$, to the wheel record.

In using the planimeter, the joints should be kept well oiled, the measuring wheel free from rust, and the whole instrument should be handled with care. When the sleeve has been adjusted on the pole arm for a certain unit, it is well to make a trial with a known area. If the value obtained be $\frac{1}{n}$th of the total area in error, the length of the pole arm may be changed in this ratio, or else the final result corrected in the same proportion. When practicable, it is best to have the needle point outside of the figure, since no correction is necessary. If it be placed inside, move the tracing point round the perimeter in the same direction as the hands of a watch. Then if the area be entirely within the zero circumference it will be negative, and since the area of the zero circle is positive the sum of the two will be the required area. Similarly, if the perimeter of the figure completely surrounds the zero circle, the area recorded will be positive, and will be added to the area of the circle. The value of b is known, and the values of a and c should be furnished by the maker. From these the area of the zero circle may be calculated according to the formula given above; or a given area may be measured with the middle point inside and outside, the difference of the two areas being the area of the zero circle.

Of other planimeters, depending on various principles, there is little to be said. Although more accurate than the two already described, they are very little used; in some cases only half a dozen instruments have ever been made. The two commonest, the suspended and the Coradi, are very accurate, but are also much more complicated in their mechanism and theory.

The planimeter may be applied in problems in surveying, in the calculation of earthworks, in the determination of horse power from indicator cards, and in many other ways which will occur to the reader.

For a comparatively complete treatment of the subject, "Mechanical Integrators," by Prof. S. H. Shaw, may be consulted; to him, and to Prof. J. B. Johnson, in his "Theory and Practice of Surveying," the writer is indebted for most of the theory given above.

Finally, in Williamson's "Integral Calculus," or in Minchin's "Uniplanar Kinematics," will be found solutions employing the calculus.

A BRIEF HISTORY OF ASTRONOMY

By W. L. Innes, C.E., O.L.S.

Astronomy is probably the most ancient of all sciences. Josephus assigns the founding of this science to the antediluvian patriarchs, although the honor is claimed respectively by the Chaldeans, Egyptians, Chinese, Indians, Gauls, and Peruvians.

According to the unanimous testimony of the Greek historians, the earliest traces of the science are to be found among the Chaldeans and Egyptians. Both the pastoral life of the people, the clear atmosphere and level country of Chaldea, were very favorable for the study of astronomy.

After a long series of observations of eclipses, the Chaldeans discovered the period Saros, a cycle of 223 lunations, or eighteen solar years, which brings back the moon to nearly the same position with respect to her nodes, perigee and the sun bringing back the eclipse in the same order. This enabled them to predict all eclipses included in the cycle, but involved neither theory nor science.

The ancient Egyptians rivalled the Chaldeans in the cultivation of astronomy, but have left very few traces behind them. The Greeks, acknowledging themselves indebted to the Egyptians for their science and civilization, have greatly exaggerated their advancement in this respect, placing them even ahead of the Chaldeans.

The Phœnicians, who certainly excelled at navigation, are also mentioned among the ancient nations who studied astronomy. It is not improbable that in their long voyages they would make use of the circumpolar stars, but their knowledge of astronomy was probably derived from the Chaldeans or Egyptians.

It would almost appear that the Chinese were the first to cultivate astronomy. Certainly, from the remotest ages, this science has been considered as inseparable from administration of the civil government.

The arrangement of the calendar induced the Chinese, as also the

Chaldeans and Egyptians, to study the motions of the heavenly bodies, and it is the boast of this exclusive people that the national record of eclipses extends over a period of 3858 years. Not only this, but that these eclipses were all previously calculated, as well as carefully observed.

The Emperor Fou-Hi, B.C. 2857, is said to have studied astronomy, and attempted to instruct his subjects in the science, but, due to their ignorance, he was obliged to content himself with giving them a rule for calculating time, by the combination of the numbers 10 and 12, which produces a cycle of sixty years—the standard from which they deduce the hours, days, and months.

B.C. 2608, Hoong-Ti built an observatory for the purpose of connecting the calendar, and appointed three sets of astronomers to observe the sun, moon, and stars respectively. They discovered that the twelve lunar months did not correspond exactly with a solar year, and that an intercalation of seven lunations in the space of nineteen years was required to restore the coincidence. From this it would appear that the Chinese discovered the metonic cycle two thousand years previous to the Greeks. It was in this reign that the mathematical tribunal was established for promoting astronomy and predicting eclipses, to which great importance has always been attached. Probably great honor was attached to a membership in this elaborate tribunal, though attended with considerable danger, as it was enacted that "whether the instant of the occurrence of any celestial phenomenon was erroneously assigned, or the phenomenon itself not foreseen and predicted, either negligence should be punished with death"—law to which two of the mathematicians of the empire subsequently fell victims.

From the time of Yao, B.C. 2317, the Chinese year consisted of 365¼ days. The exact value is 365.24222 days—certainly a remarkably close approximation, differing only by about eleven minutes. They also divided the circle into 365¼ degrees.

Another much more remarkable instance of a measurement of precision is to be found in the annals of Chinese astronomical history. In the reign of Tcheou-Kong, which commenced B.C. 1100, the obliquity of the ecliptic was found to be 23° 54′ 3.15″—a result which agrees perfectly with the theory of universal gravitation. It may be here remarked that from more recent observations upon the obliquity, it is found to be diminishing. At the present time its rate of decrease amounts to about 45½″ per century, which may be regarded as uniform for many centuries to come. This decrease will continue until it reaches the maximum of about 3°, after which it will slowly increase until it reaches the same maximum on the other side of the mean value.

About the same time the position of the winter solstice in the heavens was determined, and it corresponds to within a minute of a degree with the calculations of Laplace about A.D. 1775. This extraordinary conformity was considered by Laplace as an undoubted proof of the authenticity of their ancient observations.

It is unfortunate that the history of China prior to B.C. 722 is not very reliable. However, between that period and B.C. 400, Confucius reckons a series of thirty-six eclipses, thirty-one of which have been verified by modern astronomers. After this the decline of Chinese astronomy took place, due, it is said, to the barbarous policy of one of their emperors, who ordered all the books to be destroyed B.C. 221.

It will be observed that the early Chinese astronomy—outside of the practice of observations, which led to very little—amounted to nothing theoretically.

Fables are in existence showing the methods employed by the ancient inhabitants of India to predict eclipses and the position of planets, and indicating a high state of civilization. However, the difficulty arises in assigning the data of these and their origin. Some consider India as the cradle of all sciences, particularly that of astronomy; others think it was carried thither from Greece at the time Pythagoras travelled into that country; others, again, consider it was obtained from the Arabians in the ninth century of our era.

The Ionian school of science was founded in Greece, by Thalese, B.C. 640. He taught that the earth was the centre of the universe, was spherical, and divided it into five zones, by the Arctic and Antarctic circles and the two tropics. He also taught that the moon received its light from the sun, and that the stars were formed of fire.

Thalese was succeeded by Anaximander, who invented the gnomon or sun-dial, and with it observed the solstices and equinoxes. His greatest note is derived from inventing the geographic chart.

The succeeding leaders of this school appear to have added nothing of importance to the principles already taught.

While the Ionian sect was successfully employed in cultivating and propagating a knowledge of nature in Greece, another, still more celebrated, was founded in Italy by Pythagoras, who is said to have acquired, in Egypt, a knowledge of the obliquity of the ecliptic and the identity of the morning and evening stars.

He taught publicly that the earth was the centre of the planetary world, though, to his disciples, he taught that the earth and planets revolved round the sun.

Philolaus followed in this school. He supposed the son to be a disk of glass which reflected the light of the universe. He made the lunar month consist of 29½ days; the lunar year, 354 days; and the solar year, 365½ days.

Cicero credits Nicetas, the next leader of this school, with having maintained that the apparent motion of the stars arises from the diurnal motion of the earth about its axis.

The difficulty of reconciling the motions of the moon and sun was first overcome (at least, for a time) by the introduction of the metonic cycle, July 16th, 433 B.C., by Meton. It consisted of 125 full and 110 deficient months, which gives 6,940 days for the 235 lunations, and is nearly equal to 19 solar years. A deficient month consisted of 29 days, and a full month 30 days.

As already noted, it is not improbable that the Chinese discovered this cycle some two thousand years before.

About 370 B.C., Euduxus introduced the year of 365¼ days into Greece. He supposed the diameter of the sun to be nine times greater than that of the moon. He is noteworthy for having contempt for the Chaldean predictions, and contributing towards separating true astronomy from astrology. He supposed each planet occupied a particular part of the heavens, and that the path which it discovered is determined by the combined motions of several spheres performed in different directions. The sun and moon had each three spheres, one revolving round an axis which passed through the poles of the earth, and which occasions the diurnal motion; a second revolving round the poles of the ecliptic in a contrary direction, causing the annual and monthly revolutions; the third revolving in a direction perpendicular to the first, and causing the change in declination. Each of the planets had a fourth sphere, and, as new inequalities and motions were discovered, new spheres were added, until the machine became too complicated to be intelligible.

Eudemna was the first Greek to assign a rate to the obliquity of the ecliptic. His value was the side of a pentedecagon, or 24°.

ALEXANDRIAN SCHOOL.

Rythias, of Marseilles, about the time of Alexander the Great, determined the length of the solsticial shadow, in various countries, by means of the gnomon. The observation is interesting, as it confirms the successive diminution of the obliquity of the ecliptic. He was also the first who distinguished the climates by the different lengths of day and night.

Aristillies and Timocharis, B.C. 300, determined the positions of the principal stars of the zodiac, and this led to the important discovery of the procession of the equinoxes.

Aristarchus, the next of the Alexandrian astronomers, in his book, which has been preserved till to-day, describes an interesting method of determining the relative distance of the sun and moon. When precisely half the disk of the moon was illuminated, he measured the angle between it and the sun to be 87°, and concluded the sun to be between 18 and 19 times more distant than the moon. The theory of his method is perfectly correct, but the practical application very imperfect, on account of the difficulty to determine the exact instant when the half of the moon is illuminated. The true angle is about 87° 50′, and the true mean distance of the sun 383 times that of the moon. However inaccurate the measurement was, it served to greatly expand the ideas of the times, as the prevailing opinion was that the sun was only about three times more distant than the moon.

Another delicate and difficult observation, made by Aristarchus, was that of the sun's diameter. He determined it to be the 720th of the circumference of an inch, or thirty minutes, which is very near the truth.

Eratosthenes succeeded Aristarchus, and determined the obliquity of the ecliptic to be 23° 51′ 19.5″. This a very important observation, and confirms the gradual diminution of the obliquity, as indicated by theory.

This man is celebrated for having made the first attempt, on correct principles, to determine the magnitude of the earth. Syene was known to be on the same meridian as Alexandria, also exactly under the tropic ; for at the summer solstice the gnomon, or sun-dial, had no shadow, and the rays of the sun illuminated the bottom of a deep wall in that city. On the day of the solstice he measured the zenith distance of the sun at Alexandria to be 7° 12′, a fiftieth part of the circumference. The distance from Alexandria to Syene was 5,000 stadia ; therefore, the circumference of a great circle of the earth was 250,000 stadia, but, unfortunately, we cannot judge the accuracy of this rude determination of the sign of the earth, as the length of the stadia is unknown.

Astronomy, till the time of Hipparchus, who observed at Rhodes, consisted only of a knowledge of isolated facts. He reduced it to a system, commencing his brilliant career by verifying the determination of the obliquity of the ecliptic made by Eratosthenes.

He next determined the length of the tropical year to be 365 days, 5 hours, and 49 minutes, which is only twelve seconds greater than the truth.

By a careful observation of the solstices and equinoxes he discovered that the year is not divided at these points into four equal parts, the sun occupying 94½ days in passing from the vernal equinox to the summer solstice, and only 92½ from this point to the autumnal equinox. The sun, therefore, remains 187 days north of the equator, and only about 178 days south of it. This led Hipparchus to the great discovery of the eccentricity of the solar orbit. The discovery of the eccentricity led him to that of the inequality of the length of the solar day at different seasons of the year. The sun advances by his own proper motion to the east about one degree between two successive meridian transits, but the rate of motion is unequal, varying between fifty-seven and sixty-one minutes of a degree.

Hipparchus next directed his attention to the moon. From the comparison of a great number of the recorded eclipses by the Chaldeans, he determined the period of the moon's revolution relative to the stars, the sun, her nodes, and her apogee. These determinations are among the most valuable results of ancient astronomy, inasmuch as they corroborate the acceleration of the mean lunar motion, and thus form one of the most delicate tests of the truth of Newton's law of gravitation.

Hipparchus also determined the eccentricity of the lunar orbit, and its inclination to the plane of the ecliptic. His values, after making due allowance for slight changes which are taking place, correspond closely with modern observations. He approximated to the parallax of the moon, and concluded that the greatest and least distance of the moon are, respectively, 78 and 67 semi-diameters of the earth; also that the distance of the sun is 1,300 semi-diameters. The first determination exceeds the truth, but the second falls greatly short of it, being about one-half the true distance.

He catalogued 1,080 stars visible above his horizon, and was rewarded for his trouble by discovering the procession of the equinoxes, one of the fundamental elements of astronomy.

A comparison between his own observations and those of Aristillies and Timocharis, about 150 years before, showed that the first point of Aries had advanced two degrees, or at the rate of 48 seconds a year. Modern observations give the rate at 50.1 seconds annually.

He invented the planesphere, a method of representing the heavens on a plane surface.

He was the first who demonstrated the solution of rectilineal and spherical triangles, and constructed a table of chords, from which he derived nearly the same advantage as we do at present from a table of sines.

He also was the first to fix the position of planes on the earth's surface by means of their latitude and longitude, as well as the first to use the eclipses of the moon as a means of determining longitude.

Some three centuries elapsed after the death of Hipparchus before Ptolemy, A.D. 130, the first successor in the Alexandrian school worthy of the name, appeared.

He is particularly distinguished for having collected and arranged the ancient observations, although he is entitled to a high place among astronomers on account of his own discoveries. The most important is the evection of the moon. Hipparchus had discovered the first lunar inequality, and undertook a set of observations to determine its amount and law, but death put a stop to his labors, which were completed by Ptolemy, who discovered that the eccentricity of the lunar orbit is itself subject to an annual variation, depending on the motion of the line of apsides. The variation of the position of the apsides produced an inequality of the moon's motion in her quarters, which has been called the evection.

Ptolemy employed a very simple process for determining the moon's parallax. First, he found the latitude of a place a little south of Alexandria when he observed over the zenith of which the moon was found to pass when her north declination was the greatest possible. In this position the moon, observer, and centre of the earth are in the same straight line, and, consequently, she has no parallax. The obliquity of the ecliptic and the latitude of the place being known, the moon's greatest northern latitude was also determined. Second, he observed the moon's meridian altitude fifteen days subsequent to the first observation, when her southern latitude was necessarily the greatest possible. This observation gave the apparent altitude of the moon; but the greatest northern and southern declinations being supposed equal, her true altitude, as seen from the centre of the earth, was easily computed from the previous observation, and the difference between the true and apparent altitude gave the amount of the parallax.

Ptolemy confirmed the observations of Hipparchus relative to the motion of the stars in longitude, but diminished a quantity which Hipparchus had already estimated too low.

He is called the prince of astronomers, a title which may be given him from the universal and long-continued prevalence of his system, but to which he has no claim from the number or value of his own observations. A great deal, at least, of the information he has left behind is supposed to be computed from the tables of Hipparchus.

Albategni, born A.D. 850, appears to have been the next great astronomer. He was an Arabian, a Syrian prince, and resided at Kakka, in Mesopotamia, but many of his observations were made at Antioch.

He forms the link between the astronomers of Alexandria and of modern Europe. Having studied the methods practised by Ptolemy and the Greek astronomers, he began to observe, and soon found the places of many of the stars given in Ptolemy's tables to be considerably different from the actual situations.

He measured the rate of procession with greater accuracy than had been done before, and determined the eccentricity of the solar orbit. His value for the latter differs very little from modern observations. In determining the length of the year he made an error of more than two minutes.

He noticed that the sun's apogee advanced at a slow rate, and constructed a set of astronomical tables more accurate than those of Ptolemy.

Ibu-Junis, who lived in the beginning of the eleventh century, constructed a set of tables, and composed a history, in which he recorded numerous observations of his own and other astronomers of the same country. This work was translated in 1804, and contains twenty-eight observations of eclipses between 829 and 1004; seven of the equinoxes; one of the summer solstice; one of the obliquity of the ecliptic made at Damascus, which gave it to be 23° 35′; and likewise a portion of tables of the sun and moon, with other information.

In 977, 978, and 979 Ibu-Junis observed two eclipses of the sun and one of the moon near Cairo, which agree with theory in confirming the existence of an acceleration of the mean lunar motion.

A Tartar prince, Ulugh Begh, established at Samarcand, the capital of his dominions, an academy of astronomers, and furnished for their use the most magnificent instruments. By means of a sun-dial 180 feet high he determined the obliquity of the ecliptic to be 23° 30′ 20″, the procession of the equinoxes 1° in seventy years, and formed elements for constructing tables equal in accuracy to those of Tycho Brahe.

Hipparchus had the honor of constructing the first star table, but Ulugh Begh that of the second, after an interval of sixteen centuries. His death ended the last great astronomer of the East.

REVIVAL OF ASTRONOMY IN EUROPE.

During the life of George Purbach, born 1423, astronomy in Europe again began to flourish after a long standstill of ages. He became noted as a professor, and, though ignorant of both Greek and Arabic, translated, through his perfect acquaintance with the subject, Ptolemy's Almagest, correcting many errors in former translations. He found a disciple, John Muller, who executed many of the plans interrupted by his premature death.

John Muller, better known as Regiomontanus, studied under Purbach in Vienna for ten years, and after his master's death he moved to Rome, and translated into the Latin the works of Ptolemy and other treatises of ancient science. In 1471 he retired to Nuremberg, when, with the aid of a wealthy burgess, Bernard Walther, he founded an observatory, and equipped it with instruments principally of his own design, by means of which he detected many inaccuracies in the ancient tables. Walther continued to observe after the death of Regiomontanus for thirty years. His observations were collected and published for the first time in 1544. He introduced the use of clocks to measure time in celestial observations, and was the first to employ the planet Venus to determine the longitude of the stars.

We now arrive at the overthrow of the Ptolemaic system, and total renovation of the science of astronomy by Copernicus. His system simply supposed the heavens composed of stars perfectly at rest, occupying the remotest bounds of space, then the orbit of Saturn, next Jupiter, Mars, the Earth (accompanied by its moon), Venus, Mercury, and, lastly, the sun immovable at its centre. By this arrangement, the stations and retrogradations of the planets became simple mathematical corollaries,following from the difference of the radii of the orbits and the unequal motions. The diurnal motion of the earth explained more simply and rationally the apparent daily revolution of the heavens, and the procession of the equinoxes was referred to a small variation in the inclination of the earth's axis to the plane of the ecliptic. However, the simplicity of the system, and its consequent probability, was the only proof Copernicus could advance to substantiate its reality.

The subsequent discovery by Richer of the diminution of gravity towards the equator left no further room for doubt as to the earth's rotary motion. The measurement of the velocity of light by Roemer, and the phenomena of aberration observed by Bradley, also reduced to a certainty its annual revolution. Copernicus, after the example of the ancients, assumed the uniform circular motion of the planets, and to explain the observed variations he was obliged to suppose different centres for each orbit. The sun was placed inside them all, though at the centre of none, and, consequently, had nothing else to do but supply the planets with light and heat ; being excluded from any influence on the system, he became a stranger to all the motions.

Tyco Brahe is the next in order who contributed to the progress of astronomy. He is justly considered far superior to any of his predecessors (since the revival of astronomy in Europe) as an indefatigable and skilful observer.

He constructed the first table of refractions, and his solar tables were extremely accurate. He greatly improved the lunar tables, and detected inequalities in the moon's motion in longitude, which he named variation, and by which it still continues to be distinguished. He discovered an equation in latitude similar to evection, and fixed its amount accurately. He showed that the region of comets was far beyond the orbit of the moon, and determined the relation and absolute position of 777 stars with scrupulous exactness.

Tyco Brahe left a regular series of observations of the planets, which Kepler, his disciple, made use of to establish the system of Copernicus. Kepler may be said to have constructed the edifice of the universe from the material supplied by Tyco.

For some twenty long years Kepler labored under a mistaken idea that the ancients had not disputed, and which Tyco Brahe had accepted, namely, that the planets move in circles; but having computed with incredible labor the observations of seven oppositions of the planet Mars, he discovered that the motions could only be accurately represented by supposing it to move in an ellipse, having the sun in one of its focii.

He next proceeded to consider the angular motions of the planets, and concluded that the universally accepted uniform motion did not exist in nature. He perceived, however, that in equal times the radius vector described equal areas, and eventually extended this to include all planets.

It was some years later before he made what he considered the most important discovery. He found that the squares of the periodic times of the planets are always in the same proportion as the cubes of their mean distances from the sun. He showed it to be true of all the planets then known, and it has been found equally true in regard to those since discovered, and also to prevail in the systems of the satellites of Jupiter and Saturn. Indeed it is a necessary consequence of the law of gravitation.

The three laws known by Kepler's name are:

(1) Each planet moves around the sun in an ellipse, having the sun in one of its focii.

(2) The radius vector joining each planet with the sun moves over equal areas in equal times.

(3) The square of the time of revolution of each planet is proportional to the cube of its mean distance from the sun.

These brilliant discoveries reduced the solar system to that beautiful simplicity conceived by Copernicus, but from which he found it necessary to depart.

Kepler was the first to make a practical use of eclipses, by using one of the sun to determine a difference of meridians.

He promptly embraced the new invention by Lord Napier, of Scotland, of logarithms, and constructed a table from which the logarithms of the natural numbers, sines, and tangents could be taken at once. It may be here mentioned that without this or some other similar process the computations rendered necessary by more correct observations could not be made, and astronomy could not have acquired that precision and accuracy by which it is now distinguished. He passed some sound and accurate notions of the nature of gravity, but, unfortunately, conceived it to diminish simply in proportion to the distance, although he had shown the intensity of light to vary inversely as the square of the distance from the luminous body.

Contemporary with Kepler was the celebrated Galileo. While residing at Venice he heard it reported that a Dutch optician, Metius, had discovered a combination of lenses by means of which distant objects appeared nearer. This aroused his curiosity, and he immediately enquired into the reason. His researches were attended with prompt success. On the following day he had a telescope that magnified three times. His second telescope magnified about seven or eight times, and he was subsequently enabled to increase the magnifying power to thirty-two times.

Through his telescope he perceived numerous inequalities in the surface of the moon, from which he concluded, almost with certainty, that it was an opaque body reflecting the light of the sun. The planet Venus exhibited similar phases. He detected the four moons of Jupiter and spots on the sun, from which he deducted the rotation of that body on its axis in the space of twenty-seven days.

Science is indebted to Galileo for two still more important discoveries, namely, vibrations of the pendulum in equal times and the law of acceleration of falling bodies.

Horrox and his friend Crabtree were the first to observe, on the 24th of November, 1639, that rare phenomenon, a transit of Venus over the sun's disk.

Huyghens improved the telescope and introduced the pendulum to clocks. He discovered that the peculiar appearance of Saturn was due to a ring surrounding the body of the planet.

Picard, in 1667, applied to the telescopes cross wires in the focii, and micrometers to graduated instruments, and was thus enabled to observe the stars in the daytime.

Roemer, the friend and pupil of Picard, discovered the progressive motion of light in 1675, and measured its velocity by means of the eclipses of Jupiter's satellites, and was the first to erect a transit instrument.

The Royal Observatory of Paris was completed in 1670, and Dominic Cassini entrusted with its direction. He determined the motions of Jupiter's satellites from observations of the eclipses. He observed that Saturn's ring is double, and discovered four satellites of that planet. He also determined the rotation of Jupiter and Mars, and made a number of observations on Venus with the same view. He made a near approximation to the parallax of the sun, and was the first to calculate tables of refraction on correct principles. He left a complete theory of the vibration, or balancing, of the moon.

Newton discovered three laws of motion, which are:

(1) Every body preserves its state of rest, or of uniform motion, in a right line, unless it is compelled to change that state by forces impressed thereon.

(2) The alteration of motion is ever proportional to the moving force impressed, and is made in the direction of the right line in which that force acts.

(3) To every action there is always opposed an equal reaction; or the mutual actions of two bodies on each other are always equal, and in opposite directions.

Peterborough, October 15th, 1895.

STANDARDS IN MACHINE SHOP PRACTICE

H. V. HAIGHT, '96.

The present is, to a great extent, an age of standards. In every department of business, and of life, the tendency is to fix certain standards by which things shall be judged, measured, or valued, or to which, within certain limits, all similar things must conform. As this tendency is nowhere more evident than in the work in which we, as engineers, will soon be engaged, and, as the subject is of great importance, it becomes our duty, as engineers, to get acquainted with the present condition of affairs in this line. The subject is much too extensive to permit us to consider, even briefly, the great number of standards in connection with machine shop practice. About the best we can do is to describe the ultimate standards, and some of the more important and interesting practical standards.

ULTIMATE STANDARDS.

The ultimate standards, in scientific work, are those of length, mass, and time. From these we derive our standard units in light, heat, sound, electricity, mechanics, etc. In what is called the absolute, or C.G.S. system, for instance, the units of length, mass, and time are the centimetre, gram, and second. The unit of force, in this system, is defined to be that force which, acting on a gram of matter for one second, will produce in it a velocity of one centimetre per second. Similarly we may express other physical quantities in terms of length, mass, and time.

In the machine shop we are chiefly concerned with the standards of length. In whatever line we may attempt to establish a standard we are confronted with the problem of the ultimate standard of length. A standard screw thread, for instance, consists in a standard shape and standard dimensions. The shape may be defined without reference to any standard of length, but in order to describe the size we have to bring in the term inch, or millimetre, or some similar term. The question then arises, what is an inch, or, what is a millimetre?

You may think this a very simple question, and say that every one knows what an inch means. But take a couple of foot rules, a steel machinist's scale, and a boxwood engineer's scale, for instance, and compare their lengths. You will find that a difference between the two of one-twentieth of an inch in the foot is not uncommon. And if you were to put one of those scales in a comparator and measure the successive inches, you would find that no two of them were the same length. It is clear, then, that we must have some ultimate standard to which we can refer.

The chief considerations for a standard of length are that it shall be *definite*, *easily copied*, and *unchangeable*. It would be an advantage if we had some natural standard which would fulfil these conditions; but, so far, no satisfactory one has been found. Nearly all, if not all, of our standards of length were originally derived from some natural unit. These standards were mostly personal. We have, for instance, the digit or finger's breadth, the palm or hand's breadth, the span, the foot, the step, the double step or pace, the cubit or distance from the elbow to the extremity of the middle finger, the fathom, or greatest extension of the outstretched arms, and the gird, or yard, the distance around the girdle. In England there have been such *legalized* standards as the length of the arm of King Henry I., and the foot made up of "thirty-six barley-corns, round and dry, placed end to end." It is evident that none of these natural units fulfil the first and most essential condition of a standard—that of being definite. They were, without doubt, suited to the primitive times in which they were employed. Their chief use was in trade and commerce, where the fluctuations in prices were so great that a difference in the size of the measure was comparatively unimportant.

Various attempts have been made to establish a unit of measurement based upon an unchangeable natural unit. Many eminent philosophers, such as Huyghens in 1670, Picard in 1781, and Talleyrand in 1790, have proposed the length of the seconds pendulum. Other natural lengths proposed as standards were based upon the dimensions of the earth. Cassini, in 1718, proposed the $\frac{1}{6000}$ of a minute of a degree of a great circle, which would be nearly equal to a foot. The three units proposed to the Assembly of France as the basis of what is now known as the metric system were the length of the seconds pendulum, the ten-millionth part of a quarter of the equator, and the ten-millionth part of a quadrant of a meridian. The International Commission adopted the latter. More recently still another natural unit has been proposed. This is the wave length of monochromatic light.

These three bases of measurement, the length of the seconds pendulum, the dimensions of the earth, and the wave length of light, are believed to possess the necessary properties of being definite and unchangeable (though there is some doubt of the latter); but they are not easily copied. The earth seems too large, the wave length of light too small, and the length of the pendulum too difficult of accurate determination, to be used as an ultimate standard. We are thus forced to fall back on an arbitrary standard established by law.

THE YARD.

There are at present two chief standards of length, the British Imperial yard and the French Mètre des Archives. The yard is the standard of length in Great Britain and her colonies, and in the United States. In Russia the standard foot is the same length as the English foot; so that the foot is about the most largely used standard of length in the world. The present British standard yard is a solid square bar of Baily's metal, kept in the office of the Exchequer at Westminster. It is thirty-eight inches long and one inch square, and is composed of copper, 16 parts; tin, 2½ parts; zinc, 1 part. Near each end of the bar a hole is drilled partly through it, and in these holes gold plugs are inserted, whose polished surfaces are level with the axis of the bar. Across each plug is marked a fine line. The distance, at 62° Fah., between these lines constitutes the Imperial yard.

This standard was made legal in 1855. It was constructed to replace the old standard which was made by Bird in 1760, became the legal standard in 1824, and was destroyed at the burning of the Parliament buildings in 1834. After the destruction of the old standard an attempt was made to replace it from a measurement of the length of the seconds pendulum, the Act legalizing the old standard having made provision for its restoration by that means, if at any time it were injured or destroyed. But "it has been proved conclusively since the passage of the Act (legalizing the old standard) that there were errors in the determination of the specific gravity of the pendulum employed; the reduction to the sea level had been shown, by Dr. Young, to have been doubtful; the reduction for the weight of air was also proved erroneous; and Kater showed that sensible errors had been introduced in comparing the length of the pendulum with Shuckburgh's scale."*

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* "Standards of Length and their Practical Application." Published by Pratt & Whitney.

The attempt to restore the lost standard by reference to the pendulum was therefore abandoned, and it was finally replaced by the use of standards then in existence which had been compared with the original yard.

To guard against a repetition of this calamity, four parliamentary copies of the new standard were constructed. These are kept at different places in London. There were also forty other copies of the new standard made for distribution among the different governments. Only two of these are exactly standard at 62° Fah., and these are kept at the Royal Observatory. All of the others have a certain relation to Bronze 1, as the new standard is called, and, instead of giving their errors, the temperatures at which they are standard is given. Bronze 11 is in the possession of the United States, and is kept at the office of the Coast Survey, at Washington. When it was presented to the United States in 1856 it was stated to be standard at 61°.79 Fah., but comparisons made in 1878 have shown that it is eighty-eight millionths of an inch *shorter* than Bronze No. 1 at 62° Fah., and that it must therefore be considered standard at 62°.25 Fah.

THE METRE.

The other important standard referred to is the Mètre des Archives, the ultimate standard of the metric system of weights and measures. This great system, used now in more countries than any other system, and apparently destined to become universal, had its origin in France. Toward the end of the eighteenth century the greatest confusion existed in the weights and measures used in France. It is said that there were as many as a hundred different measures of area alone. Instead of attempting to reorganize the system, as was done in many other countries when their measures got confused, as the English measures were reorganized in 1824, for instance, the French determined to originate a new system for universal use. In 1790 the French Government asked the British Government to co-operate with them in establishing a uniform international system of metrology. The British Government rejected the proposal, but other governments were not so conservative, or else their measures were in a worse state than those of Great Britain, for the International Commission which met to consider the matter had representatives from Holland, Spain, Savoy, Tuscany, etc. The commission adopted the standard which had been proposed by Laplace and Lagrange, with the support of the principal mathematicians of that period, namely, that the unit of linear measurement should be the ten-millionth part of the earth's quadrant, the length to be determined by the measurement of an arc of 9° 40′ 24″, between Dunkirk and Barcelona. A trigonometrical measurement of the distance was accordingly made, Delambre having charge of the work for part of the distance, and Mechain for the other part. From the results of this measurement the length of the metre was deduced, and the Mètre des Archives constructed. This is an end measure standard, made of an alloy of platinum and iridium, containing 10 per cent. of the latter metal, and is standard at O°C. It has since been shown that there were errors in this work. In the report of the United States Coast Survey, the length of the quadrant of a meridian passing through Paris is given as 10,001,472.5 metres, which is probably very nearly correct. This makes the difference between the actual length of the metre and the intended length about fifteen-hundredths of a millimetre, or six-thousandths of an inch.

After all attempts to make the Mètre des Archives conform to some natural unit were abandoned, it was finally made the legal standard. In this respect, therefore, it is no better than the British yard. The decimal feature, however, and the practical identity of the litre and the cubic decimetre and of their volume in water with the kilogram, make the metric system of the greatest value. Scientists have been practically driven to use it for these reasons.

The metric system has been gradually making its way ever since its origin. The metre is now the standard of measurement in France and her colonies, Italy, Germany, Portugal, Norway, Sweden, Greece, Bulgaria, Roumania, Mexico, Brazil, Peru, New Granada, Uruguay, Chili, Venezuela, Argentine Confederation, and Japan, and it is practically the standard in British India, Austria, Bavaria, Wurtemburg, Baden, Hesse, Denmark, Turkey, and Switzerland. The metric system is legal in the United States, is used in the State of Utah, and its use is permissive in England, Canada, and Russia. It is coming into use for pharmaceutical and medical purposes, and will be used in the new edition of the British Pharmacopœia.

A great effort has been made this last year to have it made the legal standard in Great Britain. An interesting account of this is given in *Nature* of November 28th, 1895.

THE VALUE OF STANDARDS.

The great need of such accurately defined standards of length in shop work is in the making of interchangeable parts of machinery. The old method of manufacture was what might be called the individual method.

Each part of the machine was fitted into the place in which it belonged. This made it necessary for the one manufacturer to make the whole machine. The more modern method, applicable whenever a large number of machines, or parts, of the same kind are to be made, is to make the parts to standard dimensions by using standard gauges and tools. Then parts can be taken from each lot indiscriminately, and be put together to form the finished product.

Often, in fact, different parts of the machine are made by different manufacturers, and in different places. Now, in order that these parts shall fit one another, it is necessary that the different manufacturers should have a common standard. For instance, an inch should mean the same thing to the manufacturer of taps in Hartford that it does to the manufacturer of screws in Hamilton.

The chief advantages of this system of manufacture are as follows:

(1) *Lessened Cost of Production.*—It will be easily understood that a single detail of a machine, receiving the whole attention and application and capital of a manufacturer, and being produced in immense quantities, can be made more cheaply than the manufacturer of the machine could make that detail himself. Again, that time is saved which would be spent in fitting each piece to its place, in marking it, and in keeping it separate from similar pieces.

(2) *Facility of Repairs.*—A new part may be made at any time without reference to the machine or part which it is to fit. Parts may be kept on hand, so that very little time will be lost if anything breaks. Facility of repairs is especially necessary in railroad work, and the making of parts to standard form and dimensions becomes especially valuable.

The standard system of measurement and the interchangeability of parts is a necessity in our present system of manufacture. The first great application of this method was that made by Eli Whitney in applying it to the manufacture of small arms for the United States Government. In 1822 the Secretary of War admitted to Mr. Whitney that they were saving $25,000 a year, at the two public armories alone, by the use of his improvements. In later years the introduction of typewriting, typesetting, and cash registering machines, which required to be made in large quantities and sold at a low price, rapidly brought this system of manufacture into use. Another factor which has aided in the introduction of this system among workingmen in general is the perfection and low cost of small micrometer calipers. At present, parts of machines measure and are regularly duplicated in ten-thousandths of an inch. Very fine work is made even more accurate than this. As measuring tools, gauges, etc., must be more accurate than the finest work for which they are to be used, it will be seen that, in standard gauges, a very high degree of accuracy is necessary. Manufacturers of end measure pieces, for instance, guarantee them to be accurate within one fifty-thousandths of an inch.

STANDARD SCREW THREADS.

About the first and most important application of standards in machine work was to screws and screw threads. There was a time when every manufacturer made screws of whatever diameter, pitch, and shape of head happened to suit him. The disadvantage of special screw threads are feelingly described by a writer in the *American Machinist* (April 6, 1893). He says: "There lies before me a cam and its set-screw, the latter stripped. It is a set-screw, pure and simple, and has no other function in the economy of the machine than to hold that cam. The only peculiarities about it are that its head is ornamented thus, C37, and that it has thirty threads to the inch, and is $\frac{23}{64}''$ in diameter. The threads are useful, in a limited degree, for advancing the screw; the ornamentation for identification, and, in a major degree, for advancing the profits of the makers of the machine, for the catalogue, also before me, proves the identity of the fact by an excellent cut and the following text: 'Part C37; set-screw to cam C36, twenty-four cents.' Now, it is of no use to go anywhere else for that set-screw, for no sane hardware dealer keeps set-screws $1 \times \frac{23}{64} \times 30$, and the only thing to do is to wait for the part to come or bother to make one, if you have the facilities. I suppose it is a matter of ethics, rather than of mechanics, whether the makers of a machine have the right to subject their patrons to annoyance and extortion in order that their profits may be increased. If there were any good mechanical reason, I would not object to the thread, size, or price, but a $\frac{3}{8} \times 16$ screw, costing two cents, would be better for the place than the one adopted. I do not expect the breech-blocks of rifled ordnance, or the lead-screws of lathes, to be made to a standard size and kept on sale in common hardware stores; but I fail to see any good reason why so simple a thing as a set-screw should be anything but the standard size and thread."

THE WHITWORTH THREAD.

About the first standard screw thread adopted was the Whitworth or English standard. It is said that Sir Joseph Whitworth arrived at this standard by finding out the different sizes, shapes, and diameters in use at that time and taking the average. The shape of this thread is triangular, the angle between the threads being fifty-five degrees. One-sixth of the height of the triangle is cut off by an arc of a circle tangent to the sides. The diameters of the screws vary by 16ths of an inch, from $\frac{3}{16}''$ up to $\frac{1}{2}''$, and by 8ths for larger sizes. The relation between the diameter and the pitch is given approximately by the formula $p = .08d + .04$, where p = pitch and d = diameter of bolt, both in inches. The number of threads per inch $= n = \frac{1}{p}$; the nearest whole number of threads per inch being taken.

THE SHARP V THREAD.

On this side of the Atlantic the Whitworth standard has never come into general use. The chief forms used here are the sharp V and the United States standard. The sharp V was formerly almost universal, and is still used to a great extent, especially in Canada, where we do not seem to wish to follow either the English or the United States standards. The shape of the sharp V is theoretically an equilateral triangle, though in practice it is not usually brought up to a sharp edge, but left a little flat. It is not a standard thread in the sense that it has been established as such by any association or large firm, but, by common consent, manufacturers have adopted a table of diameters and pitches following those of the Whitworth standard, except for a few screws over $2\frac{1}{8}''$ in diameter. In addition to these regular sizes, there are a lot of taps and screws made with different pitches, mostly finer than the ordinary ones. Taps and dies are also made $\frac{1}{64}''$ and $\frac{1}{32}''$ oversizes for rough iron, it being the custom of manufacturers to roll iron oversize. The result of these special pitches and oversizes is that V-thread screws will not always interchange, a very serious defect.

THE UNITED STATES STANDARD THREAD.

In 1864 the Franklin Institute appointed a committee "to investigate the question of the proper system of screw threads, bolt heads, and nuts, to be recommended by the Institute for general adoption by American engineers." The committee recommended the system which had been designed by Mr. William Sellers. In this system the shape of the thread is an equilateral triangle with one-eighth of the height cut off by a line parallel to the base, giving a thread with a flat at top and bottom equal to one-eighth of the pitch. The relation between the pitch and the diameter is given by the formula $p = 0.24\sqrt{d + 0.625} - 0.175$.

While this equation gives a parabola, and that for the Whitworth thread gives a straight line, yet, within ordinary limits, there is hardly any difference. Between $d=\frac{1}{4}''$ and $d=1\frac{1}{2}''$ there is only one difference in pitch, that for the $\frac{1}{2}''$ screw, which has thirteen threads per inch in the United States standard, and twelve threads per inch in the Whitworth.

The report of the committee of the Franklin Institute recommending this system of screw threads discusses the question of a standard screw thread in such an able manner that any of you who have the opportunity should read the full report.*

Soon after the Sellers' system had been recommended by the Franklin Institute, it was adopted as the standard for the United States navy. Later, it was recommended by the Master Mechanics' Association for use in locomotive construction, and, in 1871, it was recommended by the Car Builders for use in cars. It has been adopted by a great number of railroads and large engineering firms, and is coming more and more into general use. In 1886 it was stated by the Pratt & Whitney Co., in an article puplished in the *Railroad Gazette*, that from eighty to ninety per cent. of the taps and dies sold in the United States by various dealers were of the United States standard.

Difficulties were, of course, encountered in establishing this system. A number of firms thought that the United States standard consisted merely in a certain relation of pitch to diameter, and did not make the thread the right shape. Also, as with sharp V threads, it became the custom to use taps and dies $\frac{1}{32}''$ to $\frac{1}{64}''$ oversize for rough iron. The Master Car-Builders' Association made a great effort to prevent such practices, and "to impress upon the minds of those who have control of such matters that three features are essential in the Seller's system :

"FIRST, *screws must have a given number of threads per inch.*

"SECOND, *the threads must be of the form and proportions designated.*

"THIRD, *the diameters of the screws must conform to the sizes specified.*"†

The matter of oversize iron and steel for bolts was found to be easily solved. On the Erie Railroad they simply issued an order : "All iron and steel for bolts shall be carefully inspected, to make sure that it does not run over or under size, and bars involving double cutting, or too small, shall be rejected." As soon as the manufacturers grasped the idea that the company wanted iron nearly the nominal size, there was no further difficulty. The Master Car-Builders' Association took the matter up, and, after consulting with different ironmakers, they established a limit of

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*The full text of the report will be found in "Standards of Length and their Practical Application," published by Pratt & Whitney Co., p. 78 ; and in "Modern Machine Shop Practice," by Joshua Rose.

†Report of the Committee of the Master Car-Builders' Association on Standard Screw Threads. "Standards of Length and their Practical Application," p. 61.

variation in size. The total variation is .010 inch for 1/4 inch iron (.005" above or below nominal size). The limit of variation increases .001" for each succeeding size of iron. The Pratt & Whitney Co. make a set of double-end caliper gauges to be used when inspecting iron for bolts. Iron which will go in the large end and not go in the small end is within the allowable limit of variation.

But a still greater difficulty was encountered in establishing an interchangeable system of screws and bolts. A couple of years after the Erie road adopted the Sellers' system, it was found that some nuts made at one shop would not fit bolts made at another. An investigation showed that the threads were not only different in diameter, but even in pitch and angle of thread. The company ordered the different shops to discontinue making their own taps and dies, and to buy them from manufacturers. It was found, however, that nuts made with taps bought from one manufacturer would not screw on the taps of another manufacturer. Another investigation was made, and it was found that the screw gauges used by different manufacturers did not agree with one another, nor with the standard gauges at the Brooklyn Navy Yard. The difference was not very great, but it was sufficient to prevent interchangeability. This need not be very much, for it is stated that a difference of .002" in the diameters of a bolt and a nut will make a very bad fit. It was thought that the difficulty arose from the fact that different manufacturers were using different standards of length. The Pratt & Whitney Co. undertook to test the matter. They procured what were supposed to be the most accurate standards in the country, and had them measured on the best measuring machines in existence. They found that no two of the standards measured alike on the same machine, and that no two machines would measure the same standard alike. As the matter was of great importance to the company in the manufacture of taps, dies, gauges, and other tools, they determined to obtain accurate copies of the standards of length, and to make a measuring machine to enable them to subdivide and compare these standards, and to test the accuracy of gauges, etc. The company secured the services of two men well fitted for this task, Prof. W. A. Rogers, connected with Cambridge Observatory, and Professor of Astronomy at Harvard College, whose name has long been associated with minute measurements, and Mr. George M. Bond, a graduate of the Stevens Institute. They designed and constructed a machine—the Rogers-Bond Comparator—to be used (1) to compare line measures of length with attested copies of the Imperial yard and the metre; (2) to subdivide these line measures; (3) to investigate and determine the errors, if any, of these subdivisions; and (4) to reduce these line measures to end measures for use in the workshop. With this comparator measurements may be made with *certainty* within $\frac{1}{100000}$ of an inch, and readings are in *millionths*. The company also had Prof. Rogers graduate for them five standard bars, as follows:

P. & W.$_1$ is a bar of Baily's metal, 41 inches long and 1 inch square. It is a yard measure, divided into feet, the graduations being on gold plugs at the bottom of wells sunk half an inch in the bar.

P. & W.$_2$ is also of Baily's metal, and is the same size as P. & W.$_1$ The graduations, however, are over platinum iridium plugs, sunk flush with the surface of the bar. It is both a yard and a metre standard.

P. & W.$_3$ is a bar of tempered steel, and is both a line and end measure, metre, and yard.

P. & W.$_4$ is a bar of annealed steel, and is of the same form and dimensions as P. & W.$_3$

P. & W.$_5$ is a working standard of tempered steel, six inches long and one-half inch square.

The graduations on these bars have been thoroughly investigated, and their errors determined, so that allowance may be made for them. P. & W.$_1$ was found to be 53 millionths of an inch longer than the Imperial yard at 62° Fah., while P. & W.$_2$ was 36 millionths of an inch shorter than the Imperial yard at that same temperature. This whole work took about five years. It was begun in 1881, and Prof. Rogers handed in his report in 1886.

Having obtained accurate measures of length, Pratt & Whitney proceeded to make screw gauges for the United States standard thread. By using these gauges in making taps or in testing taps bought from manufacturers, any firm will be able to secure that accuracy necessary for interchangeability.

As a result of the efforts of the Pratt & Whitney Co. in establishing the United States standard screw threads on a sound basis, and of the enterprise of large firms in adopting this system, it is now an accomplished fact that screws and bolts are interchangeable throughout the United States. There is still room for improvement, but chiefly in the line of adopting the United States standard and of discontinuing the use of oversize and odd pitch threads.

SMALL SCREWS.

The standard screw threads just described are for machine screws ¼″ or more in diameter. For smaller sizes there are not such well-defined standards. Machine screws smaller than ¼" generally have the ordinary 60° sharp V thread. The diameters in use here usually follow the American Screw Gauge, the sizes being expressed by numbers. No. 1 is .07100" in diameter, and the diameters increase by .01316" for each size up to No. 50, which is .71584" in diameter. Only the even numbers smaller than ¼" are generally used, though. Makers of screws give a choice of two or three pitches for each size, and makers of taps more. For instance, No. 4 machine screws are listed with 30, 32, 36, 40, 42, 44, and 48 threads per inch. There seems lately to be a tendency among screw manufacturers to adopt a standard pitch for the even numbers. It is called the American Screw Company's standard, and runs as follows: 2-56, 4-36, 6-32, 8-32, 10-24, 12-24, 14-20.

Makers of taps also have a list of sizes varying by 64ths of an inch from $\frac{1}{16}$" up to ¼", but they do not appear to be much used.

Carriage bolts and stove bolts have special forms of thread. Carriage bolt threads are rounded at top and bottom, and the sides are at a slight angle. Stove bolt threads are nearly square. Some firms are trying to establish a standard of form and dimensions, but it is difficult to determine just what these standards are.

PIPE AND PIPE THREADS.

There is a well-established standard of sizes and threads for wrought-iron pipe, for steam, water, and gas. The sizes of the pipes are strangely irregular, and threads are not much better, but they are definitely known and generally followed, so that we reap all the advantages of a standard. It is so very much better to have even a poor standard than to have none at all. The Briggs standard for pipe and threads was established in 1862 by Robert Briggs, C.E., when superintendent of the Pascal Iron Works, Philadelphia. The sizes were adopted in 1886 by the manufacturers of wrought-iron pipe and boiler tubes. The action of the pipe manufacturers was endorsed and adopted by the Manufacturers' Association of Brass and Iron, Steam, Gas and Water Works of the United States. The sizes are expressed according to the nominal internal diameter. The actual internal diameters are, with one exception, larger than the nominal size. For instance, a ¼-inch pipe is 0.364" internal diameter and 0.540" external diameter. The thread employed has an angle of 60°, and is slightly rounded off, both at top and bottom. The taper of the threaded end is 1 in 32 to the axis, or ¾" per foot in the diameter. The pitch varies roughly with the diameter, but without any regularity. Thus all pipes from 1" to 2" have 11½ threads per inch, while ½" pipe has 14 and ¼" pipe

18 threads per inch. The length of the threaded end, throughout which the thread continues perfect, is given by the following empirical formula: $l=(0.8\,D+4.8)\,\frac{1}{n}$, where D is the actual external diameter of the tube throughout its parallel length, and n is the number of threads per inch.

FRENCH SCREW THREADS.

The *American Machinist* of May 30th, 1895, contains an account of the recent introduction of a standard screw thread in France which presents some points of interest. A diagram is given, with curves showing the ratio of the pitch to the diameter in both the French and the United States standards. The agreement is remarkable, and shows that the preferred practice is about the same everywhere. The pitch of the French system seems, if anything, somewhat finer than our own. The pitches of the French threads are not expressed as so many threads per centimetre or other unit of measurement, but as so many millimetres per thread. The 6 mm. (about $\frac{1}{4}''$) screw, for instance, has a pitch of 1 mm., that is, it advances 1 mm. per turn. This is equivalent to about 25 threads per inch.

The Society for the Encouragement of National Industries has long been agitating for a standard thread in France, and at last the Minister of Marine has formally notified the society that he has decided to adopt, in principle, their system of screw threads. The shape of the thread is the same as in the United States standard, namely, an equilateral triangle with one-eighth of the height cut off. The diameter of the screw is to be measured after the truncation of the triangle, as in the United States system.

The system also provides for the clearance of the threads, for the greatest variation in size of the unthreaded portion of the bolt, for screws of intermediate sizes, when such may be necessary, and for the size of bolt heads and nuts. These latter are to be inscribed in a circle whose radius is equal to the diameter of the bolt.

They have an effectual way of introducing the system. The Minister of Marine has addressed letters to the vice-admirals, maritime prefects, and directors of establishments, enjoining the use of the new system; and, after January 1, 1896, orders for new machines of all kinds shall specify the use of the new system. This latter will compel manufacturers who wish to get contracts from the navy to use the new system.

BOLT-HEADS AND NUTS.

At the time at which the Franklin Institute recommended the Sellers thread, now known as the United States standard, they also recommended a standard size for square and hexagon bolt-heads and nuts. The words of the report are :

"The distance between the parallel sides of bolt-head and nut, for a rough bolt, shall be equal to one and one-half diameters of the bolt plus one-eighth of an inch. The thickness of the heads for a rough bolt shall be equal to one-half the distance between its parallel sides, and the thickness of the nut shall be equal to the diameter of the bolt. The thickness of the head, for a finished bolt, shall be equal to the thickness of the nut. The distance between the parallel sides of bolt-head and nut, and the thickness of the nut, shall be one-sixteenth of an inch less for finished work than for rough."

These proportions are not followed very closely by makers of bolts and nuts. In five catalogues which the writer examined which contained tables of sizes of bolt-heads and nuts, not only did the tables in different catalogues differ from one another, but even in the same catalogue there were different tables of sizes. Some of these sizes were larger and some smaller than the standard, and none of them seemed to follow any rule. This standard is not of such great importance as some others, for the chief advantage of a standard size for bolt-heads and nuts is in being able to use a set-wrench. The lack of a recognized standard necessitates either a greater variety of wrenches or the use of an adjustable wrench.

WIRE AND SHEET METAL GAUGES.

This is decidedly a case where the matter of standards has been overdone. One would think that a standard of dimensions for wire and sheet metal would be a good thing, but when every manufacturer and association of mechanics think the same, and each gets out a different gauge, the result is quite confusing. It seems to have been the practice for each manufacturer of wire or sheet metal to get out a gauge suited to his business. Don't think that he sat down and wrote out in decimals of an inch the sizes he wanted, but rather that he had a steel gauge made with notches in the edge, representing the sizes he manufactured, and that he then numbered these notches consecutively in any way it happened to suit him. In some gauges the numbers get larger as the wire gets larger or the metal thicker, while other gauges are figured in the reverse order. The number of different gauges is surprising. In 1879 Hughes enumerated no less than 55 gauges, 45 of which were for measuring or determining sizes of wire used in Great Britain.

These gauges have two chief defects. They are indefinite and inaccurate. Some one has said that they have only one fault—their existence—but we may specify more particularly. They are indefinite because of their large number. No. 16 wire means practically nothing, because there are over a dozen different wire gauges in common use. No. 16 may be almost any size from .0359″ to .28150 in diameter, that is, from about $\frac{1}{32}$ of an inch to over $\frac{1}{4}$ of an inch. But even when we specify the gauge, which we must do *very* carefully, there is still a chance of inaccuracy. The notches in these gauges will certainly wear with continued use, so that even if the gauge were correct when new it would not be so long. The result is that even No. 16, by a specified gauge, is indefinite. This latter defect has been to a great extent eliminated by determining the sizes of the different numbers in decimals of an inch, and by the use of micrometer calipers in measuring wire. This makes the Imperial yard, instead of Tom Jones' gauge, the ultimate standard.

An agitation has been going on for the last twenty years or more to induce the public to abandon the use of fixed gauges and numbers, and to measure and designate wire and sheet metal by its diameter or thickness in thousandths of an inch. The difficulty of using the ordinary gauges is well described in a circular issued, in 1876, by Messrs. Miller, Metcalf & Parkin, Pittsburg:

"How is it possible for a roller to know just how many millionths of an inch another man, whom he never saw, means when he says No. 28 'full' or No. 27 'easy'? And how is he to guess how many thousandths of an inch the other man's gauge is wrong in its make, or how many hundredths it has worn in years of steady use? There is a simple and easy way out of this whole snarl, and that is to abandon fixed gauges and numbers altogether."*

More recently (in December, 1894) the committee of the American Society of Mechanical Engineers on Standard Gauges for Thickness reported in favor of abolishing the system of fixed gauges and of substituting the use of the thousandth of an inch for measuring and designating wire, sheet metal, etc. Several members of the committee, and members of English engineering societies with whom they corresponded, favored the hundredth of a millimetre as a basis, but it was not thought wise to advocate it at present.

Part of the report reads as follows: "Resolved, that we, the members of the joint committee of the American Society of Mechanical Engineers and American Railway Master Mechanics' Association, earnestly deprecate the use of any of the numerous wire and sheet metal or other

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*Catalogue of Brown & Sharpe Manufacturing Co., p. 361 (1895).

trade gauges now in vogue, and strongly urge the use of thousandths of an inch for all kinds and classes of small measurements.

"In practice we recommend the use of micrometer callipers or notched gauges, the latter with notches of dimensions suited to the convenience of different industries, and, where necessary, different selections of sizes in thousandths of an inch, suited to each trade, being incorporated in their working gauges; provided, however, that they are always dimensioned in thousandths of an inch, and marked in terms thereof, the number of thousandths being marked opposite each gauge notch thus, .001".

"We further recommend that the members of the various engineering societies assist the introduction and general adoption of this system by using it in their own work."

One would think that such efforts would have a tendency to kill off some of the numerous gauges, or, at least, to prevent the origination of new ones. This does not seem to be the case, however. For instance, the Standards Department of the British Board of Trade issued a new table of sizes for wire, to be the legal standard after March 1, 1884. It is not the actual standard, however, so that it merely adds one more to the list. More lately still, in 1893, the United States Congress passed a bill authorizing a new gauge as the only legal gauge for plate iron and steel. This gauge has one or two minor advantages, but otherwise it is even a little worse than the worst of them.

We seem, then, to be about as far as ever from abolishing fixed gauges. This much we can do, however, in ordering or giving the size of wire or sheet metal: let us always give the diameter or thickness in thousandths of an inch, instead of by gauge number. This will not only advance the cause of science, by rendering arbitrary gauges useless, but it will serve to protect those who practise it from annoyance and loss, as numerous mistakes arise from a misunderstanding of the size intended, when it is specified by a gauge number.

We have considered but a very few of the standards in use, but you will readily understand how impossible it would be to adequately treat of the subject in a short paper, even were the writer qualified to do it justice. At present, most of the information on this subject is scattered through numberless machine shops, periodicals, catalogues, pamphlets, books on machine design, etc., which, if collected and put in book form, would doubtless prove of great interest and value.

For the convenience of any who may wish to refer to tables of dimensions of different standards, a list of references is appended. As far as possible, these references are to catalogues, as these are cheap and easily obtained.

Catalogue of Brown & Sharpe, Providence, R.I.
Wire Gauge, American, or Brown & Sharpe.
“ Birmingham, or Stubs'.
“ Washburn & Moen Manufacturing Company's.
“ Trenton Iron Company's.
“ Stubs' Steel.
“ Twist Drill and Steel (actual average sizes of Stubs' Steel Wire).
“ Steel Music.
American Screw Gauge.
United States Standard for Iron and Steel Plate.
“ “ Screw Thread.
Standard Gears.
“ T Slot Cutters.

Catalogue of Pratt & Whitney Co., Hartford, Conn.
Pipe and Pipe-Threads, Briggs' Standard.
Limit Gauges for Round Iron.
Standard Taper Pin Reamers.

Catalogue of the J. M. Carpenter Tap and Die Company, Pawtucket, R.I.
Pitches for United States Standard, Sharp V, and Whitworth Threads.
“ for Machine Screws (American Screw Company's Standard).
“ for Stove Balt Taps “ “ “
Drill List for Machine Screw Taps.

Catalogue of Rice Lewis & Son, Limited, Toronto, Canada.
Imperial Standard (New Birmingham) Wire Gauge.
Gauge for Russia Sheet Iron.
Weight of Round, Square, and Flat Bar Iron.

Catalogue of Rhode Island Tool Company, Providence, R.I.
Square and Hexagon Nuts, United States Standard Sizes.
“ “ Additional Sizes.
Washers, Standard Sizes.
“ Additional Sizes.

Catalogue of Morse Twist, Drill, and Machine Co., New Bedford, Mass.
Tap Drills for Sharp V, United States Standard and Whitworth Threads.

Catalogue of Chas. H. Besley & Co., Chicago, Ill.
London Wire Gauge.
Brass Tubing, Sizes and Weights.

Modern Machine Shop Practice, Joshua Rose.
Gauge for Russia Sheet Iron.
American Zinc Gauge.
Belgian Zinc Gauge.
Birmingham Gauge for Rolled Sheet Silver and Gold.

THE CHICAGO CANAL, AND SOME SANITARY PROBLEMS CONNECTED THEREWITH

BY DR. P. H. BRYCE, M.A., Toronto.

Probably most people know the character of the work on the Chicago Canal from an engineer's standpoint better than I do, but you may not have learned the reasons why it has been undertaken as a sanitary work. As you are aware, Lake Michigan has a height of 579.60 feet above sea level at Sandy Hook, and is some two feet higher than Lake Huron. The waters of the lake at Chicago are separated from those of the Des Plaines River, which flows into the Mississippi River, by some two miles only of a watershed. The elevation of this land between the two greatest water systems of the continent is only some six or eight feet, and is, indeed, so little that in the construction of the canal a spill-way, or the river diversion works, has had to be planned, whereby the lower portion of the valley of the Des Plaines will, during flood time, be relieved of the flood waters in the upper reaches of the river, when they exceed 600,000 feet per minute, by their being turned into the lake through this aqueduct, some twelve miles long and two hundred feet wide, and having a fall of six feet to the mile.

The little stream called the Chicago River, at its nearest point, only two miles from the Des Plaines River, empties into Lake Michigan at the south end of the lake, and has become naturally the great central sewer of the city; while Lake Michigan, by nature a beautiful clear blue water, is as naturally the source of the Chicago water supply. Thus most of the broad facts of our lesson are before you. The city of Chicago, as you are aware, has had a remarkable history of growth and development. Fifty years ago it was a great marshy tract at the head of the lake, with only a few Indian huts here and there.

In 1870 its population was........ 298,000, say 300,000
" 1880 " " .... 593,000, an increase of 66 per cent.
" 1890 " " ....1,100,000, " " 120 "

With the invariable history of the cities on our inland waters, Chicago began years ago to find, as her population increased, that the short intake pipes laid into Lake Michigan were supplying other materials besides the pure lake water; and so we have the history of, first, the extension of pipes into the lake, and, second, the construction of a tunnel with the hope of obtaining a water taken beyond the point of pollution of the waters with sewage; but all was in vain.

Then a scheme was thought of, which was to divert the sewage by pumping it over the watershed out of the Chicago River and into the Des Plaines valley. This accomplished some good, but it was found that it only required a heavy rain to fill up the sewers, when the river became filled, much beyond the capacity of the pumps, and the black stream began to pour again into Lake Michigan. Thus it was soon felt that no mere temporary expedients could avail, and the Chicago engineers sought some permanent remedy. This was first in the shape of a great aqueduct out some four miles into the lake, and that the case was urgent is seen in the fact that in 1891 there were recorded some 1,700 deaths from typhoid in a city of 1,000,000 or so inhabitants. This tunnel was completed in 1893; but with six other pumping stations, with local sewers at no great distance from their intakes, only partial improvement was possible.

Along with this tunnel scheme was conceived the idea of constructing a great sewer to the Mississippi, which would flow at all times. This is being rapidly constructed. It is to have an ultimate maximum capacity of 600,000 cubic feet per minute, with a depth of from 22 to 35 feet. Its uniform width in the rock section is 162 feet, and will be 200 feet in the earth portions when completed. There can be no doubt of its being a great and costly work, but, as is apparent, it is simply the cutting of a great canal through clay, black loam, and an easily worked limestone. The State of Illinois, by legislation in 1889, provided for the incorporation of the Sanitary District of Chicago, and granted a permit for the work to be carried on, having a beautiful and sublime disregard for any one but the good people of Chicago, who, getting tired of drinking their own sewage, proposed to supply it to all the dwellers along the Father of Waters down to its mouth.

In return for any incidental inconvenience from this source, they said, in effect, to said dwellers on the Illinois and Mississippi, "We will supply you with a foot more water to float boats in and improve your commerce"; and to the people living in the other cities and towns, and who go to the sea in ships and do business on the great lake waters, and who are suffering from chronic low water, at the Limestone crossway, in the Detroit River, and elsewhere, "Don't be disturbed if we rob you of the one-twenty-fifth of the total water which flows down the Detroit River, since, at any rate, the Chicago people will be happy and healthy."

Setting aside, however, all but the sanitary problems attaching to this great work, we can see in them the following immediate effects :

(1) The great sanitary improvement of Chicago, if the lake is freed from sewage pollution and pure water is supplied to the citizens.

(2) A certainty of the pollution of the Illinois River, already bad, even at Joilette, some thirty miles down, owing to the sewage from the present pumping works.

(3) An immediate and definite lessening of the volume of water which receives the sewage of the towns along the St. Clair, Detroit, and Niagara Rivers.

It is of interest for us to estimate what the pollution of the river at Joliette will mean, and we can understand this by estimating the degree of dilution of the sewage by the waters which will flow through the canal from the lake. As already stated, the estimated flow through the rock section at its maximum capacity will be 600,000 cubic feet per minute, or 864,000,000 per diem. It is calculated that this will be realized with a population of 2,000,000. Assuming this population, and a flow of one hundred gallons per head of sewage in the canal, we find that 200,000,000 gallons of sewage per diem equals a flow, including the extra amount from rainfall on the streets, of one hour's flow of the canal. In other words, the dilution of the sewage will be 1 in 24 parts. If we compare this with other cities, we find that Pettenkofer states that in the Isar, at Munich, the sewage is 25 per cent. of the river flow, and considers this a sufficient dilution to remove all nuisance. Stearns, the engineer of the State Board of Massachusetts, does not consider that 1 in 40, or even 1 in 120, can be accepted as a standard under all circumstances at which rivers may receive sewage pollution. This is in the matter of the creation of a nuisance only, and is not intended to refer to the potability of a river water for a public supply. It is quite apparent, therefore, that the pollution of the Illinois River, in its upper portion, at any rate, will be beyond redemption ; and, as has already happened, we may expect the question of damages to towns below to crop up at every stage.

As compared with such pollution, that of the St. Clair, the Detroit, or the Niagara, might be described as accidental ; but we already know that the dilution in eighteen miles from Buffalo to Niagara Falls, or the diffusion in five-eighths of a mile between Walkerville and Windsor intake, roughly calculated as 600 times, has not removed the danger from sewage, if it contain the germs of typhoid fever.

The case of Chicago, situated on a lake, in a position similar to Cleveland, Toronto, etc., is an interesting and instructive one, since it supplies us with a sequence of conditions which have in a smaller degree been realized by all lakeside cities and towns between Chicago and Kingston. The manifest first source of danger is the increase in the population of these cities. The cause of this is a natural one, since in 1890 our great lake system of waters carried 20 per cent. of the tonnage of the water-borne freight of the United States, and in 1893 26½ million tons passed through the Detroit River. But more than this, we find that over 90 per cent. of the cities of over 10,000 in the United States and Canada are situated on navigable waters.

Thus those on the Great Lakes increased in the years between the census of 1880 and of 1890 as follows:—Wisconsin, 43 per cent.; Minnesota (Duluth), 38.56 per cent.; Michigan, 73 per cent.; Ohio, 40 per cent.; Pennsylvania, 46 per cent.; New York, 37 per cent.; Indiana, 46 per cent. Michigan (those on Detroit River), 83 per cent. New York State (on Niagara River), 63 per cent. Canadian side (on Detroit and Niagara Rivers), 38 per cent.

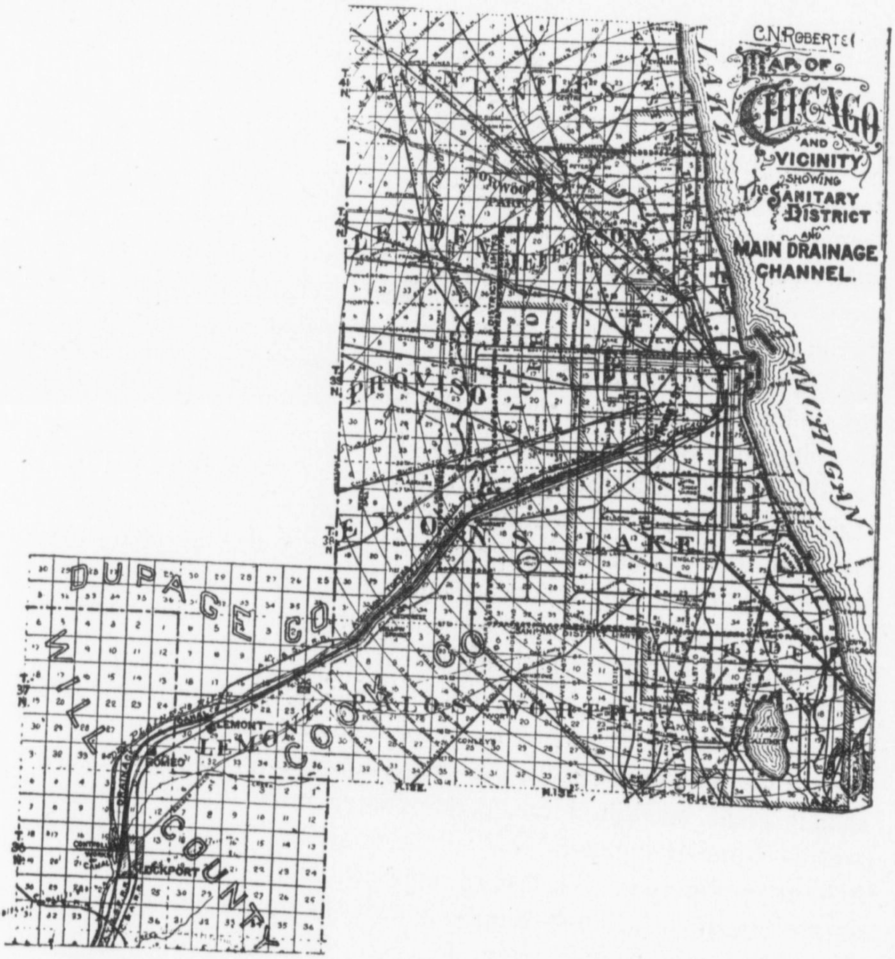
We therefore find that as municipal operations are always behind public needs, we have constantly public water supplies deficient in quantity, and very often in quality. And that, in addition to this, there is invariably a pollution of even large bodies of water, which, in spite of oxidation and the decomposition and nitrification of the organic matters in sewage, establishes a factor of permanent pollution in the lake waters near such cities.

These two results of city building are constants, and to engineers they supply the most ample field for the exercise of their talents in making the best use of present methods, and of developing others for the effective dealing with the difficult problems which are constantly cropping up in regard to water supply and sewage disposal.

In a paper read before the American Public Health Association, recently, in Denver, I illustrated by these facts the need of International Rivers Conservancy Boards to prevent notably the evils arising from polluted water supplies, but also those political dangers so readily created and solved only with such great difficulty.

The directions in which we have to look for dealing effectively with these two conditions are, of necessity, two—and, broadly, two only. Which shall we put first?

It would be largely true that, if we could supply unpolluted water to our cities, we need not be very anxious as to whether a few hundred thousand gallons, more or less, of sewage were carried into our great lakes and rivers. But even then it is found, as in Glasgow, that tidal streams may attain to a degree of foulness which makes them sources of nuisance



and positive danger for the neighborhoods through which they pass. I anticipate such will be found in the instance of the Chicago Canal. But can we afford to depend upon our water supplies without any attempts to deal with sewage as well? In the instance of Chicago, artesian waters have been found available for some of the towns and cities along the Illinois River; others have or will be forced to put in filtration works, which may or may not purify. It seems quite possible, however, to supply these towns by a common water pipe carried from Lake Michigan, but it will be expensive. We may then consider whether it would have been possible to deal with Chicago sewage in any other way than that proposed.

At Pullman, 40 miles away, the sewage has for years been purified by irrigating the land; could Chicago have done the same? There were, so it was stated by statistics, during the winter of 1894, 150,000 people unemployed. Everywhere outside of the city, and notably in the valley of the Des Plaines, great tracts of prairie land are lying uncultivated or nearly so. Is it not a practical question to discuss whether, with some of the engineering skill utilized in making the canal, a system of dealing with sewage by a sewage farm, as at Berlin, Germany, could not have been instituted, which by irrigation would have given employment and homes to many thousands of people now supported by charity, or, for public safety, by the municipality? The utilization of the waste organic products of cities has become the characteristic work of the great municipal engineers of Britain and Germany, and their labors have made of them philanthropists to a degree which, perhaps, has not to any other class been possible. Should we apply it to the case of Toronto, what would be possible? The utilization of land which bountiful Nature seems to have supplied especially for our needs! Hundreds of acres are lying around Ashbridge's Bay, and acres more forming every year, and demanding our attention. Our beautiful bay is increasingly a cesspool. Surely, when we have thousands of unemployed, permanent employment to hundreds can be given by allowing them to labor at the work of cultivation on a great sewage farm. This is one amongst many of our unsolved municipal problems.

In the other work of the purification of water, we have in Ontario an equally great need, and are, perhaps, at a more doubtful stage. The principles underlying each work are the same, but the reasons urging work forward are different. Each town hopes to be able to dispose of its own sewage cheaply, by simply turning it into the nearest stream, but is often prepared to spend much to purify water polluted by another town. It seems very clear, then, with these facts before us, that as sanitary engineers our energies will be directed with the greatest effect towards designing schemes comprehensive in character, and which, like township drains and irrigation works, are instituted on the assumption that the common need, and not that of some individual, or individual town or city, has interests apart from those of the general community. With the cultivation of such a spirit, it becomes easy to see how, not only intermunicipal and interstate interests may be provided, but also how the questions of international importance will be found of easy and simple solution.

DOUBLE TRACK TUNNEL, HAMILTON—TORONTO, HAMILTON & BUFFALO RAILWAY

By J. Chalmers, Grad. S.P.S.

Before taking up the construction of the tunnel, it will be well to give a short description of the road and its construction.

The Toronto, Hamilton & Buffalo Railway is a new railroad system, part of which is now in operation. Beginning at Waterford, on the Michigan Central Railway, the road extends eastward through Brantford to Hamilton, where the centre of the system is located. From Hamilton it continues in a southeasterly direction, until it again joins the Michigan Central at Welland, on which road running powers are granted into Buffalo. The part yet to be built is the connection between Hamilton and Toronto.

Up to the year 1894, Hamilton had but one outlet by rail—the Grand Trunk Railway. For a city of 40,000 inhabitants this was considered a hindrance to its growth and prosperity—competition, the life of trade, applying to railways as to other departments of commerce.

About this time the Toronto, Hamilton & Buffalo Railway Company offered to build and operate a first-class road between Waterford and Welland, passing through Brantford and Hamilton, making the latter city the centre of the system, if Hamilton would grant assistance in the way of a bonus. In October, 1894, the city voted to the road the sum of $225,000, on condition that the said company would build and have in operation by December 31st, 1895, a road as described above. In addition, a franchise was granted giving the company a right of way along one of the streets.

Previous to this, the Toronto, Hamilton & Buffalo Railway Company had purchased the Brantford, Waterloo & Lake Erie road, running between Waterford and Brantford, which road now forms part of the present system. Work was begun between Brantford and Hamilton soon after the by-law was passed granting the bonus, and was vigorously pushed forward unto completion.

On the 24th of May, 1895, the road was opened for traffic between Hamilton and Waterford, the first passenger train conveying the 13th Battalion through to London. On the Welland branch location was begun in March of the same year, in July construction commenced all along the division, and by the end of December the grading was completed, the track laid, and a train service between Hamilton and Buffalo.

The location of Hamilton, surrounded, as it is, by the Niagara escarpment, having here an altitude of 600 feet above Lake Ontario, makes access by rail difficult from either the south or west. As both terminals of the system lie on top of the escarpment, it was necessary to overcome this altitude on both the Brantford and Welland divisions. On the Brantford division the line begins to ascend the escarpment about four miles west of Hamilton, and reaches the summit six miles further on. The maximum grade on this division is 1.5 per cent., while the maximum curve is 6 degrees.

From Hamilton to the summit the grading was heavy, the cuts being mostly hard clay, with a small percentage of rock. From the summit on to Brantford the work was comparatively light, with the exception of a large cut at Cainsville, where the line passes under the Grand Trunk.

On the Welland division the line leaves Hamilton from the east, and follows the foot of the escarpment for about three miles, where it turns southward ; leaving the escarpment and crossing a slightly rolling country, it strikes the escarpment again seven miles from the city. From here it ascends about uniformly to the summit, five miles further on, the maximum grade being 1.04 per cent., compensated for curvature (ratio 0.045 feet per degree of curve). From the summit the line takes a southeasterly direction toward Welland, and, with the exception of two small curves, is carried through on a tangent to where it joins the Michigan Central Railway. The country here is flat and almost clear of wood, giving easy grades and light work on construction. The heaviest cutting was ascending the escarpment, in which lime and sandstone rock was encountered, the former in beds varying from three to sixteen inches.

The bridges on this division are steel, and, with the exception of one over the Welland River, are of the plate girder type, the other being a pin-connected truss of 150 feet span. The width of the roadbed is 22 feet in cutting, and 17 feet 6 inches on fills. The track is laid with 3,000 ties to the mile, and 80-pound rails used, fastened with suspended joints connected by four-bolt fish plates. When the Toronto branch, now under consideration, is complete, a through connection will be formed between the Canada Pacific Railway, at Toronto, and the Michigan Central, at

Welland and Waterford, making direct connection with all eastern and western cities, and placing Buffalo within two hours' ride of Toronto.

The heaviest work was encountered in Hamilton, where the road crosses a ridge or hogsback, which extends from the foot of the escarpment, across the centre of the city to the lake, through which it was necessary to tunnel. Through the city the road is double-tracked. Considering James street, where the central station is located, the central point of the system, the track follows eastward along the north side of Hunter street, the use of which was granted to the company for a distance of 800 feet. Here it crosses the street and continues in a southeasterly direction to the foot of the escarpment, there crossing the Hamilton & North-Western Division of the Grand Trunk on a level crossing. From there the line passes eastward along the foot of the escarpment to the city limit. East of James street all streets are crossed by level crossings, which are protected by gates operated from elevated signal towers over the tracks. Electric bells are placed in each tower, and connections made with the track on either side, at a sufficient distance away to give the signalman timely warning of the approach of a train.

TUNNEL AND APPROACHES.

West from James street, the line continues along the north side of Hunter street, to where it enters the east approach of the tunnel. One of the main street car lines crosses on James street, and 400 feet further west the Hamilton and Dundas suburban line crosses at McNab street. In addition to gates, these crossings are protected by derailing switches, and all trains are required to come to a full stop before crossing.

The east approach of the tunnel commences at McNab street, and is 400 feet in length, the cut gradually increasing in depth, until at Park street, where the tunnel begins, a depth of 13 feet is reached. Charles street, which crosses about the middle of the approach, is lowered about five feet to the level of the track, and protected by gates similar to the other streets.

Before entering the tunnel the line is thrown over on a five-degree reverse curve, to coincide and be parallel with the centre line of Hunter street, the tangent of which it follows for a distance of 3,200 feet, to the end of the west approach—the junction of the Brantford and Toronto divisions, the former diverging to the left on a six-degree curve, and the latter on a five-degree curve to the right.

The tunnel, known as an open-cut double-track tunnel, is located along Hunter street; the extreme length from outside face of portals is

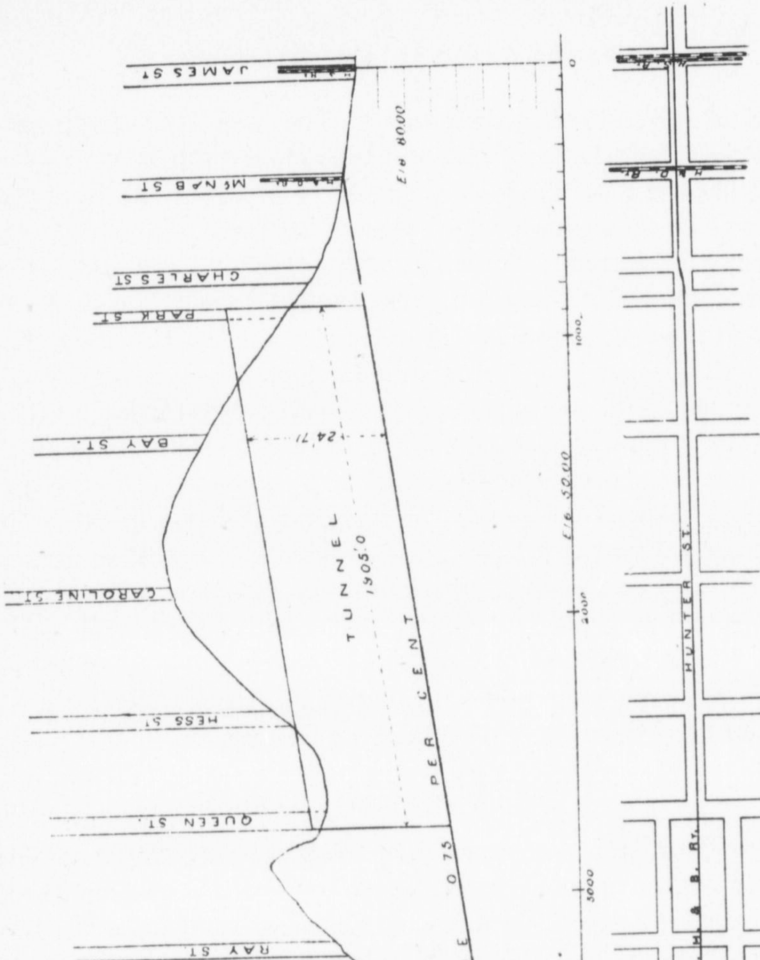


Fig. 1

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1,905 feet, with an extreme width of 37 ft. 6 in. The street is 66 ft. wide, and closely built on both sides with dwelling-houses, which extend out to the street line. The work is masonry throughout, with the exception of 80 feet of steel arch at the east end. For cross-section and details see Fig. 2. Along the outer edge of the boulevards were full-grown shade trees, a few of which were saved, while in the centre of the street were sewers, gas and water mains. Wherever possible new house connections were made direct with other sewers; but where this could not be done new sewers were laid along the back of the property affected to the nearest cross street, where connection was made with the permanent sewerage system. The gas and water mains were moved on to the boulevards, the former carried by overhead pipes across the street, and surface connections made with the house services, while the latter was laid temporarily underground until the street was restored to its original condition.

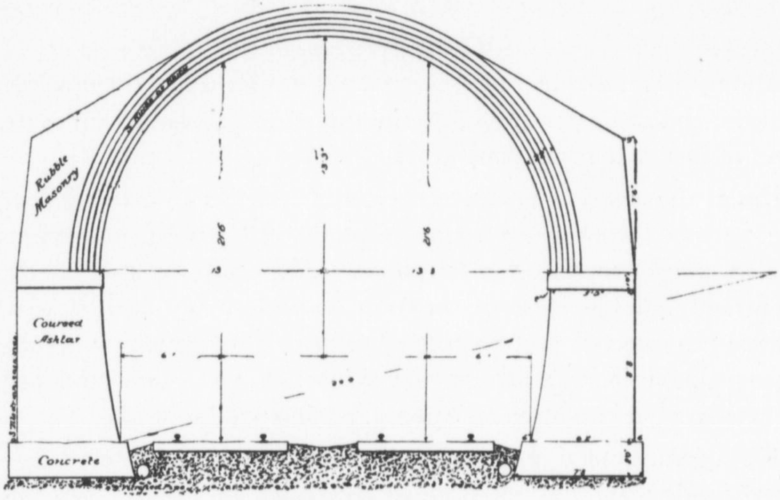
About the middle of June the contract was let, and work commenced toward the end of the same month, the tunnel to be opened for traffic by the end of December, the same year.

Under the terms of contract, the contractor was to take the location of the purposed work as he found it, remove all paving, macadamizing, sidewalks, trees, telegraph and telephone poles within the street lines, and the contractor, at the close of the work, to restore any alley or lot which may have been occupied or obstructed to its original condition at his own expense; also, to restore the paving, sidewalks, and poles, and put the same in as good a condition as before commencing the work.

Throughout the tunnel and approaches the grade is 0.75 feet per hundred, beginning at McNab street, as shown in Fig. 1. Just east of Caroline street the maximum depth of 47 feet is reached, gradually decreasing, until at Queen street the bottom is 25 feet below the surface. On the west side of Queen street the tunnel ends, and from there west is an open cut for a distance of 2,250 feet, having an average depth of 26 feet, width of roadbed 35 feet, and side slopes, 1 vertical to 1½ horizontal. Four streets carried on overhead bridges cross the west approach. These bridges are framed structures, resting on piles, and are the full width of the streets. At Queen street the line leaves Hunter street, where the latter takes a jog to the south, as shown on the plan, Fig. 1, and passes through the centre line of a tier of blocks of private property, which was for the most part closely built with dwelling houses, a few of which were removed bodily, the rest torn down and disposed of in the easiest manner.

NATURE OF MATERIAL.

The nature of the material varied through successive grades from quicksand to rock. From McNab to Park streets the soil was a sandy loam, easily excavated. At Charles street a bed of cemented gravel was reached about eight feet below the surface, gradually approaching the top, until at Bay street there was a covering of but two feet of alluvial soil. The gravel bed at Bay street reached a depth of 30 feet, and was composed of drift boulders and gravel, varying from fine beach gravel to stones one foot in diameter, the whole cemented in one solid mass. The cementing material was carbonate of lime held by water in solution, and deposited on the pebbles, in some instances to a thickness of 0.10 of an inch, binding the whole compactly together. Midway between Bay and Caroline streets the cementing material disappeared, and the gravel became more stratified. For a depth of from 15 to 20 feet the strata was conformable with the surface, while below this it assumed an inclination to the horizontal of about 30 degrees sloping toward the west. This formation extended to Ray street, where it ran out to the surface. Underlying this concrete was a strata of water, carrying quicksand, from 2 to 5 feet in depth, and of a treacherous character. Imbedded in this, and immediately below the cement gravel, just west of Bay street, were found masses of driftwood, mostly roots and branches of trees, and, in one instance, some bones evidently belonging to the mastodon family, which were, however, in such a decayed



Cross Section of Tunnel
T H & B Railway
Fig 2

condition that they crumbled on removal. The wood, however, which was cedar, was comparatively sound and in a good state of preservation.

From Ray street to the end of the west approach the surface material was a stratified clay, deposited in horizontal layers. The soil stratum from Bay to Poulette streets consisted of a blue boulder clay, very hard and tenacious in character; this reached an average height of 9 feet above the bottom of the tunnel. From Poulette street to the end of the west approach was a hard Medina shale, reaching to within 6 feet of the surface and dipping both ways at about an angle of 20 degrees. Overlying this again at the west end was boulder clay and cemented gravel similar to that in other parts of the cut.

EXCAVATION.

With the exception of what was taken out by steam shovels, the material was all excavated by hand. From the east approach to near Bay street it was loaded into wagons in the cut, and taken to fill up low land in the vicinity. The tunnel was excavated to grade from the end by a series of lifts, 6 to 8 feet in height, working out to an open face. This method was especially advantageous in the hard material where blasting was required, which was the only means of loosening up the cemented gravel. Owing to the close proximity of houses on both sides of the cut, blasting operations had to be conducted with great care in order to avoid damage to property. The usual method of procedure was to drill a row of holes at some distance back from the face of the lift, the nature of the material determining their position, to a depth of 4 to 8 feet; light charges of dynamite were put in and connected with a battery by which they were fired; heavy timbers were piled over the charges to prevent the rock from flying, and during the three months that blasting was carried on no serious damage was done to any building along the work.

When wagons could be no longer used in the cut, a large derrick was erected at Bay street, and the material hoisted out in buckets, each bucket holding from 25 to 26 cubic feet. The style of derrick was the ordinary stiff leg type, mast 14 inches in diameter by 45 feet long, boom the same diameter by 30 feet in length, with boom and fall ropes ⅞ inch steel cable, and operated by a 16 horse power double drum hoisting engine. On the bank a platform was erected, about 12 feet square, high enough to allow a wagon to drive underneath, in the centre of which was a hopper, with a three-foot opening in the bottom. On this platform two men were stationed to empty the buckets into the hopper, which discharged into the wagon box below. A sufficient number of buckets was used to cause no delay in hoisting, and under favorable conditions 30 cubic yards of material could be excavated per hour, the number of men employed depending on the hardness of the material. When the working face got beyond the reach of the boom, tracks were laid in the cut, and cars used to convey the buckets ; the economic length of track not exceeding 200 feet. Later on derricks of a similar type were erected at convenient points along the work, and excavation carried on in the same manner.

The west approach, and also the tunnel, for about half the depth, as far as Bay street, was excavated by steam shovels—two shovels working in this section until November. This material was loaded on cars, and taken three miles out on the Brantford division, where it was used to fill a trestle bridge over a deep ravine. After the steam shovel had finished between Bay and Caroline streets, a Lidgerwood cable conveyor was erected to complete the excavation. This consisted of two towers, 70 and 50 feet high, erected 450 feet apart, the larger set in the bottom of the cut, while the smaller one rested on the tunnel at Bay street. The towers were the shape of an inverted V, 25 feet wide at the base, and made of 12 x 12 inch timbers firmly framed together, and braced horizontally and diagonally by 3 x 12 inch plank. A 1½ inch steel cable was stretched over the towers and firmly anchored at both ends. An endless ⅝ inch steel cable, called the carrier rope, passed over a sheave in each tower, and connected with the operating engine, which was a 20 horse power of the double drum type, manufactured by the Lidgerwood Cable Co. To this carrier rope was attached the "skip," a steel frame, which travelled on the main cable, to which the fall rope blocks were attached. To prevent the fall rope sagging when the skip was run out from the head tower, light steel frames or suspenders were used. These were 4 feet long, with a small pulley in the lower end, on which rested the fall rope, while the suspenders were carried by a ½ inch steel rope, on which, at regular distance, were fastened steel stops, or buttons, 5 inches long, and varying in diameter from ¾ to 1¼ inches, the smallest being fastened nearest the front end. In the hangers were corresponding stops or openings, arranged so that the button on the rope would pass through the first and retain the last. These were carried on a projection in front of the skip, and, as it travelled back along the cable, the suspenders were caught and retained by the several buttons, until the skip on its forward movement would again pick them up.

At first automatic dumping buckets were used, holding 70 cubic feet, but these did not work well, owing to the sticky nature of the material, besides producing too much sag in the main cable. Buckets similar to those used on the derricks, and holding 1¼ cubic yards, were then used, and men were stationed on the dump to empty and spread the material. The capacity of the conveyor was about 20 buckets per hour, with the traveller moving at the rate of 400 feet per minute.

After the excavation between the towers was complete, they were taken down and not re-erected, as the amount of the material then in the cut would not warrant the cost.

Of the different methods of excavation employed, the derricks proved the most satisfactory, as they could be quickly moved, re-erected, and operated in a small space.

SHORING.

Where the excavation was concrete the banks stood up vertical without any support; the only precaution necessary was to see that no loose stones fell and injured the workmen below. In the boulder clay some trouble was experienced from portions of the bank falling into the cut after the bottom had been trimmed out for the walls, leaving an overhang of from 2 to 3 feet in the hard material. This was caused by pressure from above, assisted by water in the quicksand overlying the clay. As no supports could be applied, the clay just reaching the top of the abutment walls, the toe of the banks was left projecting out, and trimmed off as the masonry advanced. As soon as the walls were built all pockets behind the walls were immediately filled with clay, and rammed solid to prevent further accident.

From 100 feet west of Bay street to Queen street, the banks had to be strongly supported. A light shoring was put in as soon as the steam shovel got low enough that its working did not interfere with the bracing. This was made of five 2-inch planks, 12 inches wide, firmly nailed together to break joints, and cut to the required length. These were placed from 12 to 16 feet apart, and held in place by wedges driven against a plank, placed vertically at the ends. As the depth increased, additional support was required. The shoring was rearranged to form a series of trusses, made by placing the transverse braces vertically over each other, 8 to 10 feet apart, adding an additional brace as soon as the depth of the cut would allow, the whole tied together by diagonal and vertical braces of 2 by 10-inch plank. These trusses were placed every 12 feet, and firmly braced to each other. Between the end of the braces and the bank, heavy planks were placed horizontally, behind which was driven a close sheet piling, extending to near the bottom of the cut.

This arrangement of bracing proved fairly satisfactory while the dry weather continued, but after the banks had become saturated with rain, and loosened by repeated freezing and thawing, the support did not prove strong enough, and some serious slides occurred. The bracing did not appear to have sufficient stiffness in the individual members, due partially to their small cross-section, and also being built of plank instead of solid timber. Failure always occurred by the streets bending horizontally under pressure, the defection rapidly increasing, as but little resistance was offered to this motion. Another source of weakness was the difficulty of getting the pressure equally distributed; the streets acting, more or less, independently of each other.

Toward the close of the work, and before the abutment walls were completed, it became necessary to modify the lower shoring, to allow trains to pass through. This was done by placing mudsills, 12 by 12 inches, 20 feet long, across the bottom of the tunnel, 6 feet apart. Against the ends of these were erected posts of the same length and dimensions as the mudsills, while across the top, between the posts, were streets of the same size. This formed a rectangular opening, wide enough to operate two tracks in, and facilitate the passage of trains without hindering the work. Behind the posts were short streets supporting the sheet piling, as before described.

The slides that occurred were not altogether due to the weakness of the supports, the banks not falling forward into the cut, but dropping vertically downward. As the excavation got through the quicksand and into the clay, the sand would commence to run, taking away the support of the upper material, which would press downward, thereby loosening the shoring and causing the whole to collapse. The trenches made for the water mains along the boulevards also formed a weakness in the banks, which invariably sheared off along this line.

MASONRY.

The tunnel is masonry throughout, with the exception of 80 feet of steel arch before mentioned, and is built to the dimensions shown in Fig. 2. The retaining walls on Park and Charles streets, together with the spandril wall of the arch, is third-class masonry, or rubble work; while the abutment walls of the tunnel and retaining walls at east and west portals are range ashlar. The portals are cut-stone masonry, hammer dressed, and of the design as shown in Fig. 4.

Limestone was used for all stone masonry. That for the abutment and retaining walls was supplied from the Longford quarries, while the stone used in the portals was supplied from Beamsville. The Longford stone is a fine-grained, compact variety, possessing an average crushing strength of 20,800 pounds per square inch; of a pale, bluish-gray color, with some crystals or calcite; and effervesces strongly with acid, leaving no residue in the test tube. It is quarried in beds of from 4 to 28 inches, and of any size up to six feet square. The Beamsville stone is an open-grained, porous variety, pale gray in color, and crushes at 13,085 pounds per square inch; it contains a varying percentage of sulphur, and, when tested with acid, leaves considerable residue in the tube.

The arch is built of selected hard burnt brick, five rows in thickness, and covered with Trinidad asphalt.

FOUNDATIONS.

The substratum on which the foundations rest is a uniformly hard boulder clay, impenetrable by water, and not likely to produce unequal settlement. With the exception of a short distance in the centre of the tunnel, the foundation material is concrete, with a minimum thickness of 2 feet 2 inches. On the approaches, the top elevation of the concrete, with respect to sub-grade, varies according to the location; under the portals the top is 2 feet 6 inches below sub grade, and 2 feet thick, while, throughout the tunnel, the foundation reaches one foot above sub-grade, and level with the base of the rail, as shown in Fig. 2. From the east portal, for a distance of 200 feet, where the bed of the tunnel passes through the quicksand previously mentioned, the foundations were carried down to the clay, which did not exceed more than 5 feet below the normal surface.

For concrete, the specifications called for one part of Portland cement, two parts clean sharp sand, and four parts broken stone, the stone to be clean, and broken not to exceed cubes of 2 inches, or less than ½ inch, the whole properly mixed and laid in courses not more than 9 inches thick.

The concrete was mixed by hand, on platforms 10 by 12 feet square, kept close to the foundation pits; the stone, sand, and cement sent down in shutes from the top, and wheeled on to the mixing board as required. A batch usually consisted of 4 cubic feet of cement, 8 cubic feet of sand, and 16 cubic feet of broken stone. The sand was first spread evenly over the board, the cement added and thoroughly mixed; enough water was then added to make a thin mortar, which was again evenly spread out. Stone was then added, first being thoroughly wet, and the whole mass then turned twice to insure proper mixing. It was then put into the trench, and tamped until mortar flushed to the surface. The top surface, after being carefully levelled, and built to the proper grade, was allowed to stand twenty-four hours before receiving the masonry.

Towards the close of the work, stone was substituted for concrete, in order to gain time, and also to leave the shoring undisturbed until the masonry could be immediately put in place. For this large flat stones were used, having an area not less than 12 square feet, nor less than 12 inches thick, firmly bedded in the clay, and all joints well bonded and grouted with cement mortar.

ABUTMENT WALLS.

The abutment walls were built of range ashlar, to the dimensions shown in Fig. 2. The coping projects 2 inches beyond the line of the wall, and extends the full width in one piece. The specifications covering this work were: the masonry must be laid in horizontal courses, having parallel beds and vertical joints on the face, with no stone less than 9 inches in thickness, well bonded and levelled, and laid in good cement mortar. At least one-third of the stone in the face to be headers, evenly distributed throughout the wall; stone in the face-work to be not less than three feet in length, nor less than one foot six inches in width, and every stone to have a bond of at least eight inches with the underlying course, and no mortar joint to exceed three-quarters of an inch in thickness. The backing to be of good-sized, well-shaped stones, laid on their natural bed, and carried up at the same time as the face-work. All stones must be laid to break joints, and thoroughly bond the wall in all directions.

In the walls, every 200 feet apart, spaced alternately on each side, are manholes to provide for the safety of the workmen while trains are passing. These are built capable of holding five men.

For building the abutment walls a travelling derrick was first used. A heavy car was built and mounted on ordinary car wheels, spaced to run on a track of 13 feet 9 inches gauge. At each corner of the rear end was mounted an ordinary stiff leg derrick, firmly secured to the car The framework consisted of two longitudinal stringers on each side, 4 by 14 inches, 20 feet long, spaced 6 inches apart, between which the car wheels rested, while in the centre were three stringers, 6 by 12 inches, spaced evenly between the outsides. Across the ends were firmly framed into the longitudinal stringers two pieces 12 by 14 inches, 15 feet long, and at 7 feet from the outside were two pieces 6 by 8 inches, passing across under the longitudinal framing, and bolted through the top. Passing under these and through the end framing were four ⅞-inch truss rods,

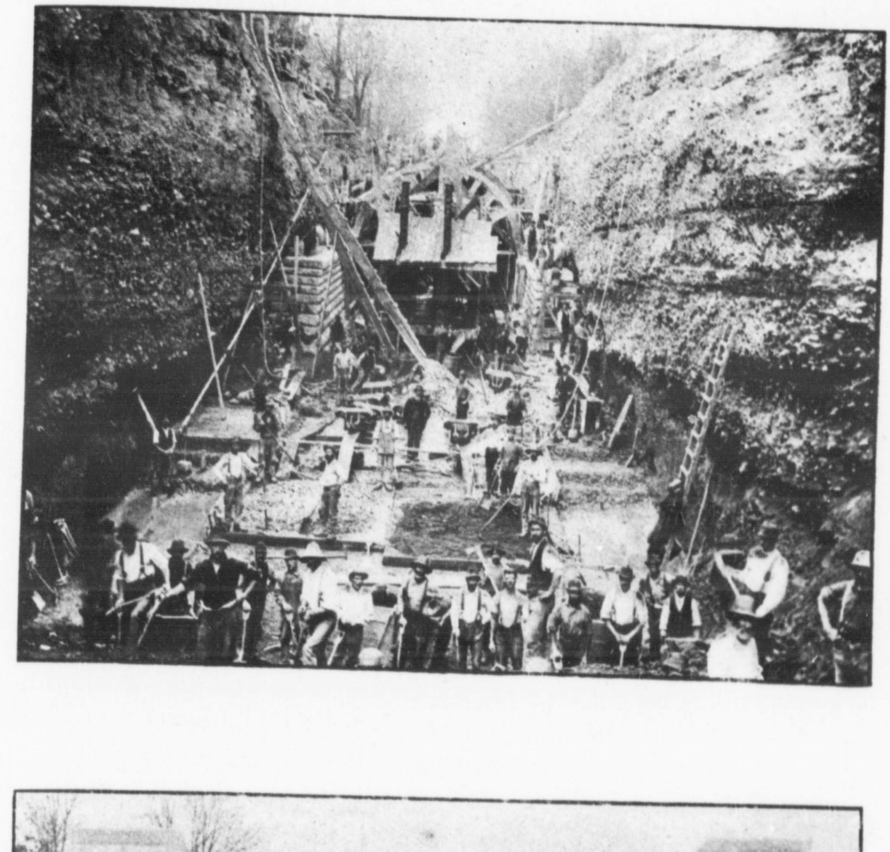


Fig. 4

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securely fastened, the whole covered with a 3-inch floor. The principal dimensions of the derricks were: mast, 12 by 12 inches, by 14 feet; boom, 10 by 10 inches, by 20 feet; and stiff legs 8 by 10 inches, by 17 feet; fall and boom ropes ⅞ inch steel cable. Power was furnished by two double drum hoisting engines, 14 horse power each, mounted on the front of the car. This derrick proved very satisfactory for the work in hand, having a quick action and being easily operated.

Building was begun at the east end, and for the first two hundred feet the stone was delivered to the derrick in wagons, but afterwards it was run in on stone cars, a track being laid along each side of the tunnel, using the derrick car-track for the inside rails. The stone was delivered by wagons at Charles street, where it was sorted and run in as required. In this way both abutment walls were carried on together, a gang of masons working independently on each wall.

The travelling derrick was employed up to midway between Bay and Caroline streets, and from the west portal to Hess street; between these two points the shoring and operating of trains prevented its further use. The walls in this section were built by derricks, similar to those used in excavating, erected on each bank. Each derrick had a reach of sixty feet, and was repeatedly moved along the bank as the walls advanced. This method of building was necessarily slow, as the fall blocks had to be operated through 30 to 40 feet of bracing, ofttimes requiring to be repeatedly raised to the top and lowered again in another pocket before the stone was placed in position. The sand and cement were sent down in shutes and mixed below, water being supplied from the city water service; latterly, when the cold weather began, the mortar was made above, and sent down as required. Fig. 3 shows the shutes in position and methods of working.

PORTALS.

These were built of first-class masonry, hammer dressed, and laid in Portland cement, with joints not greater than ¼ inch. The design, as shown in Fig. 4, fully illustrates the work.

BRICK ARCH.

The arch is semicircular, and built of No. 1 selected hard burnt brick, five rows thick, laid on edge. The dimensions, as shown in Fig. 2, are: radius 13 feet 3 inches, with thickness of shell 22 inches. The brick was built over centres set up five feet apart, resting on a six by six inch stringer, placed against the face of the coping, which was supported by posts resting on the foundations. The centres were lined up by two maple wedges placed under each end, and cut so that when placed together the outer sides were parallel. To facilitate easy removal the centres were made in two parts, after the fashion of a king-post truss, and firmly bolted together. When ready to erect, a platform was built level with the top of the coping, the centres bolted together on that and raised to place. When ready to be taken down the platform was re-erected, the centres lowered down, unfastened, and carried along to where again required. A pine lagging 2 inches thick, placed closely together and lightly fastened, formed the covering.

The arch was built in from 25 to 100 feet sections, depending on the length of the abutment wall ready. To more firmly bind the different sections together, the outer end, in each case, was reached back from the springing line to within 3 or 4 feet of the centre, which part was finished square across. Both sides were carried up at the same time, and care was taken that they should be exactly parallel. No special provision was made for keying the arch other than that the key now was selected to fit tight and snug. As soon as the spandril walls were built, the centres were struck, and the arch allowed to take its permanent set; the deflecting under a maximum load of 2,800 pounds per square foot not exceeding ⅛ inch

Before going into the arch all brick was thoroughly wet. This was done by having vats made 12 feet square and 1 foot deep, filled with water, in which the brick was allowed to soak. At first they were continuously put in and taken out of the vat, a process which was not entirely satisfactory. The workmen's hands, through continuously handling the brick in the cold water, became chafed and sore, and, unless watched closely, they were apt to send down brick scarcely wet. This difficulty was overcome by having frames made holding from 30 to 50 brick. In these the dry brick was placed and set into the soaking vat. After remaining in the water a sufficient length of time the frames were lifted out, emptied, and refilled again with dry brick. In this way every brick was evenly saturated, and the workmen were able to keep their hands comparatively dry. In laying the brick, care was taken that all dirt was removed from the lagging and from between the rings, also that all joints were properly cemented and grout poured into all openings until flushed. The several rings were fitted closely together, but not cemented other than what adhered between the joints of the brick.

SPANDRIL WALLS.

As shown in Fig. 2, the spandril wall is built out flush with the back of the abutment wall, and carried up 7 feet 6 inches, with a total batter of 9 inches, from which point the top is finished tangent to the arch. The stone was sent down in shutes, and laid by hand, as derricks could not be conveniently worked. The top surface was covered with a heavy coat of mortar, thick enough to cover all projecting points of the stone and secure a flat surface to the outer covering.

STEEL ARCH.

To carry Park street over the tunnel, it was necessary to grade up the north and south approaches for a considerable distance on each side. To make this grade as flat as possible, the outside of the arch was lowered by using steel instead of continuing the brick up to the portal, and by such gaining 14½ inches. The arch frame was made of 7-inch **T** beams, 25 pounds per foot, bent to the proper radius, and spaced 3 feet centres, the lower ends secured in pockets 2 feet deep, cut in stonework. Each rib was in two pieces, fastened at the top by two splice bars, 18 by 5 by ¾ inch. Extending longitudinally between the ribs, and spaced 3 feet apart, were 3 by 3 inch **T**s, weighing 7 pounds per foot, and firmly riveted at the ends, forming panels 3 feet square, which were covered with ¼-inch buckle plates, raised in the centre 1½ inches, and fastened to the framework by ½-inch rivets, spaced 5 inches apart.

The steel work was shipped from the shops in half sections, two panels long, and ready for erection, thus reducing the field riveting to every third panel, besides the splice plates. All the steel received two coats of graphite paint, one before and one after erection, while around the edges of the buckle plates and joints was put a coating of pure asphalt; the whole was then covered with cement to the depth of one inch above the top of the ribs. The spandril wall was built and otherwise finished over the arch similar to that on the brickwork.

ASPHALT COVERING AND BACK FILLING.

The outside of the arch and spandril wall was protected by a covering of asphalt one inch thick. A coating of tar was first applied to the brick, after which the asphalt mixture, similar to that used in street paving, was applied and firmly rolled. Over this was spread a coat of pure asphaltum, the whole forming an impervious covering to water, and keeping the arch dry on the inside. When this covering was complete the earth was filled back over the arch, care being taken that all pockets behind the spandril walls were properly filled, and rammed solid, and the street restored to its original surface. With the exception of what was excavated by the cable conveyor, all back filling was done by wagons, using the material taken out of the cut. Of this there was not enough left to fill up the street, and some 12,000 to 15,000 yards had to be borrowed to complete the work.

CEMENT.

The specifications called for a cement equal in quality to the finest Louisville brand, and subject to the standard specifications of the American Society of Civil Engineers.

The abutment and retaining walls were built with a natural cement, made at Queenston. This was a slow-setting cement, requiring from five to eight days to harden in the wall, at the end of which time it possessed a moderately high tensile strength.

The brick arch and foundations were laid in Portland cement, the Samson brand, manufactured by the Owen Sound Cement Company, being used, which gave uniformly good results.

The following is a series of tests of the later cement made by Mr. C. H. C. Wright, of the School of Practical Science, covering the time which it was used on the work :

DATE.	Residue on No. 100 Sieve.	Residue on No. 50 Sieve.	Specific Gravity.	Hot Test. Pats immersed 48 hrs. in water at 115° temp.	Tensile strength, 1 day in air, 6 days in water, average of 10 bricks broken.
1895.					
March 11....	12.3 12.7	1.9 1.8	3.098	Sound	384
April 25...........	17.5 17.6	2.0	3.10	"	531
July 16............	20.5 20.4	2.2 2.1	3.10	"	464
August 1...	22.0 21.8	3.3 3.0	3.11	"	426
September 28......	22.1 21.5	9.8 9.5	3.10	"	478
November 14......	23.1 22.0	10.0 9.8	3.09	"	465

PROVISIONS FOR BUILDING MASONRY IN FREEZING WEATHER.

In order to carry on the work efficiently in frosty weather, provisions were made for heating the mortar and keeping the stone free from ice and snow. For heating the sand, long iron pipes, about eighteen inches in diameter, were laid on the ground and the sand filled around on the outside, while a fire was kept constantly burning in the pipe. Water was heated in large kettles, and the mortar mixed in small quantities as required for immediate use. The temperature of the mortar when newly made varied from 90 to 100 degrees, and, if kept in a compact form, would retain that temperature until ready for use. During extremely cold weather salt was mixed with the water to form a weak brine, and also was sprinkled over the bed of the stone. For removing ice and snow from the material going in the walls steam was applied through a hose, cleaning the stone and warming the surface to some extent. For first-class masonry, in addition to these precautions, the stone was heated, previous to going in the wall, by hanging it over a fire for a short time, taking care that no injury was done by the smoke or excessive heat. On the brickwork the same precautions were taken, and all tempering and grouting was done with hot water.

DRAINAGE.

This very important item in all underground work cannot receive too careful attention, for on it depends, to a great extent, the durability of the superstructure.

In the present case the manner of treatment and disposal of the water was much easier than is often met with.

With the exception of two places on the work, the water caused no serious trouble, though more or less had to be contended with all along. During construction wells were dug at convenient points as the excavation progressed, and all water drained into these, from where it was pumped up into the city sewers, the ordinary direct-acting plunger pump being the type used. These were placed in the bottom of the cut, and steam supplied from the hoisting engine on the bank.

Just west of the portal, where the floor of the tunnel passed into the quicksand, a retaining wall of concrete was put in, reaching across the bottom, and connected with the foundations on either side. This extended down to the clay, and prevented all water held in the quicksand on the upper side from passing beyond this point, it being here intercepted and led into the side drains, thus keeping the sub-grade, which for the next 200 feet beyond this was quicksand, dry and firm.

For draining behind the walls 3-inch tile was laid along the top of the foundation footing, and every 100 feet apart weepers of the same size were put in flush with the top of the concrete, leading through the walls into the tunnel; all joints being carefully covered with broken stone to allow the water easy entrance into the pipe. Along the inside of the tunnel, on both sides, were laid 8-inch vitrified pipes, extending the full length, and discharging into open drains at the west portal. Catch basins were put in at the east portal and Charles street to collect the surface water gathering in the east approach, the former connecting with the tunnel drainage and the latter with Charles street sewer.

VENTILATION.

No special provision is made for ventilation other than the natural draft through from the ends, and, under ordinary conditions, the tunnel will clear itself from smoke and gases in from twenty to forty minutes.

QUANTITIES AND COST.

From the limited time given to complete the tunnel, the work was not carried on so economically as it otherwise would have been had the time limit been twelve instead of six months. No expense was spared in providing plant or labor, and everything was done to push the work ahead. The plant in operation on the work consisted of two steam shovels, with the necessary trains to move the material, ten steam derricks, three horse power derricks, and a cable conveyor, besides other plant necessary for the work. Excavation was carried on night and day until complete, and, at times, masonry was also built by night shifts. The excavation was practically finished by December 28th, the tracks connected through the tunnel, and the Government inspection made of the road on December 30th, immediately after which a regular train service was established, thus complying with the spirit of the by-law.

The following tables give the quantities in the different parts of the tunnel:

Excavation in approaches	163,000	cubic yards.
“ tunnel	98,000	“
Back filling over tunnel	42,000	“
Concrete laid	2,952	“
Masonry, 1st class	317	“
“ 2nd class	8,193	“
“ 3rd class	519	“
Spandril wall, 3rd class	4,893	“
Brick arch	5,366	“
Asphalt covering	72,000	square feet.
Steel arch	66,133	pound

or 829 pounds per lineal foot of arch.

For masonry, asphalt covering, and drains, the tunnel cost $87.00 per lineal foot. This does not include the portals, excavation, back filling, or shoring, which would bring the total cost about $120.00 per foot.

Comparing the cost of the steel with the brick arch, the former, including the concrete covering and spandril walls, exceeded the cost of the brick arch and spandril walls by 33 per cent.

The following table gives the percentage of cement used, and cost of laying one cubic yard of the different classes of masonry.

	Cement used per cubic yard of wall built.	Cost of building one cubic yard of wall.
Abutment walls, 2nd class masonry	0.55 bbls.	$1.32
Spandril walls, 3rd class masonry	0.80 "	1.85
Brick arch, masonry	0.95 "	1.95
Concrete	1.11 "	0.95

After the travelling derrick was abandoned, and the abutment walls built by derricks on the bank, together with extremely wet and cold weather existing under the latter conditions, the cost of building increased about 50 per cent., while under the same climatic conditions, when it was necessary to use warm mortar and remove ice and snow from the work, the cost of laying brick increased about 30 per cent. The cost of the spandril wall also increased, under these unfavorable conditions, from 20 to 30 per cent. above that shown in the table.

These quantities and prices in the table are averages covering a period in which the conditions for working were favorable and uniform. The figures given is the actual cost of laying the masonry, which includes the hauling of the material into the tunnel for the abutment walls, and conveying brick, stone, etc., from the surface to the work below; also making and carrying the mortar. No allowance is made for plant further than the expense of operating derricks when used, or of bringing material on the ground.

In this table is given the average amount of masonry laid per day of nine hours, per mason, under the same conditions as applying to the previous table.

Abutment walls, 2nd class masonry	6.7 cubic yards.
Spandril " 3rd "	3.3 "
Brick arch	2.2 "

Allowing 485 bricks per cubic yard of arch built.

The abutment walls were built by gangs of four masons and foreman to each derrick, which the above table refers to.

In laying the concrete it was found that 52 barrels of cement, 36 cubic yards of broken stone, and 19 cubic yards of sand laid 45 cubic yards of concrete in the wall.

The tunnel was designed under the supervision of Mr. E. B. Wingate, chief engineer of the Toronto, Hamilton & Buffalo Railway Company, who are owners of the tunnel; and the work was carried on under the care of Mr. P. Mogenseu, divisional engineer on the works, and the writer as assistant divisional engineer. One contract covered the whole work, from the east approach to Locke street, and was successfully carried out by Mr. A. Onderdonk, contractor.

Under the direction of Mr. Doheny, superintendent of construction, and associated with him Mr. M. R. Northway, the contractor's engineer, the work was prosecuted with unceasing vigor from start to finish.

Though the above work presented no special engineering difficulties, the locality and conditions under which it was constructed, as well as the short time allowed for completion, make it worthy of mention in the annals of modern railroad construction.

THE PENDULUM.

By A. M. Scott, '96, Arts.

HISTORICAL SKETCH OF THE PENDULUM, WITH ITS APPLICATIONS IN PHYSICS.

The history of the pendulum begins in 1589 with the discovery, by Galileo, of the isochronous nature of the small oscillations of any swinging body. From this humble beginning, first due to crude observations on a swaying chandelier, there has been an almost continuous progress up to the present; eminent physicists have given months and years to careful experimenting; noted mathematicians have applied themselves to the solutions of problems which arose; great attention has been given to the detection and avoidance of errors, and the ingenuity of scientists, no less than the skill of machinists, has been taxed to produce instruments perfect in every detail, until now the pendulum has become indispensable as a mechanical appliance, while the exact determination of the length of the second's pendulum, with its variations for different places, was never of more importance than to-day by reason of its numerous applications in the field of science. It is not the purpose of the present paper to follow the steps in the development of the pendulum in exact chronological order, but rather to deal with the subject by topics: first, the successive forms which the pendulum of experiment has taken; second, the various corrections found necessary; and, last, the physical applications which have been made of the pendulum.

I. After Galileo, who established the principal laws of motion of the pendulum in 1629, the next important discovery is due to P. Mersenne, who, in 1646, submitted to geometers the problem of finding the centre of oscillation, or, as he termed it, the "centre d'agitation ou de balancement" of an oscillating body, the existence of which he had before proved by experiment. The finding of this centre meant the determination of the simple equivalent pendulum; but though Descartes and Cavendish, with others, joined Mersenne in his investigations, the solution of the problem was not found until given by Huygens in his "Horologium Oscillatorium," 1673; the application of this solution by the three Bernoullis to the pendulums then in use made it possible, by 1726, to calculate exactly the length of the simple equivalent pendulum, allowance being made even for the weight of the suspending thread.

By Mersenne and Descartes the simple pendulum was considered as "un plomb suspendu à un filet"; later, the discovery of the centre of oscillation and the tendency of particles above or below this to change the rate of motion, together with the effects due to the weight of the thread, have caused it to be defined as "a particle of matter suspended by a right line devoid of weight, and oscillating by the force of gravity about a fixed point, called the point of suspension." It is now known to be an ideal instrument which can never be realized in actual experiment, but first notions fixed the form of the pendulum for many years. As used by all physicists and astronomers till the end of the eighteenth century, it consisted of a ball of lead or copper about an inch in diameter suspended by a thread, usually of pite or aloes, which was held at its upper extremity between firmly fixed metallic jaws. Godin and Bradley, in their experiments from 1743 to 1749, introduced the use of a metallic thread, and joined it to a knife-edge which rested on a fixed plane and became the centre of suspension for the pendulum. Bouguer, about the same time, tried the use of two truncated cones joined by their larger bases instead of a spherical ball, but this was not generally adopted.

The next improvement was suggested by P. Boscovich in his work on "Optics and Astronomy," published in 1785, one chapter of which was headed: "De determinatione longitudinis Penduli oscillantis ad singula minuta secunda temporis medii." Though a purely theoretic paper, this was very carefully prepared, and is important as showing the progress made in pendulum study at that time, and also on account of the great number of ingenious details and corrections therein suggested, many of which were not adopted until years later; other references may be made to it in the course of this sketch. He thought that the knife-edge could be so constructed that its oscillations would not affect those of the ball and suspending thread. This was found difficult to do, but Borda and Cassini, in their memorable experiment in 1810, to determine the length of the second's pendulum at Paris, attached a screw to the upper part of the knife-edge perpendicular to its length; along this a small metal button could be raised or lowered, and served as a regulator by means of which the time of oscillation was made the same for the knife-edge alone, resting on the plane surface, as when the ball and thread of the pendulum were attached. "Ce synchronisme une fois établi, il est clair que le mouvement du pendule, n'etant pas contrarié par celui du couteau, devait être le même que si la masse du couteau et de sa mouture avait ête pour ainsi dire nulle," writes Borda; this was confirmed by experiment.

The pendulum used by these men consisted of a platinum ball about 1⅓ inches in diameter, suspended by a finely drawn iron wire, which was fastened to the knife-edge just referred to; platinum was chosen for the ball on account of its great density, and iron wire preferred for its strength, also because it offered less surface to the resistance of the air than silk or vegetable fibre threads. To correct the error that might arise from irregularities in the shape of the ball, or inequalities in density, the wire was joined to a small cap consisting of a segment of a spherical shell, which had the same radius as the ball and could be attached at any point. On account of the extreme care taken in their experiments, and the degree of perfection to which they brought the so-called simple pendulum, Borda and Cassini seemed to leave nothing for their successors except to repeat the same methods, unless some new form were devised for the instrument.

In the meantime, Chevalier de Prony, in 1792, and more definitely in 1800, had set forth the advantages to be gained by the use of a rigid pendulum of considerable mass, which might oscillate about several parallel axes, all in the same plane, passing through the centre of gravity of the instrument. These were as follows:

"1°. De dispenser absolument d'avoir égard à la forme du corps;

"2°. De pouvoir employer une masse d'un assez grand poids pour qu'elle oscillât, sans interruption, pendant tout le temps qui s'écoulerait entre deux passages consécutifs d'une étoile par un même vertical.

"3°. Enfin, de faire les expériences avec un corps de forme invariable, de la plus grand solidité, dont la conservation indéfinie et le transpor n'offrent aucune difficulté, et sur lequel on peut repétér, à une époque quelconque et dans les lieux différents, des expériences parfaitement comparable et des mesures absolument indentiques avec celles qui ont servi à une détermination primitive."

From the times of oscillation about three axes, the moment of inertia of the system about the centre of gravity could be calculated; thence, with the aid of the distances of the axes of suspension from the centre of gravity, the lengths of three simple equivalent pendulums determined. But though the merit of this method was recognized by scholars, and a simpler form of the instrument devised by Bohnenberger in 1811, it was viewed with disfavor by advocates of Borda's methods, and did not come into use. It was not till the principle of the reversibility of the centres of suspension and oscillation, first due to Huygens, was applied to the pendulum by Captain Kater, in 1817, that the reversible pendulum assumed a practicable form, with two fixed axes of suspension parallel to each other, and a system of movable weights, to be adjusted so that when either axis was the centre of suspension the other would become the centre of oscillation.

On account of the absence of any very definite mathematical principles governing the adjustment of the weights of the Kater's pendulum, which process, as we can testify, is necessarily long, and may become tedious, the ball and thread continued to be used until the time of Bessel's experiments, in 1826, to test Laplace's theory of the influence of the form of the knife-edge on the true length of the pendulum. Laplace, in studying Borda's pendulum in 1816, maintained that the knife-edge was not a true geometrical angle, but a cylindrical surface, which, during the oscillation, rolled upon the plane supporting it; and, therefore, that the distance measured from the knife-edge to the centre of the ball, to determine the length of the pendulum, must be increased by the radius of this cylinder, to get the true length. Also, the centre of suspension, being raised slightly above the plane of support, constantly changes in position if the surface of the knife-edge is not truly circular, and thus, theoretically, a disturbing influence on the duration of an oscillation, and the length of the simple equivalent pendulum, is introduced. Bessel's numerous experiments verified this hypothesis, and led him to conclude that it is impossible to measure exactly the length of the simple pendulum by the use of a single knife-edge, owing to the error introduced by the cylindrical form, whose radius of curvature is indeterminate, from its very small size. In addition to this, the experiments of Baily, in 1832, to find the necessary correction for reduction to a vacuum, along with the calculations of Stokes, showed that the resistance of the air to the suspended thread increased as the thread became finer, thus helping to make the simple pendulum a very complex instrument.

For these reasons, attention was then turned to the perfecting of the reversible pendulum, chiefly to get rid of the errors arising from the resistance of the air, and from the form of the knife-edges, both of which are present to some extent in the Kater's pendulum. On account of its unsymmetrical form, the resistance of the air is different in the two positions, and when the adjustment has been made so that the oscillations in air are of equal duration for both positions they will not be so in a vacuum; further, no allowance is made, or can be made, for differences in the form of the suspending knife-edges, which, though they may be nearly so, are not identical. Bessel, in a paper entitled, "Construction d'un pendule symétrique à axes reciproques," 1849, described an instrument afterwards constructed by Repsold, and now known by his name; in this were embodied the results of Bessel's thorough and extensive investigations into the different sources of error. It was made perfectly symmetrical in form, that the influence of the air should be the same for both positions; and the knife-edges were so arranged that they might be exchanged, in order to eliminate the error arising from their differences of form, that is, from their unknown "cylindricité," as it is called. After a long mathematical calculation, Bessel's conclusion is: "Mais il y a un moyen très simple d'eliminer complètement l'influence de la cylindricité: on dispose le pendule de telle sorte que l'on puisse échanger entre eux les deux couteaux, et l'on fait les expériences avant et après cette permutation. Il en résulte deux erreurs égales et de sens opposé qui disparaissent dans la moyenne." The account of an experiment and calculation to determine the height of the centre of suspension above the plane of support is given in the same paper, and summed up as follows: "Mais on voit, d'apres cela, que cette expérience ne fournit aucun motif de penser qu'un couteau aussi admirablement travaillé que celui de M. Repsold, dont il a été fait usage dans ces recherches, donne le centre du mouvement sensiblement trop bas."

Repsold's pendulum was first employed in 1866, at Geneva, and, though the mistake was made in not having a solid support for it in the first experiments, it was adopted the following year by the International Geodetic Association, and has since been generally considered among European scientists as the best instrument for the absolute determination of gravity. The report of this Association for 1883 gives the following conclusions, based on previous observations:

"1°. Le Pendule à reversion de Bessel possède à un très haut degré tout les qualitiés requises pour les déterminations absolues de la pesanteur, si l'on fait osciller deux Pendules d'un poids essentiellement différent sur le même support.

"2°. Il faut non seulement employer les mêmes couteaux pour les deux Pendules, mais ces couteaux doivent pouvoir être échangés pour chaque Pendule. Les couteaux en agate sont préférables à ceux en acier.

"3°. Il faut faire les observations dans des localitiés (locaux) d'une temperature presque constant; l'emploi du vide n'est pas recommandable.

"4°. Les durées d'oscillation doivent être observées pour les deux positions du Pendule dans les mêmes limites d'amplitude."

These do not show much change from Bessel's results, except the use of the two pendulums to correct the error arising from the flexure of the support. In the experiments performed under the direction of the United States Coast and Geodetic Survey during the last twenty years or more, the Kater's pendulum has been used for absolute measurements, and both this and Repsold's are recognized as giving very accurate results.

A word must yet be said of the invariable pendulums used by Kater in 1819, and by many since his time, for making differential measurements. They had only one axis of suspension, the knife-edge and the weight were securely fastened to the rod; then the period of the pendulum at various places was compared with its period at a certain base station, where, by means of a reversible pendulum, an absolute measurement had been made. Most of the recent experiments have been made with some form of invariable pendulum; a very fine one, prepared under the direction of T. C. Mendenhall, Superintendent of the United States Coast and Geodetic Survey, for use in his work, is described in an appendix to the report of the survey for 1891. It was made one-fourth the usual length, and beat half-seconds, a form used successfully in 1883-4, by Von Sterneck, in the mines of Pribram and Bohemia, and was allowed to swing in a receiver, which could be exhausted of air; besides three different pendulums which could be used successively, the outfit included a dummy with a thermometer attached, to be placed in the receiver for the accurate determination of the temperature. This instrument possessed the merit of being convenient to carry, and so easily adjusted that an observation could be taken in a few hours. Many forms of invariable pendulums have been constructed; one prepared for Commandant Defforges, in France, of symmetrical form, with reciprocal axes, and knife-edges that could be exchanged, gives probably the most correct form in theory, though Major Herschel, of England, strongly objected to the use of the term "invariable" as applied to such instruments.

The method of determining exactly the duration of an oscillation, or the number of oscillations made by a pendulum in a given time, has always been by comparison with the pendulum of a clock which beat seconds of mean time. In the first attempts, the pendulum of experiment was placed before the clock pendulum, so that both could be observed easily; the former was made to begin its oscillation at the same time as the latter, and in the same direction; the observer then looked from time to time to see if they continued to oscillate together; if not, the suspending thread of the experiment pendulum was made shorter o longer, as it went slower or faster than the other, and this process of adjustment was kept up until the two pendulums continued to oscillate exactly together. This method was used and described by the abbot Picard, 1669, and by J. D. Cassini, 1681, and was followed by others till 1735, when M. de Mairan, who was making some experiments at Paris with the pendulum to obtain results for comparison with those to be obtained from similar experiments at the equator, devised a new method, which he designated "la méthode des concours."

The pendulum of experiment having been placed before the other, the concurrence took place when both at the same instant reached the limit of an oscillation on the same side of the vertical; the interval of time between two successive concurrences, expressed in seconds, gave the number of oscillations of the clock pendulum in that time, and the number made by the other was two more or two less, as it went faster or slower. From this result the number of oscillations made by the experiment pendulum in twenty-four hours could be easily calculated.

Mairan's method was defective, because the decreasing amplitude of the arc of vibration brought the thread of the experiment pendulum to a different position for each concurrence, and, further, because the concurrence was noted when both pendulums were at a standstill, a difficult thing to decide accurately, as their rates of motion were, in general, different. Bouguer and La Condamine used a method between this and Picard's; the pendulum was made to oscillate nearly in unison with the clock, so that they would swing for a length of time together; then the observer determined as accurately as possible, when a complete oscillation had been gained, by the aid of a scale attached to the clock, and divided into twelfths. Godin and Bradley used an eye-and-ear method, noting when the thread of the pendulum passed the vertical position exactly in coincidence with the ticks of the clock; the method of coincidences, as used by Borda, Kater, and many others during the present century, was explained in full detail by Boscovich, 1785, in his work already referred to. The pendulum of experiment was to be placed exactly in front of the clock pendulum, both made to oscillate, and some means provided to determine when both passed the vertical position at the same instant. Observations were made through a telescope to decide, with Borda, when a bright spot on the clock pendulum, made by the intersection of two white lines, disappeared behind a screen at the same instant as the suspending wire of the other; and with Kater, when a similar spot was hidden by the tail of the experiment pendulum, all observations being taken at the vertical position of both pendulums.

Various applications of the same principles have been made by experimenters to ensure greater accuracy in this method, the principal changes being due to the use of mirrors and lenses attached to the pendulums, and electric batteries with a clock or chronometer in circuit, to register the time on a chronograph, or to emit flashes of light at regular intervals. Lieut.-Col. Herschel used a small disc fastened to the clock pendulum, having, instead of the two intersecting lines, a ray of light focussed on it by a lens, and reflected into the telescope; this made a very bright spot, whose appearance or disappearance could be easily noted, and gave very satisfactory results.

In the report of the United States Coast and Geodetic Survey for 1881, C. S. Peirce describes a method which includes a scale divided into half millimetres, fixed on the clock pendulum, a lens attached to the other pendulum, an adjustable lens between them, and an eye-piece to view the image of the scale as given by the two former; this is capable of fine adjustment, and accuracy is claimed to the thousandth part of a second in finding the length of an interval.

In the report of the same survey for 1891, in connection with the invariable pendulums already mentioned, Prof. Mendenhall gives the full description of the method he used. By means of a couple of shutters, regulated by electrical connection with the chronometer, a flash of light from within a closed box was sent out at regular intervals against a mirror attached to the pendulum. If the flash came when the pendulum was in its lowest position, it was reflected into the telescope, but not at other times. As the flash was sent out every second, this was equivalent to finding the coincidences between the pendulum of experiment and a second's pendulum, the appearance of the flashes in the telescope showing the times of successive coincidences.

This same person had already used a new and interesting way of finding the time of an oscillation in his work in Tokio in 1880. A "trip-hammer" of very little weight could be introduced at any instant under the pendulum, so that when the lowest point was next reached the hammer was thrown, and an electric circuit broken; the instant of the break was recorded on a chronograph, which registered seconds. The "trip-hammer" was introduced at the beginning and end of the period of observation, so did not interfere with the vibrations between, and the time of an oscillation could be found by counting the whole number made in the time as registered. This method gave a very accurate determination of the time, several successive swings showing a variation of only the sixty-thousandth part of a second in the time for a number of oscillations, lasting about thirty minutes; it comes nearest to the ideal method in this respect, that the time is registered automatically, and is, therefore, not affected by any error in the observer's judgment.

II. Although Galileo established the laws of motion of the pendulum, it seems to have been, as one author remarks, more the result of intuitive genius than of accurate observations, for he failed to perceive the difference in duration of the oscillations, due to the amplitude of the arc, though his observations extended to arcs of ninety degrees on each side of the vertical. The abbot Picard, in his experiments about 1669, noticed that the duration of an oscillation was greater for a large arc than a small one, and tried to correct the error introduced through the diminishing amplitude by observing only very small oscillations, in which he was followed by La Condamine, and also by Bradley, 1743. In the theory of the pendulum, as given by Huygens, 1673, and by Newton, 1683, the oscillations are supposed to be executed in the arc of a cycloid, in which case no account need be taken of the amplitude. Bouguer made a number of experiments to determine the effect of the varying amplitude, and, besides finding the duration of the first vibrations, in an arc of more than three degrees, clearly longer than when the arc was small, he also discovered that the amplitudes, taken at equal intervals of time, showed a decrease in geometric progression. But the formula for the reduction of the time of an oscillation to that in an infinitesimal arc was first due to Daniel Bernoulli, who published it in 1747.

Neglected for a number of years as unnecessary, this correction was not used until recommended by Boscovich, who explained its necessity, and put it in the form of a geometric series, based on the law of the decrease of the amplitude in geometric progression, as stated by Bouguer. By Borda, through whom most of the suggestions of Boscovich regarding the pendulum found expression in actual experiment, this was further simplified, and the formula given in the form still used; though some doubt was cast on the correctness of this by Captain Sabine, a proof was given by Mathieu, in 1826, and it has been generally accepted since.

The effect of the fluid medium in which a pendulum oscillates was first studied by Newton, who caused a pendulum, ten and a half feet long, to oscillate in air, and also in water, first with a ball of wood, then with one of lead, and a similar one with a ball of iron to oscillate in water and in mercury. The result he arrived at was that the amplitude of the oscillation was affected by the resistance of the air or other fluid, but that there was no perceptible influence on the duration, the loss of motion by the pendulum during the upward part of its swing being compensated by its gain during the downward part. Some indications are found, however, of a correction by Newton to the length of the second's pendulum "ob pondus aeris"; the necessity for the correction was afterwards shown by Bouguer, and a method described to determine accurately its amount, from which it is sometimes called Bouguer's correction.

The error it is intended to overcome results from the loss of weight of the pendulum, due to the mass of air it displaces, and is spoken of by Kater as the error due to the "buoyancy" of the atmosphere, to correct which it is necessary to know the ratio of the weight of air displaced, to the weight of the pendulum. Bouguer determined the relative weight of the air to mercury by finding the height through which a barometer must be raised that the mercury should fall one line; then by comparing the specific gravity of the pendulum with that of the mercury, the relative weight of the pendulum to the air displaced was found. Kater made use of the known ratio of the densities of air and water, then found the mean specific gravity of the pendulum, and was able to calculate the ratio of the weight of air displaced to that of the pendulum. "That the number of vibrations in a vacuum might be the same as in air, it is necessary to increase the length of the pendulum in the proportion of this ratio to unity, since the length of the pendulum which oscillates in the same time varies as the intensity of gravity."

This was the only correction generally considered necessary to reduce the time of oscillation to vacuum until Bessel advanced the theory that the mass of air set in motion by a pendulum affects the duration of an oscillation. Similar conclusions had been reached by Buat, in 1786, who ventured to differ from Newton, and wrote concerning him and his experiments: "Il ne s'attacha qu'aux pertes des amplitudes, sans observer la durée des oscillations."

Bessel's argument is as follows: "Si l'on désigne par m la masse du corps qui se meut dans un fluide, par m' celle du fluide qu'il déplace, on a pris, depuis Newton, pour valeur de la force accélératrice qui agit sur le corps

$$\frac{m-m'}{m},$$

et c'est avec cette formule qu'on a toujours réduit les expériences du pendule.

"L'adoption de cette valeur est fondée sur cette hypothèse que la force motrice à laquelle le corps est soumis, et qui est égale à $m-m'$, se repartit sur la massa qui constitue le corps. Mais, en réalité, elle se repartit non seulement sur cette masse, mais aussi sur toutes les particules qui sont mises en mouvement en même temps que le corps ; donc aussi sur les portions du fluide qui sont entraînées ; par suite, le denominateur de la formule qui représents la force acceleratrice est necessairement plus grand que *m*."

After a long and intricate calculation, his conclusion is, in the words of Baily, "that a fluid of very small density surrounding a pendulum has no other influence on the duration of the vibrations than that it diminishes the gravity and increases the moment of inertia. When the increase in the motion of the fluid is proportional to the arc of vibration of the pendulum, this increase of the moment of inertia is very nearly constant ; in all other cases it will depend on the magnitude of the arc." It may be inferred, and has been found, that the amount of the correction for reduction to a vacuum will vary with the length, magnitude, weight, density, and figure of the pendulum, and also, in the case of the reversible pendulum, will not be the same for both positions. Bessel gives a formula to find the length of the pendulum reduced to vacuum :

$$\lambda = \frac{(i^2 + s^2)(m + km')}{s(m - m')}$$

where i is the radius of gyration, s is the distance of the axis of suspension from the centre of gravity of the pendulum, m and m' the masses of the pendulum and of the fluid displaced, and k a constant to be determined by experiments, in which m is made to vary and all other conditions remain the same.

Captain Sabine tested Bessel's theoretical correction in 1829, on a Kater's invariable pendulum, and found it too small. Baily, in 1832, made extended and careful experiments to find the ratio of the new correction for reduction to a vacuum to that before used. He tried many different pendulums, and varied the magnitude, figure, mode of suspension, etc., as much as possible, coming finally to the conclusion "that the amount of the required correction cannot (according to our present knowledge on the subject) be determined by calculations, but must in every case be determined by actual experiment."

Baily's experiments made to determine this ratio n, as he called it, corresponding to $1 + k$, where k is Bessel's constant in the formula for reduction, gave values ranging from 1.728 to 2.070 for different pendulums ; Sabine had found a value 1.655 for a Kater's invariable pendulum, and Bessel's formula gave a still smaller value.

C. S. Peirce, who studied the same subject, says in the report of the United States Coast and Geodetic Survey, 1876, that "the presence of the air lengthens the period of oscillation in no less than four distinct ways:

"1°, by its buoyancy;

"2°, by being carried along within enclosed parts;

"3°, by the hydrodynamic effect of its pressure;

"4°, by its viscosity."

"By the viscosity is meant what Stokes terms the index of internal friction, and Maxwell the kinematical viscosity." The account of an experiment to determine the amount of the correction to be applied to a Repsold's pendulum is given in the same paper, and a curve drawn showing its variations for temperature and pressure.

Another correction which has caused some discussion is that for the reduction to the level of the sea. Kater, one of the first to use it, says: "The force of gravity increases in the inverse ratio of the square of the distance to the centre of the earth." Young, in 1819, contended that some allowance should be made for the attraction of the matter above the sea level, and gave a formula to include this; the formula, as well as a fuller discussion, is given in another part of this paper.

Besides these corrections, that for the rate of the time-keeping apparatus is always applied, as well as one for variations in the length of the pendulum due to the changes of temperature; this last is usually determined by experiment. Many minor corrections have been used or investigated by different persons; their necessity in many cases depends on the conditions under which the experiment is performed. A complete list of the corrections to be applied to obtain accurate results was given by C. S. Peirce in 1876. Speaking of the time of an oscillation, he says: "The observed duration has to receive the following corrections:

"1°. The correction for the rate of the time-keeper.

"2°. The correction for amplitude of oscillation.

"3°. The correction for pressure and temperature of the air.

"4°. The correction for expansion of the metal by heat.

"5°. The correction for the slip of the knives.

"6°. The correction for the wear of the knives.

"7°. The correction for inequality of knives.

"8°. The correction for stretching of pendulum by weight of heavy bob when down.

"9°. The correction for the flexure of support.

"10°. The correction for attraction of the sun, moon, and tides.

"11°. The correction for elevation above sea level."

A discussion of each in turn is then given. (See United States Coast and Geodetic Survey, 1876.)

III. The first application of the pendulum arose from the use of the so-called simple pendulum as a measure of time. It was noticed in 1643, by Calignon de Perrins, a gentleman of France, that a pendulum thirty feet long, though left entirely undisturbed, performed each day alternate oscillations corresponding to the ebb and flow of the tides.

This phenomenon, known as reciprocation, was thought to be the proof of a slight mutation in the axis of the earth ; but the most careful experiments by M. Bouguer, P. Ximenes, and Andreas Mayer, about a hundred years later, proved only the existence of air currents, and of unequal expansion at different hours of the day of the walls from which the pendulum was suspended.

Next in order of time, but perhaps first in order of usefulness, came the application of the pendulum to clocks, it being substituted for the older balance now known only as a curiosity. Just within the Porta del Popolo, in the Passeggiata del Monte Pincio, at Rome, there stands yet one of the old-fashioned clocks, kept in motion by a steady stream of water which runs into a bucket and causes one end of the balance to fall at regular intervals.

The introduction of the pendulum marked such a great advance in time-keeping apparatus that the honor of having first used it has been claimed for a number of different persons, Galileo, Huygens, Bürji, as well as several local clockmakers. Galileo was certainly the first to conceive the idea of constructing a clock with a pendulum, and even gave his son Vincenzo, in 1641, a design for the mechanism of the apparatus ; but both father and son died before it had been carried into execution, and the design was only unearthed after more than two centuries by M. Alberi, who published it among Galileo's works in 1856. Huygens, without any knowledge of the great Italian's design, planned and had constructed, in 1657, the first pendulum clock, and to this noted physicist and astronomer, therefore, properly belongs all the honor of giving to the world this useful application of the pendulum.

The effect of changes in temperature on the rate of the pendulum clock was noticed by Picard in 1669 ; the gridiron compensation pendulum was invented by Harrison in 1725, and the mercury pendulum by Graham in 1726. Oerstedt, in 1809, and Jürgensen, a Danish clockmaker, from 1828 to 1832, studied the effect on the rate of the clock of variations in the density of the air ; various forms of compensating pendulums have been devised from that time to the present, the descriptions of which scarcely come within the sphere of the present paper, though it might be mentioned that a very high degree of accuracy has been attained.

By Newton, in 1686, an entirely different use was made of the pendulum; it became in his hands a means of proving one of the fundamental laws of attraction, viz., that the weight of a body is proportional to its mass, irrespective of the substance composing it; that is, that the force of gravity is not selective.

"Descensus gravium omnium in Terram (dempto saltem inæquali retardatione quæ ex Aeris perexigua resistentia oritur) æqualibus temporibus fieri jamdudum observarunt alii; et accuratissime quidem notare licet æqualitatem temporum in Pendulis."

His experiments were made with two pendulums, eleven feet long, the balls being of the same size and shape, and hollow, so that different materials could be put in them, while the resistance of the air remained constant. "Unam implebam ligno et idem auri pondus suspendebam (quam potui exacte) in alterius centro oscillationis, . . . et paribus oscillationibus juxta positæ ibant una et redibant dintissime." He tried the following different materials: gold, silver, lead, glass, sand, common salt, wood, water, and wheat; and the exactness of the work done may be inferred from his own statement: "In corporibus ejusdem ponderis differentia materiæ, quæ vel minor esset quam pars millesima materiæ totius his experimentis manifeste deprehendi potuit."

Since Newton's time, the pendulum has been recognized as an important instrument in the hands of the physicist on account of the relation between the time of an oscillation, the length of the simple equivalent pendulum, and the force of gravity. This relation is shown by the conclusion reached in Prop. LII., Prob. XXXIV., Book I. of the Principia:

"Itaque oscillationes in globis et cycloidibus omnibus, quibuscunque cum viribus absolutis factæ, sunt in ratione quæ componitur ex subduplicata ratione longitudinis fili directe, et subduplicata ratione distantiæ inter punctum suspensionis et centrum globi inverse, et subduplicata ratione vis absolutæ globis etiam inverse."

Applied to the attraction of the earth on a simple pendulum, this gives the well-known formula:

$$t = \pi \sqrt{\frac{l}{g}}$$

a demonstration of which is ascribed by M. C. Wolf to E. Jacquin, 1861.

The accurate determination of the length of the simple pendulum which beats seconds of mean time has been the cause of a very large number of pendulum experiments. The earlier experimenters, including Huygens, Picard, and many others, considered it an invariable length for all places on the earth's surface, which idea seemed to be confirmed by Picard's experiments in a number of places, extending from Denmark to the south of France.

The object of these men was to establish a system of measures of length with the length of the second's pendulum as the unit, and even when this was known to be variable the project was not given up; it was proposed by some to use the length of the second's pendulum at Paris, by others that at the equator, as the unit. It was not till 1791 that "l'Académie des Sciences," of Paris, decided that the second's pendulum should not be chosen as the unit of length. The reasons given by "les Commissaires" were:

"1.° Que cette longeur repose sur l'adoption d'une unité tout à fait arbitraire, la seconde de temps;

"2.° Que sont adoption fait intervenir dans la détermination de l'unité de longeur deux considérations qui lui sont étrangères, celle du temps et celle de l'intensité de la pesanteur."

Whether the reasons as given were altogether valid or not, it seems certain that much confusion would have been caused among those using the metric system by having a unit so difficult to determine accurately, or so liable to repeated corrections. C. S. Peirce, however, speaks quite decidedly to the contrary: "I will even go so far as to say that a physicist, in any remote station, could ascertain the length of the metre accurately to a one-hundred-thousandth part more safely and easily by experiments with an invariable reversible pendulum than by the transportation of an ordinary metallic bar."

But before it had been decided not to use the length of the second's pendulum as a standard of measurement, new interest in pendulum experiments had been created by the fact that an absolute determination of this length was equivalent to an absolute determination of the acceleration of gravity, which is probably the most wonderful result given to physical science by the pendulum. There has always been a charm about such a work as finding the absolute force of gravity. Major Herschel, in *Nature*, 1880, says: "There is something seductive, it must be admitted, in the conciseness and completeness which attends an absolute determination, as contrasted with the dependence of a differential one."

The first determination of the length of the second's pendulum was made by P. Mersenne, in 1644; the fact that its length is variable was shown experimentally by M. Richer, in 1672; and Newton's famous law of universal gravitation gave the reason for this variation in the fact that the attraction of gravity increases as the distance from the centre of the earth increases. Assuming the earth to be a homogeneous spheroid, whose axis varies continually in length from the equator to the poles, Newton established that "incrementum ponderis pergendo ab Æquatore ad Polos sit quam proxime ut sinus versus latitudinis duplicatæ; vel quod est, ut quadratum sinus reti latitudinis. . . . Gravitas ad Polum sit ad gravitatem sub Æquatore ut 230 ad 229, et excessus gravitatis ad Polum ad gravitatem sub Æquatore ut 1 ad 229." The length of the second's pendulum being proportional to gravity, these results give two formulæ:

$$l_\phi = l_\circ\,(1 + m \sin^2\phi)$$
$$l_\phi = l_{45}(1 - m' \cos^2\phi)$$

where $l_\circ$ = length of second's pendulum at the equator,
l_{45} = length at latitude 45°,
l_ϕ = length at latitude ϕ,
$m = \frac{1}{229} = .00437$,
$m = \dfrac{\frac{1}{2}m}{1 + \frac{1}{2}m}$.

Clairant showed that the law of the square of the sine of the latitude, as given by Newton, held in the case of homogeneous concentric shells; Laplace, in "Mecanique Celeste," Livre III., established a formula previously given by Clairant for the determination of m:

$$m = \frac{5}{2}\frac{f}{g_0} - \frac{a-b}{b},$$

where f = acceleration of the centrifugal force at the equator due to the earth's rotation;
g_0 = acceleration of gravity at the equator;
a and b = the equatorial and polar semi-axes.

In his "Collection des Memoires relatifs à la Physique," M. C. Wolf gives a table of 23 different values of m, m', and l_0, which have been calculated by various mathematicians from Newton, in 1713, to Helmert in 1884. From these he gets mean values of the three quantities which give Newton's formulæ, as follows:

$$l_\phi = \overset{\text{mm}}{991.000}\,(1 + .00520 \sin^2\phi)$$
$$l_\phi = 993{\cdot}577\,(1 - .00259 \cos 2\phi)$$

These give a connection between the latitude of a station and the length of the second's pendulum there, based on an assumption of the mean figure of the earth; and many experiments have been conducted, first, to test the hypothesis; second, to determine the real figure of the earth. A large part of the variation in the value of g, as found experimentally, from that given by the formula was found to be due to the height above the sea level, and in most cases, according to C. S. Peirce, when "the value of gravity upon high lands and mountains is corrected for difference of centrifugal force, and distance from the earth's centre, it is very little greater than at the sea level," as given theoretically.

There were cases, however, which could only be accounted for by the increase of the force of attraction due to the density of the land or mountain on which the observation was made being greater than the mean density of earth. Bouguer had conducted experiments from 1737 to 1740 to determine the variations in the length of the second's pendulum for three different altitudes in Peru. In space free of matter, gravity might be expected to decrease according to the inverse square law starting from the centre of the earth, but was observed to decrease more slowly; this excess of gravity above its value in a free space was thought to be due to the attraction of the matter above the sea level. Bouguer obtained a formula for the value of gravity on a plateau of height h, as compared with that at sea level,

$$g_1 = g\left(1 - \frac{2h}{r} + \frac{3h}{r}\frac{d}{D}\right)$$

where

r = radius of the earth.
d = density of plateau.
D = mean density of the earth.
g = value at sea level.

This formula was revived by Young, and applied in 1819 to make a correction to Kater's value of the length of the second's pendulum at London, d being assumed to be 2.5 and D, 5.5. He put it in the form

$$\frac{g - g_1}{g} = \frac{2h}{r}\left(1 - \frac{2}{3}\frac{d}{D}\right)$$

An interesting application was made of this attraction of matter above the sea level to discover the mean density of the earth. M. Carlini, in 1821, determined the value of gravity on Mont Cenis, and used M. Biot's result, taken at Bordeaux, for the length of the second's pendulum, for comparison with his own value. Correcting Biot's result for the latitude of Mont Cenis, and assuming the mountain to be the segment of a sphere 1 mile high with a base 11 miles in diameter, he found D in terms of d.

In 1826, Mr. Airy (afterwards Sir George Airy and Astronomer Royal) thought that this mean density might be determined by observing the difference in the rates of a pendulum at the top and bottom of a mine. After several unsuccessful attempts, the experiment was conducted, in 1854, at the Harton Pit, a coal mine in the county of Durham, England, and a value of 6.565 was found for D when d was assumed equal to 2.5.

Major Von Sterneck tried the same method from 1882 to 1885, in the mines at Pribram, in Bohemia, and Freiberg, in Saxony. His work, though conducted with the greatest possible care, gave values for D ranging from 5.66 to 7.60; d was assumed as 2.75. He attempted to explain this variation only by the hypothesis of an underground variation in density, which could not be determined.

Prof. Mendenhall, in 1880, made determinations of the relative values of gravity at Tokio, and at the summit of Fujiyama, by means of a Kater's pendulum divested of everything but the heavy weight and the corresponding knife-edge. This mountain was chosen from the regularity of its shape, and the result obtained as accurately as can be expected from this method. The value of d was estimated from specimens at 2.12, and D found to be 5.77.

There is considerable uncertainty in the assumption of the mass and density of the matter above sea level, and this, with Airy's view, shared by C. S. Peirce, "that mountains are not additions to a spheroidal earth, but the exposed parts of lighter matter buoyed up," leads to Poynting's conclusion, "that our knowledge of the distribution of the terrestrial matter is not yet sufficiently accurate to enable us to obtain good values of the mean density from the observed attraction of terrestrial masses."

Considerable doubt was thrown on the accuracy of Young's formula for reduction to sea level by Wm. Sterneck's calculations, and the necessity for the term $\left(\frac{3h}{r}\frac{d}{D}\right)$ called in question. The formula is now used in the form

$$\frac{dg}{g}=\frac{2h}{r}\left(1-\frac{3}{4}\frac{d}{D}\right)$$

that is, the doubtful term has been decreased one-half.

In connection with mean density of the earth, the recent work of Prof. C. V. Boys, F.R.S., with the torsion balance, is of great interest and importance, as showing a method more likely to give accurate results.

The application of the pendulum which produced greatest excitement among physicists was perhaps Foucault's "Demonstration physique du mouvement de rotation de la Terre au moyen du Pendule," which appeared in 1851; no less than 78 memoirs were published that same year, and 31 the following year, as compared with 7 in 1849, and 3 in 1850. He showed the rotation of the earth by means of the rotation of the plane of oscillation of a pendulum, free to oscillate in any plane through its axis.

In *Nature*, XXII., 1880, Mr. C. R. Cross describes an interesting method of obtaining a permanent record of Foucault's pendulum experiment. A pendulum 16 feet long, consisting of a 5-pound cannon ball suspended by a wire, was made to oscillate; the ball carried a bristle, which served as a marker. At intervals of 15, 30, or 60 minutes, a smoked glass was raised to receive the markings of the bristle, care being taken to give the glass no rotation; the inclination of the tracings was clearly shown.

Without reference to other minor applications of the pendulum, and the anomalies which appear, "defying all attempts at explanation," this sketch will be concluded by the chief reasons now given for continuing to conduct experiments for the determination of gravity, absolute or differential. In a conference on gravity determinations held in Washington, in 1882, the following persons took part: J. E. Hilgard, Superintendent of the United States Coast and Geodetic Survey; Major Herschel, R.E.; Prof. C. S. Peirce, Prof. S. Newcomb (on the part of astronomy); and Messrs. George Davidson and C. A. Schott (on the part of geodesy); Major J. W. Powell, Director of the United States Geological Survey (on the part of geology), was unable to attend.

A number of reasons were given for the prosecution of pendulum experiments:

1°. The determination of the figure of the earth.

2°. The obtaining of a means to preserve and transmit to posterity an exact knowledge of the length of the yard and metre.

3°. The inferences which may be drawn in regard to the geological constitution of underlying strata.

4°. The use of gravity as the unit of force, and the consequent necessity for the accurate determination of its variations.

5°. The perfecting of our knowledge of the earth's crust, so that we may be able to infer from the vertical attractions of mountains what their horizontal attraction must be, and the deflection produced on a plumb line.

6°. The relation of the force of gravity to latitude and longitude. "The force of gravity is related in the same way to the latitude and longitude as the intensity of magnetic force is related to magnetical declination and inclination, and as a magnetical survey would be held to be imperfect in which measurements of intensity were omitted, to the same extent must a geodetical survey be held to be imperfect in which the determinations of gravity had been omitted."

The principal object among the foregoing is that of determining the actual figure of the earth, and its variations from the supposed mean figure. "Bearing in mind the large body of past work which has undoubtedly sufficed to indicate very closely what the mean figure is, it should now be recognized as more particularly the object of pendulum research to enlarge our knowledge of the *irregularities of figure*, rather than to aim at improving the *mean* figure."—Major Herschel.

For this purpose the invariable pendulum was most strongly recommended, as giving results in every way comparable.

The general opinion was that the probable accidental error in the results should not exceed $\frac{1}{200.000}$, and the total error to be feared $\frac{1}{100.000}$ of the value of the gravity. Four classes of errors must be taken into consideration :

1°. Those nearly constant throughout the work at one station, arising from flexure of support or other fixed conditions.

2°. Those constant for a time, but varying from day to day, as the temperature, pressure, etc.

3°. Those continually varying through the observation.

4°. Those arising from the comparison of the pendulum with the timepiece.

The reasons given by this conference for the prosecution of pendulum research, and the problems to be solved by its aid, tend to show more than ever the value of continued and accurate experiments. "L'Astronomie, la Géodesié et la Géologié sont toutes trois intéresseés à l'extension des mesures du pendule."

THE ACCOUNT OF AN EXPERIMENT TO DETERMINE THE ACCELERATION OF GRAVITY AT TORONTO BY MEANS OF A KATER'S PENDULUM.

This experiment, which has been extended over a period of three months or more, up to the present, has been conducted in a room known as the instrument room, on the ground floor of the School of Practical Science, situated at a distance of 500 feet from the public street. The room is about 30 ft. × 27 ft., with a ceiling 11 ft. high, is lighted by four large windows, all fitted closely with double sashes, and has only one doorway, which opens into the large entrance hall. To prevent currents of air, or sudden changes of temperature, this doorway is provided with two doors with about a foot of space between, each fitted with a sliding panel in the lower half; these leave two openings 18 in. × 6 in., not, however, opposite each other, and provide for the passage of air. It might be mentioned, also, that great care is exercised in the heating of the room to keep it at an even temperature. During the time of the experiment the thermometer varied from 61° to 67° F., with an average of 64°·5°, and no sudden changes.

The instruments used in the experiment were:

1. The Kater's pendulum.
2. An astronomical clock.
3. A chronograph.
4. A comparator with microscopes.
5. A standard bar.
6. A telescope mounted on a tripod.
7. A small lamp.
8. A barometer and several thermometers.

The Kater's pendulum, of which a drawing is shown (Fig. 1.), was manufactured by Nalder Bros. & Co., Clerkenwell, London, England. It was ordered for the School of Science in July, reached here in October, 1895, and its first use, involving the learning of all its peculiarities, has been by the writer for the determination of gravity. It consists of a cylindrical rod made of steel, 50¾ inches long, and a little less than ½ in. in diameter; in each end is screwed a small brass pointer to indicate the size of the arc of vibration. There are three weights, all movable, two of equal size, cylindrical in shape, and having an outer surface of lacquered brass; of these the heavier, weighing 1,107 grams, is near one end of the rod and outside the axis of suspension, the other, weighing 225 grams, is near the opposite end and between the axes of suspension. They are held in position by brass screws, which press against the rod. The third weight is about midway between the axes of suspension, and is a slider, the outside of which is covered with a thread bearing three nuts, one to bind the slider in position by means of a split at one end, the others movable, and serving to hold each other in place; the slider weighs 120 grams.

The axes of suspension are knife-edges, made of very fine steel, the two axes as nearly alike as possible; each consists of two triangular prisms, the faces terminating in the knife-edge having an inclination of 60°, and an ultimate angle of about 120°. The base of these prisms, that is, the face opposite the knife-edge, rests against a support consisting of a brass plate with rounded ends, about 2½ inches long, 1⅝ inches wide, and ¼ inch in thickness. This plate is joined to a collar of brass ⅝ inch long and an inch in diameter, which forms with the plate one solid piece. This support of the knife-edges is pierced by a cylindrical opening, into

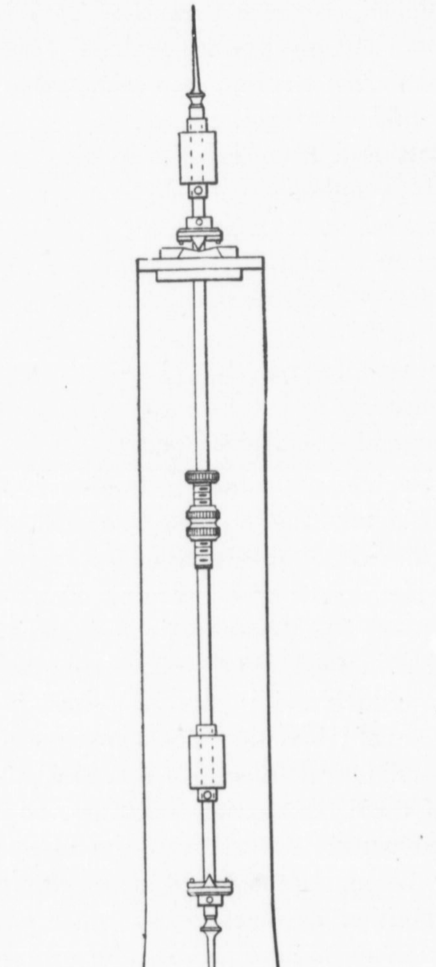

Fig. 1

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which the rod of the pendulum fits closely; three steel screws pressing against the rod hold the support firmly in position. Each knife-edge is fastened to the brass plate by means of two smaller plates with bevelled edges, which press upon the sides of the knife-edge, and prevent it from moving in any direction; each of the smaller plates is held in place by two screws, which, like the former ones, may be tightened either by a screw-driver or an adjusting pin. The two knife-edges, forming one axis of suspension, can be adjusted to have the same level, and be exactly in line before the block holding them is put on the rod.

The stand on which the pendulum swings consists of a column of cast steel, 4 feet high, 11 inches wide at its lowest part, 3½ inches wide at the top, ½ inch thick, strengthened by a buttress at the back reaching nearly to the top. The column rests on a rectangular metal base, 19 inches long, by 14 inches wide, and 1 inch thick, to which it is fastened by strong bolts. In the base are two levelling screws, as shown in the drawing, and a third point of support, consisting of a small leg or projection, at the side opposite the levelling screws. The top of the column is turned so that it ends in a horizontal plane 4½ inches by 3½ inches, nearly over the centre of the base, this small plane being steadied by a metal brace at one side, the whole column, buttress, horizontal plane, and brace, being cast in one piece. A rectangular opening is made at one side in this upper plane surface, into which the pendulum rod passes.

The plane of suspension consists of two pieces of agate, one for each knife-edge, forming an axis of suspension. Each is longer than the knife-edges, which are an inch long, and is wider at the base than at the top, being held in position by clamps made of brass plates, with bevelled edges; these are screwed firmly to the upper surface of the stand. The upper surface of the agate is **V** shaped, and the bottom of the **V** has been carefully prepared as a plane surface; this shape ensures the knife-edges resting on the same part of the plane at successive swings. The distance between the agates is slightly less than the distance between the knife-edges, so that the whole length of each knife-edge rests on the plane.

The stand of the pendulum rests on a stone cap 19 inches square by 6 inches thick, surmounting a brick pier, which is built on the solid ground beneath the floor, and extends to a depth of 2½ feet.

The astronomical clock used for observing the coincidences was manufactured by E. Howard & Son, Boston, Mass.; it is fastened to a heavy stone pillar, 10 ft. long, 2 ft. wide, and 1 ft thick, which is set in a solid brick pier 6 ft. square and 3 ft. deep, resting on a concrete foundation 6 in. thick. The pier is independent of the floor or walls of the building, and the clock is supposed not to be affected by the machinery in the School. To ensure greater accuracy in the time, the rate of the clock was compared each day during the time of the final observations with the Observatory clock, with which it has electrical connection; for this the writer is indebted to the kindness of Mr. Blake of the Observatory staff.

The chronograph used was in circuit with the clock, and registered automatically every two seconds, as measured by the clock; the circuit also included a break-circuit key, by which the observer could register the exact time of a coincidence without leaving his post at the telescope.

The comparator was used to measure the distance between the knife-edges on the pendulum; it was manufactured in Waterville, Maine, and put in position by Prof. Wm. Rogers. It consists of a heavy bed of iron about 10 ft. long, and supported on three piers; this bed has a way or track along which a carriage bearing a microscope can be moved on a horizontal plane in a straight line. Two methods of measuring may be adopted; in one the carriage is moved a certain fixed distance, determined by stops securely fastened in the track, the points of contact between the carriage and the stops being small projections of very hard steel. By means of a movable cross-thread a comparison is then made of this fixed distance first with the distance to be measured, and also with a length on the standard bar, by which the required distance is found in terms of some known length on the bar. In the other a second microscope is used, and the fixed distance is that between the cross-hairs of the microscopes. During the comparison of the required length with the standard bar, neither of the microscopes is moved, the comparison being made wholly by means of the micrometer screws and the movable cross-threads. The microscopes were manufactured by the Bausch & Lomb Optical Co., New York and Rochester.

The standard bar was made by Prof. Rogers in his laboratory at Waterville. It is a copy of a copy of the English standard yard kept at London, and the French standard metre, the first copies of which were made by Prof. Rogers at the request of the United States Government for comparison with the standards at Washington. This was bought for the School of Science, and was accompanied by a full description of the methods used and the care exercised in making it, as well as the first copies, also some comparisons with the originals which he has been able to make since that time. According to him, the metre (the one used in this experiment) is standard at a temperature of 14°.66 C., and expands 10.246 mikrons for 1°C. He also gives a table showing the correctness to the successive decimetre divisions.

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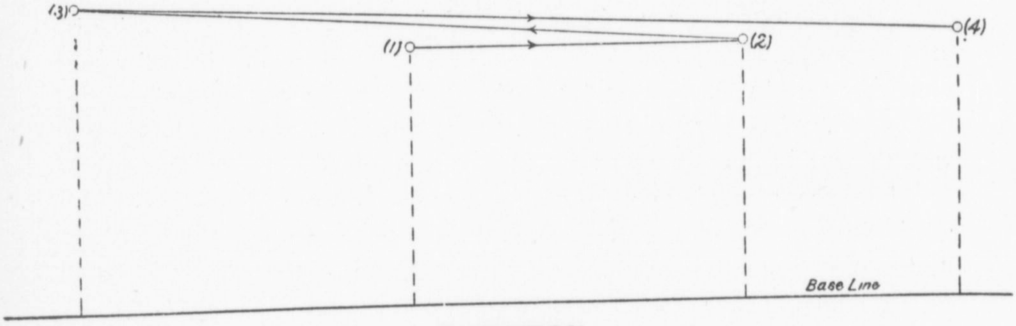
An ordinary surveyor's telescope mounted on a tripod was used for observing the flashes marking the coincidences. A small lamp belonging to a transit, with a lens sending out parallel rays, was set on another tripod on a small plane, whose height could be adjusted by a screw, to form the source of light.

The thermometers used were loaned by Mr. Rosebrugh, two Centigrade and one Fahrenheit. As these agreed with one another and with the one attached to the barometer, no test was made to determine any correction. The barometer is a fixture in the room, and was compared with the one in the Observatory when put in position.

The accompanying cut (Fig. 2) shows the interior of the room and all the instruments except the lamp and the chronograph, as well as some other instruments kept in the room.

The first work in connection with the pendulum, after it had been set up, was to make the adjustment of the weights to bring the intervals between coincidences the same, or approximately so, for both positions. In order to avoid the error introduced by variation in the rate of the clock, an interval of about an hour was considered most suitable, and, assuming the length of the second's pendulum to be 39.11 inches for this latitude, the knife-edges were set a distance apart of 39.07 inches approximately. A great many observations were then made, first leaving the heavy weight fixed, and moving the lighter weight by short distances, over quite a length, to find the effect on the intervals, then moving the heavy weight slightly, and repeating the process. So far as the observations extended, any change in the position of the lighter weight produced the same effect on both intervals, that is, both were lengthened, or both shortened, the change when the heavy end of the pendulum was up being always greater than when it was down. The observations at this stage were made with the Kater's pendulum standing on the floor beside the clock, and the times of the coincidences of the two estimated as nearly as possible by the eye. Much time was spent at this work, as it took so long to make a single observation; at length the intervals were brought approximately together, and the next step was to get a more accurate method of observing the coincidences. The method then adopted, and adhered to throughout the experiment, was as follows:

The Kater's pendulum was set about 13 feet from the clock, almost in line with it at one side, and in such a position that the two pendulums oscillated in planes making a very small angle with each other. To each pendulum was attached a small mirror, as near the point of suspension as possible. About midway between the two the lamp was mounted on the tripod, so that the light from it fell on the mirror attached to the Kater's pendulum, was reflected to the one on the clock, and again reflected through the telescope to the eye of the observer, who stood about 17 feet from the clock. The plan and elevation showing the path of a ray of light from the lamp to the eye of the observer are given in the drawing (Fig. 3).



ELEVATION

PLAN

Fig. 3

The mirrors used were small, circular pieces of glass, about ⅝ inch in diameter, silvered on one side; the lens of the lamp was about the same size; but, though the source of light was only a small olive oil flame, it was found that too much light was being reflected to permit the accurate observation of a coincidence. To remedy this, a paper blackened with India ink was fastened over the lens of the lamp, leaving a horizontal opening ⅜ inch long and $\frac{1}{25}$ inch wide, in the centre of the paper, for the light to emerge. Similar papers with slits of the same size were put on both mirrors; later, another paper with a slit of not more than $\frac{1}{50}$ inch in width was put on the clock pendulum.

The adjustment of the apparatus was made when the Kater's pendulum was at rest; that is, at its lowest position for every swing, irrespective of amplitude. First, the light was moved to the right or left, till the reflection from the first mirror came in the vertical plane passing through

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the two mirrors; then its height was adjusted, till this reflection fell exactly on the slit in the second mirror, after which the telescope had to be adjusted to catch the second reflection. Some idea of the accuracy of adjustment required may be gathered from the small size of the pencil of light allowed to emerge from the lamp, and the still smaller size of the uncovered portions of the mirrors. It was found that a change of half a turn, or even less, of the screw which adjusted the plane holding the lamp produced a noticeable difference in the regularity of the coincidence intervals; that is, a change of $\frac{1}{100}$ of an inch in the height of the source of light could be detected in the course of an observation lasting forty minutes. Great care was necessary, also, that the wick should be the right height in the lamp, and the lamp itself placed in the proper position on its support. As it was necessary not to stop the clock, this adjustment was considered accurate when, with the Kater's pendulum absolutely at rest, a bright flash could be seen on the clock mirror at every swing of the pendulum. A still more delicate test of the accuracy of adjustment was found in the course of the experiment, which will be mentioned.

On account of the uniformity in the amplitude of the oscillations of the clock pendulum, it was not considered necessary to see the flash reflected from it at the instant it passed through its lowest position. The observations were taken when the clock pendulum was about one degree from the vertical; it was found that each time the two pendulums, while swinging, occupied these respective positions, that is, the Kater's, its lowest or the vertical position, and the clock pendulum the proper position to reflect into the telescope, the flash of light could be seen several times at intervals of two seconds; the time midway between the first and last flash was that taken for the coincidence.

Four coincidences occurred during a single interval:

1°. Both pendulums swinging to the right.
2°. Both pendulums swinging to the left.
3°. Kater's to the left and clock to the right.
4°. Clock to the right and Kater's to the left.

The interval or the time between corresponding coincidences was found by taking the difference between the times of the first and fifth, second and sixth, third and seventh, etc., as indicated on the chronograph. When the height of the lamp was just right, the time between the first and second coincidences was the same as that between the third and fourth, and that between the second and third the same as between the fourth and fifth. It was in this way that a change in the height of the lamp could be detected, or the delicate test before mentioned applied to adjust it to the proper height.

The foregoing method of observing coincidences recommends itself on the ground of being cheap, and, under the circumstances of the present experiment, easily prepared, though the extreme care necessary in making the adjustments, as well as keeping them, would show the necessity for more permanent fixtures in some respects. The method is believed to differ from any elsewhere described, and offers the advantage of helping to eliminate errors of observation by the number of coincidences occurring in each interval.

When the apparatus had been arranged, and observations were begun to determine the time of the coincidences accurately, it was found that the number of flashes which appeared at each coincidence was so great as to produce uncertainty concerning the time midway between the first and last flashes, for which reason it was decided to alter the distance between the knife-edges to make the interval about ten minutes instead of an hour. This was done with the assistance of Mr. Stewart, lecturer in the School of Science, the knife-edges being set at a distance apart of 38.85 in. approximately. This opportunity was taken to get the weight of the pendulum and each of its parts. A careful readjustment of the knife-edges was also made to get the two forming each axis of suspension exactly in line, also to get the two axes of suspension in the same plane; for these purposes the horizontal plane surface of the comparator was used as the testing surface. On setting the pendulum up again, its rate of oscillation was found to be so changed that only three flashes in general showed through the telescope at each coincidence; sometimes two, and sometimes four, were seen.

The work of adjusting the weights was then begun again to make the interval the same for both positions; this time the work proceeded more rapidly. To avoid correcting for amplitude at this stage, the pointer at the lower end of the pendulum was always pulled aside to the same mark on the scale when being started. The scale, which was not mentioned in the description of the pendulum, is graduated in degrees and tenths of a degree; it extends to three degrees on each side of the vertical, and is bolted to the base of the pendulum stand in such a position that the pointer hangs just before it.

In starting the pendulum, it was pulled aside to a certain mark, allowed to rest a moment, and then oscillated freely by its own weight; a lead pencil or small ruler was generally used to push it aside. Special care was taken not to touch the pendulum, or any part of it, with the bare hand; to lift it off in reversing a chamois was always used.

When the intervals had approached within about half a minute of each other, some disturbing influence was found to be at work, which so affected the pendulum in the position with the heavy end up that the interval showed a variation of more than a minute without any change in the weights. This was ascribed successively to various causes, the machinery in the building, the jarring due to the street cars, changes in temperature, and irregularities in the knife-edges ; and a series of experiments, lasting more than a week, was conducted under all different conditions possible, to locate it definitely. No flaw could be detected in the knife-edges, and after experimenting on Sunday, when street cars and machinery were both stopped, and the temperature did not differ materially from the previous day, attention was turned to the agate planes, forming the plane of suspension, which, up to this time, had been assumed to form a perfectly level surface.

A careful examination of these showed that one agate was slightly too thin at one end, nearly $\frac{1}{100}$ of an inch, so that while one knife-edge rested through its whole length on the plane of suspension, the other rested only on a short distance at the higher end of the agate. To remedy this, it was assumed that the two knife-edges forming an axis of suspension were exactly in line, and perpendicular to the axis of the rod, as this adjustment had been very carefully made. The thin end of the agate was then raised slightly by thicknesses of paper, which was used as being a substance easily procured of uniform thickness. Two tests were applied to find when the knife-edges rested evenly on the agates: one by observing when the line of light, which showed where the two substances met, was uniform ; the other by exerting pressure, to see if any motion of the end of the pendulum, perpendicular to its plane of oscillation, could be produced. When the proper number of thicknesses of paper had been inserted that both tests were satisfied, the agates were adjusted to bring the level surfaces at the bottom of the **V**'s exactly in line, and the adjustment of the weights was continued. Though the tests were not as rigid as might be desired, the absence of any appreciable irregularity in the time of the interval since this change was made in the agate tends to show that a high degree of accuracy was obtained.

When the intervals came within two or three seconds of each other, one being $10'\,8.''5$, and the other about $10'\,10.''5$, preparations were made for taking the final readings. Up to this time the only weight moved in the adjustment for this interval was the lighter of the two large weights, and it was supposed that the intervals could be brought together by moving the nuts of the screw of the slider. The following table shows the way in which the observations were recorded, and gives the results of the first swing intended to be used as a final observation :

SWING NO. I. A—HEAVY END DOWN.

Coincidences.	Semi-arc in Minutes.	Temperature.	Pressure in mm.	Interval.	Period in Seconds.	Correction for Amplitude	Period Corrected.
5h 58′ 23″							
59′ 26″	61	18.°C	745.25	....	....	....	....
6h 3′ 29″							
4′ 30″	58			....	....	....	....
8′ 32″							
9′ 34″	55	....	....	10′ 8.5″	.9967240	− 211	.9967029
13′ 39″							
14′ 38″	52	....	....	10′ 9″	268	− 159	109
18′ 41″							
19′ 44″	49	....	....	10′ 9.″5	292	− 142	150
23′ 47″							
24′ 48″	47	....	....	10′ 9″	268	− 129	139
28′ 49″							
29′ 54″	45	....	....	10′.9″	268	− 116	152
33′ 55″							
34′ 56″	43	17.°8	746	10′ 8″	215	− 106	109

Mean period .9967115

I. B—HEAVY END UP.

Coincidences.	Semi-arc.	Temperature.	Pressure.	Interval.	Period Uncorrected.	Correction	Period Corrected.
5h 13′ 9″							
14′ 10″	52	18°	745	....	....	....	....
18′ 15″							
19′ 16″	47			....	....	....	....
23′ 21″							
24′ 22″	42	....	....	10′ 12″	.9967425	− 116	.9967309
28′ 25″							
29′ 28″	38	....	....	10′ 11″	375	− 95	280
33′ 31″							
34′ 32″	35	....	....	10 ′10″	320	− 78	242
38′ 35″							
39′ 38″	32	....	....	10′ 10″	320	− 64	256
43′ 41″							
44′ 42″	29	....	....	10′ 10″	320	− 54	266
48′ 45″							
49′ 48″	26	18°	745.25	·10′ 10″	320	− 44	276

Mean period .9967271

The temperature was read at the beginning and end of the swing in each position from two thermometers attached to the pendulum stand, one at the top and one at the bottom; the one on the barometer was used as a check on the others; the pressure was read on the barometer at the same time. The reading for the amplitude of the semi-arc was taken by the eye always between the two coincidences, which came near together, as a result of the flash being seen when the clock pendulum was not at its vertical position.

The mean of the two intervals from the 1st to the 5th and 2nd to 6th coincidence was taken as the time of the interval, with initial and final amplitudes of the corresponding semi-arcs. The reduction to infinitely small arc was made on the period or time of a single oscillation by an adaptation of Borda's formula:

$$\nu = \frac{PM \sin \overline{\phi + \phi'} \sin \overline{\phi - \phi'}}{32 (\log \sin \phi - \log \sin \phi')}$$

where ν = correction to be subtracted from P,
P = period of a single oscillation,
M = modulus of common system of logarithms,
ϕ = initial semi-arc,
ϕ' = final semi-arc.

When five such swings had been made, it was discovered that one knife-edge was slightly loose; as it could not be ascertained when it had become so, it was decided not to use these results. After the screws had been tightened to hold this knife-edge firmly in place, the others were tested to see if they were solid. The lighter weight was then moved very slightly, to bring the intervals more nearly together, with the result as shown in the sixth and successive swings. It might be mentioned that every swing made from this time to the end of the experiment is recorded in the following table, p. 115. The attempt had been made throughout the work to determine the effect of the machinery, and of the jarring of the street cars, on the time of an oscillation; but though it is believed, and not without evidence, that these made the pendulum oscillate more slowly than it otherwise would, the variation in the time of the period due to the knife-edges resting in a slightly different way on the agates was sufficient to prevent the effect of the other disturbing causes from being measured accurately. To avoid, therefore, introducing an error which could not be corrected, all the swings, with the exception of one or two, were taken when the machinery in the building was not running, and four were taken on Sundays, that there might be the minimum amount of disturbing influence.

When the results of the first two swings had been calculated and found to agree more closely than had been considered probable, it was decided to use every means to prevent any personal judgment or prejudice from affecting the results.

Care was taken not to know the exact time of a coincidence or the length of an interval until the paper was taken off the chronograph, and the continued agreement of the results from day to day, for nearly two weeks, was accepted by the observer as evidence that his judgment was not materially affecting the results.

During the time of swings XVII. B and XVIII. B, the gas engine was running in the building; this rests on a foundation on the ground about 25 or 30 ft. from the pendulum, and was thought to be the cause of the period in B of both these swings being lengthened as compared with A. A good example of the effect of a disturbance caused by jarring is shown in the next table, showing the times of coincidences for the position B. Just after the eighth coincidence had been noted, the new stamp mill in the School of Science was started, and continued to run during the rest of the swing.

B.—FEB. 22ND.

Coincidences.	Intervals.
3 h. 9′ 46″	
10′ 46″	
14′ 47″	
15′ 47″	
19′ 50″	10′ 4″
20′ 50″	
24′ 51″	10′ 4″.5
25′ 52″	
29′ 54″	10′ 4″.5
30′ 55″	
34′ 57″	10′ 5″.5
35′ 57″	
40′ 1″	10′ 6″.5
41′	
45′ 3″	10′ 6″.5
46′ 4′	

The effect was seen at once in the lengthening of the interval, and what this jarring was found to do to a noticeable extent was ascribed in a lesser degree to the jarring produced by the gas engine and the dynamo. Unfortunately for the theory as applied to the gas engine, the same discrepancy occurred between the periods in one of the swings taken on Sunday (XX.) as in the two (XVII. and XVIII.) taken with the gas engine running, from which it was concluded that some other unknown influence was at work, probably some irregularity in the knife-edges.

After swing No. XI. the nuts on the slides were moved a half-turn towards the heavy weight to reduce the period in B, which up to this time was slightly longer than the period in A. After XIV. they were given a quarter turn from the heavy weight, and were not again moved during the experiment.

In order to estimate the probable accuracy of the value of the acceleration of gravity, commonly called g, as calculated from the foregoing results, the effect of an error of one second in the length of an interval of ten minutes was calculated from the formula

$$t = \pi \sqrt{\frac{l}{g}}$$

whence by differentiating and dividing is got the relation

$$\frac{dt}{t} = -\frac{1}{2}\frac{dg}{g}.$$

Substituting the values of dt, t, and g

$$\frac{.0000055}{.9966780} = -\frac{1}{2}\frac{dg}{980.xxx}$$
$$dg = .001$$

that is, an error of one second in the interval produces a corresponding error of .001 cm. in the value of g. As the error in the mean of the seventeen swings is believed to be less than one second in the interval, the value of t is thought to be accurate enough to give g correct to the third decimal place in centimetres.

Two corrections besides that for the amplitude of oscillation were applied to the period as given by each swing:

1°. For the rate of the clock.

2°. For the temperature.

The rate of the clock was found each day except Sunday during the time of the experiment by a comparison with the Observatory clock generally twice a day, about 3 or 4 p.m., and about 8 p.m.

The error in the rate was calculated from these comparisons, in seconds lost per hour, usually about .03 per hour. A noticeable variation in the rate will be seen in that given for February 18th. It was found afterwards that the stamp mill was running in the School of Science when one of the comparisons was made, and to this is ascribed the change.

In the correction for temperature the coefficient of expansion for the steel rod of the pendulum was assumed to be that given by M. J. René Benoît in the "Travaux et memoirs de bureau international des poids et mesures," Vol. VI., 1888. The value of the coefficient of expansion was determined by a method devised in 1864 by M. Fizeau, of which Benoît says, "Le principe de la méthod de mesure imaginée, en 1864, par M. Fizeau, consiste, comme on le sait, à faire servir à la détermination exacte de trés petites variations de longeur les modifications qu'elles impriment à des franges d'interference produites entre deux surfaces planes et parallébes."

For "acier, recuit," he gives the value $10^{-9}(10353+5.80\ t)$ as the "moyenne de deux déterminations très concordantes, faites sur deux échantillons d 'acier, l' un francais, l' autre anglais, dans un intervalle de température plus considérable" (from 0° to 80°-85°). For the pendulum rod it was assumed $10^{-9}(10451.6)$ for the temperature 17°C., which was a mean for the experiment. The correction used was a purely theoretic one, deduced from the formula giving the time of an oscillation; but by using 17° as the standard temperature any error for the higher temperatures would be counterbalanced by a corresponding one for the lower ones. The length of the pendulum at the time of each swing was found in terms of its length at 17°, and the correction calculated

$$D=\frac{\sqrt{l_{17}}-\sqrt{l}}{\sqrt{l}}\quad t$$

where D = correction to be added or subtracted,
l_{17} = length of pendulum at 17°,
l = length of pendulum during swing,
t = period of oscillation during swing.

The corrections and corrected values of the periods are given in the following table:

Date	No. of Swing.	Period Corrected for Arc.	Mean of the Period in Each Swing.	Mean Temperature.	Loss of Clock in Seconds per Hour.	Correction for Temperature.	Correction for Rate.	Mean Period Corrected.	Period for Position A Corrected.
Feb.	A	.9967054							
13	VI. B	7065	.9967060	17°.8	.03438	−42	−95	.9966923	.9966917
	A	.9967009							
"	VII. B	7019	.9967014	17°.9	"	−47	−95	872	867
	A	.9966929							
14	VIII. B	969	.9966949	18°.6	.03454	−84	−96	769	749
	A	.9966876							
15	IX. B	922	.9966899	18°.7	.03475	−90	−96	713	690
	A	.9966813							
16	X. B	841	.9966827	15°.4	"	+83	−96	814	801
	A	.9966823							
17	XI. B	892	.9966858	16°.2	.03705	+41	−103	796	761
	A	.9966849							
18	XII. B	836	9966843	16°.4	.09298	+31	−257	617	623
	A	.9966792							
19	XIII. B	758	.9966775	16°.8	.01602	+10	−44	758	775
	A	.9966844							
"	XIV. B	834	.9966839	16°.5	"	+26	−44	821	826
	A	.9966835							
20	XV. B	849	.9966842	17°.3	.02785	−16	−77	749	742
	A	.9966838							
21	XVI. B	871	.9966855	17°.5	.02870	−28	−79	748	731
	A	9966787							
22	XVII. B	853	.9966820	16°.7	.02415	+16	−67	769	736
	A	.9966776							
"	XVIII. B	878	.9966827	16°.9	"	+4	−67	764	713
	A	.9966827							
23	XIX. B	834	.9966831	15°.6	.04006	+73	−111	793	789
	A	.9966774							
"	XX. B	882	.9966828	15°.5	"	+78	−111	795	741
	A	.9966748							
"	XXI. B	759	.9966754	15°.4	"	+83	−111	726	720
	A	.9966810							
24	XXII. B	865	.9966838	16°.8	.03494	+10	−97	751	723

Corrected value of the period:

1°. Mean of two positions, .9966775 sec.

2°. Heavy and down, A, .9966759 sec.

The latter of these is used as being less liable to error from disturbing causes.

The unknown quantity, which had yet to be determined, was the length of the pendulum or the distance between the knife-edges, in the determination of which even greater accuracy, if possible, is desirable than in the measurement of the period of oscillation.

The first step in the work was the finding of the value of a division on the micrometer head of the microscopes belonging to the comparator. This was done by measuring the successive millimetres of one centimetre on the standard bar, and reducing the lengths to the temperature 14°.66;

the centimetre used for this purpose was the one in which the measurement for the length of the pendulum would be made, from the 98th to the 99th division. As the field of view included more than a millimetre in length, each measurement was made by means of the movable cross-threads only. The average of ten different measurements of the ten millimetres was taken as the number of micrometer divisions in one millimetre.

The results were as follows for the average of each measurement :

1°. For the microscope on the carriage—

1	2205.13
2	2204.87
3	2205.02
4	2204.87
5	2205.45
6	2204.84
7	2204.99
8	2205.10
9	2204.90
10	2205.10
Mean	2205.02 divisions to 1 mm.

2°. For the fixed miscroscope—

1	2260.58
2	2260.71
3	2260.45
4	2260.55
5	2259.61
6	2260.56
7	2261.36
8	2259.90
9	2260.90
10	2261.10
Mean	2260.57 divisions to 1 mm.

From these results the value of a division was taken as

$\frac{1}{2205.02}$ mm. for the former,

$\frac{1}{2260.57}$ mm. for the latter.

When these observations were completed, a comparison was made with a set of measurements taken in the same way some months previously, on the 51st centimetre of the bar; that calculation gave 2205.12 divisions to 1 millimetre for the first microscope, a result which agrees closely with the one used.

It was intended at first to use only one microscope, and measure the distance between the knife-edges by means of the stops, after having made careful tests for the corrections to the decimetre divisions on the standard bar to compare with those given by Professor Rogers.

But from some cause not quite determined, the use of the stops was not found satisfactory. When the carriage bearing the microscope was brought against the stop, and its position determined by finding the distance in micrometer divisions between the fixed cross-thread of the microscope and a graduation mark on the bar, successive readings for the same positions of the carriage, which was brought up to the stop afresh for each reading, were found to vary by quantities too large to be eliminated by taking even a very great number of readings. Whether this was due to lack of skill in manipulating the apparatus, to slipping of the carriage, to the elasticity of the points of contact of carriage and stop, to some slight disarrangement of the mechanism of the carriage, or to some other unknown cause, was not fully ascertained, but after a lengthened series of experiments it was attributed to a defect in the design of the comparator. The carriage is a heavy mass of iron weighing 60 or 75 lbs., and this can hardly be brought against a small stop with just the same force every time in succession; and, as the force varies, even though slightly, the compression of the steel projections is different, or their elasticity causes the carriage to slip back to a different position. The other method of measuring, by means of the second microscope held by a fixed stand, had to be adopted; the tests for the corrections to the decimeter divisions could not be made, as so short a distance as one decimetre could not be measured with the two microscopes.

The focal length of the microscopes was found to be too short to permit observations being made directly on the knife-edge, the sloping side of which prevented the microscope from coming near enough to the edge. Observations on the ends of the knife-edges were not satisfactory, as the position of the edge was not clearly defined, and a slight rounding off was also noticeable, which was thought sufficient to prevent even a fair degree of accuracy in taking the distance between the ends of the knife-edges as the distance between the axes of the pendulum. Use was therefore made of two subsidiary blocks in the manner described by Captain Kater; these were prepared by Mr. Plaskett, electrical machinist of the University. He first procured two pieces of hardened steel, 4½ in. long by 3 in. wide, and 1¼ in. thick. One of the largest faces of each was then planed off, scraped and ground to as near a perfect plane as possible; when rubbed together, they were found to adhere with sufficient force to lift their own weight. With these two faces placed together, the pieces were then firmly fastened in the lathe; for about half its length this block, for the two now appeared as one piece 4½ in. long by 3 in. square, was turned to form a cylinder about 3 in. in diameter and 2 in. ong, and a ¾ in. hole bored in the centre.

The round surface of this cylinder was then smoothed and polished with fine emery powder and rouge, and the exposed end planed, scraped, and polished to a plane surface; a surface plate belonging to the comparator, and prepared by Professor Rogers, was used as the testing surface for this purpose; from it another was prepared which was ground against the end of the cylinder. When the smoothing and polishing were completed, this cylinder was cut off from the rest of the block. It separated into two pieces, each being a part of one of the original blocks; when the finished ends were rubbed together slightly, they adhered quite firmly to each other, showing that a close approximation to a plane surface had been secured.

On placing the polished round surface under the microscope, a number of marks made by the emery powder were distinctly visible. A definite spot was chosen on each where two marks crossed, their position being defined by a fine line drawn with a razor edge parallel to the axis of the blocks; the line was drawn when the two blocks were placed with their finished ends together, and showed under the microscope with about the same distinctness as the graduation marks on the standard bar. Nine rubber bands, each exerting a pressure of 2 lbs. approximately, as tested by a spring balance, were put around the two blocks to hold them firmly together, the ends having been carefully cleaned before. They were then put on the bed of the comparator under the microscope, and a number of measurements taken of the distance between the chosen marks already mentioned, with the following results:

FIRST MICROSCOPE

	Blocks Reversed.
1542 divisions	1540.5 divisions.
1542 "	1541 "
1542.5 "	1542 "
1541.5 "	1543 "
1541 "	1543 "
1543.5 "	1541 "

Mean distance .69927 mm.

SECOND MICROSCOPE.

	Blocks Reversed.
1584 divisions	1582 divisions
1585 "	1583 "
1585 "	1582 "
1584.5 "	1582 "
1584.5 "	1581 "
1583 "	1582.5 "

Mean distance .70036 mm.

Mean value of the distance as determined by the two microscopes, .6998 mm.

It is worthy of mention that these blocks were so nearly the same thickness that, when placed end to end on the bed of the comparator, both could be distinctly seen under the microscope where a change of $\frac{1}{200}$ in. in the distance from the object-glass throws the object to be observed entirely out of focus.

The pendulum was next removed from the stand and laid on the bed of the comparator; it was supported by five blocks of uniform thickness to prevent flexure. The knife-edges were placed in a horizontal position, and the pendulum rod secured from slipping or rolling by a small wedge in a groove in one of the blocks; the groove was made by nailing two small pieces of wood on the block, half an inch apart. The blocks themselves were made of a piece of very dry walnut which has been lying in the basement of the University for many years; they were 3 inches long by 1½ inches wide and ¾ inch thick, and were prepared by Mr. Plaskett; when in use, they were placed on edge. The steel blocks already described were then placed on the pendulum, with the groove on their under side fitting over the rod; each had its plane surface against a knife-edge, and was held firmly in position by two rubber bands.

Afterwards the microscopes were set so that the marks on the blocks, between which the distance had been measured, came in the centre of the field of vision; while no difficulty was found in identifying the marks, some time was necessary to adjust the microscopes properly without disturbing the arrangement of the pendulum.

The two thermometers were placed on smaller wooden blocks, so that their whole length was against the pendulum rod, and the whole was left until the next day to allow the change in the temperature due to handling the rod to pass away. The position of the marks on the blocks with reference to the fixed cross-threads was observed at intervals of a couple of hours for the next two days, during which, fortunately, there was no machinery running, and no jarring to shift the position of the apparatus in even the slightest appreciable degree. The movable cross-thread of each microscope was adjusted exactly over the chosen mark on one of the blocks, and its distance from the fixed cross-thread read in micrometer divisions. These distances, one for each microscope, had then to be added to or subtracted from the distance between the fixed cross-threads, called *d*, in order to determine the distance between the marks on the blocks.

The following table shows the readings taken, the temperature of the rod, and the distance calculated in terms of *d*. Each reading is the mean of a number taken at the same time, and among these the greatest variation seldom exceeded one micrometer division.

Temperature.	Microscope 1°.		Microscope 2°.		Distance between the Marks on the Blocks.
	No. of Micrometer Divisions.	Length in mm.	No. of Micrometer Divisions.	Length in mm.	
18°.2	− 343	.1556 mm.	− 576	.2548 mm.	d − .4104 mm.
18°.9	− 334	.1515 "	− 572	.2530 "	d − .4045 "
18°.7	− 339	.1537 "	− 574	.2539 "	d − .4076 "
18°.9	− 338	.1533 "	− 572	.2530 "	d − .4063 "
18°.9	− 341	.1547 "	− 572	.2530 "	d − .4077 "
19°.	− 334	.1515 "	− 578	.2557 "	d − .4072 "
19°.3	− 333	.1510 "	− 575	.2544 "	d − .4054 "
19°.2	− 332	.1506 "	− 573	.2535 "	d − .4041 "
19°.	− 338	.1533 "	− 575	.2544 "	d − .4077 "
18°.6	− 344	.1560 "	− 578	.2557 "	d − .4117 "
18°.5	− 346	.1569 "	− 577	.2552 "	d − .4121 "
18°.3	− 349	.1583 "	− 578	.2557 "	d − .4140 "

To find *d*, the distance between the fixed cross-threads of the microscopes, the pendulum was removed from the comparator, and the standard bar placed under the microscopes so that the zero graduation mark came in the field of view of one; the 986th millimetre division was then found to come in the field of view of the other. After adjusting it as carefully as possible, and without touching the microscopes, readings were taken to find the distance of the graduation mark in each field of view from the fixed cross-threads, whence in the same manner as in the former comparison *d* was found in terms of a known length on the standard bar. In making the calculations, this length was assumed to be 986 mm at 14°.66 C., and the coefficient of expansion to be $10^{-9}(10246)$, the values given by Professor Rogers. The following table shows the results.

Temperature.	Microscope 1°.		Microscope 2°.		*d* Distance between Fixed Cross-threads.
	No. of Micrometer Divisions.	Length in mm.	No. of Micrometer Divisions.	Length in mm.	
18°.7	− 95	.0431 mm.	+ 115	.0509 mm.	986 0414 − .0078
	− 96	.0435 "	+ 114	.0504 "	− .0069
	− 95	.0431 "	+ 117	.0518 "	− .0087
	− 96	.0435 "	+ 116	.0513 "	− .0078
18°.1	− 100	.0454 "	+ 108	.0478 "	986.0352 − .0024
	− 100	.0454 "	+ 107	.0473 "	− .0019
	− 99	.0449 "	+ 104	0460 "	− .0011
	− 99	.0449 "	+ 105	.0465 "	− .0016
18°.2	− 98	.0444 "	+ 106	.0469 "	986 0363 − .0025
	− 98	.0444 "	+ 105	.0465 "	− .0021

Mean value of *d* from above—986.0336 mm.

It was assumed that the expansion of the heavy bed of the comparator to which the microscopes were attached would be negligible for the small changes in temperature from the time when the pendulum was being measured to the time when the standard bar was being observed, that is, *d* was assumed constant. As the change in temperature of the pendulum rod and the standard bar during the three days of the observation was only 1° C. that of the bed of the comparator would be less on account of its mass; hence the error introduced would be very small.

To find the distance in millimetres between the marks on the blocks, when in position on the pendulum rod, the value of *d* was substituted n the first table (p. 120), after which this distance was found for a temperature of 17°, assuming $10^{-9}(10452)$ as the coefficient of expansion of the rod.

The next table shows the results of these calculations.

Temperature.	Distance in mm. at Observed Temperature.		Correction for Temperature.	Distance at 17°C.
18°.2	986.0336 – .4104	985.6232	– .0125 mm.	985.6107 mm.
18°.9	– .4045	.6291	– .0198 "	.6093 "
18°.7	– .4076	.6260	– .0178 "	.6082 "
18°.9	– .4063	.6273	– .0198 "	.6075 "
18°.9	– .4077	.6259	– .0198 "	.6061 "
19°.	– .4072	.6264	– .0209 "	.6055 "
19°.3	– .4054	.6282	– .0240 "	.6042 "
19°.3	– .4041	.6295	– .0240 "	.6055 "
19°.	– .4077	.6259	– .0209 "	.6050 "
18°.6	– .4117	.6219	– .0167 "	.6052 "
18°.5	– .4121	.6215	– .0157 "	.6058 "
18°.3	– .4140	.6196	– .0134 "	.6062 "

Mean value for distance between the marks on the blocks—985.6066 mm.

Distance between knife-edges, that is, length of the simple equivalent pendulum,

$$l = 985.6066 + .6998 \text{ mm.}$$
$$= 986.306 \text{ mm.}$$
$$\text{and } t = .9966759 \text{ sec.}$$

whence by calculation

$$g = 979.949.$$

CORRECTION FOR BUOYANCY.

The weight of the pendulum is 3160.2 grams. The volume of air it displaces is 492 c.c. approximately, as determined by a careful measurement of all its parts.

The weight of this volume of air is

$$\frac{28.86}{22327} \times \frac{492}{1} \times \frac{273}{273+t} \times \frac{p}{760}$$

where t is the temperature
and p is the pressure.

The mean value of the weight of air displaced calculated from the above formula for the 17 swings of the pendulum, at mean temperature and pressure during the time of swing $= .5892$ grams.

Using Kater's correction for buoyancy, g must be increased in the ratio

$$.5892 : 3160.2 ;\ i.e.,\ 1 : 5363.54,$$
$$\text{whence } g = 979.949 + .1827$$
$$= 980.132.$$

Using Baily's correction for reduction to vacuum as more exact, this correction should be multiplied by 1.945, the experimental value of the multiplier found for a Kater's reversible pendulum with a half-inch rod, and swinging heavy end down. The correction then becomes

$$.1827 \times 1.945 = .3554,$$
$$\text{and } g = 980.304,$$
$$\text{also } l_{\phi} = 99.3256 \text{ cm.}$$

Apply the formula

$$\frac{dg}{g} = \frac{2h}{r}\left(1 - \frac{3}{4}\frac{d}{D}\right) \text{ for}$$

reduction to sea level.

$h = 350$ feet

$g = 980.304$

$\frac{d}{D} = \frac{1}{2}$

$\log \frac{r}{r_0} = \bar{1}.9993101$ (Chauvenet's Astronomical Tables).

$r_0 = 20923600$ feet, equatorial radius.

whence $dg = .021$.

$g = 980.325$.

$l_{\phi} = 99.3276$ cm., at sea level.

When the measurement had been completed, another observation was made on the length of the period of oscillation to see if the knife-edges had undergone any perceptible alteration. The values of the period given by this swing, without corrections for temperature and clock-rate, were

A. .9966841 sec.

B. .9966870 "

which agree almost exactly with the former results.

Another set of measurements was then undertaken by the same method as the first, and under conditions slightly more favorable, as the mean temperature of the rod while being measured was almost exactly 17°, with slight variations above and below; distance between the knife-edges from second measurement,

986.304 mm.

That this came so near to the former result tends to show that the probable error in the length due to errors of observation is very small. There are two indeterminate sources of error: the unknown expansion of the bed of the comparator during the time of measurement, and irregularities or defects in the subsidiary blocks. The error due to the former could hardly be greater than .011 mm, which would be the expansion due to a change of 1° in the temperature of the cast-iron bed, while it would probably be much less as the temperature of the room only changed about 1° during the measurement, and the change in so great a mass of iron would be much slower. There is no means of estimating the error due to the latter cause, but assuming it as .01 mm, the greatest error in the length of the pendulum, supposing the errors from all sources to be in the same sense, would be about .022 mm. The corresponding greatest error in the value of g would be .022, while the probable error in this value is estimated by the writer at .01.

Helmert's formula, $g=980.5934-2.5967 \cos 2\phi$, for finding the value of gravity at any latitude, gives 980.472 for the latitude of Toronto. The observed value given above agrees as closely with this as many of the observed values on this continent do with the values computed by the formula, and more closely than some given in recent years by the most eminent observers. This seeming discrepancy between the computed and observed values may be understood when it is remembered that the constants in Helmert's formula are calculated upon an hypothesis concerning the figure of the earth, based on a limited number of observations of the acceleration of gravity, while no allowance is made for variations due to local causes.

In the present instance, the proximity of the large body of water, Lake Ontario, and the absence of known rocky or other formation of high specific gravity in the vicinity, would indicate that the acceleration of gravity here should be smaller than would otherwise be expected.

CONCLUSION.

In collecting material for the foregoing historical sketch of the pendulum and its applications, the writer has not had time nor opportunity to consult all or nearly all the original memoirs on the subject. Such a task would be impossible in so limited a time with a bibliography comprising over 750 authors and more than 1,300 different published articles, many of which are to be found only in the libraries of Europe. This important work was done recently at the instance of "la Société Francaise de Physique," by M. C. Wolf, the eminent astronomer of Paris. Under his direction, this society published in 1889, in Tome IV. of their "Collection de Memoires relatifs à la Physique," the complete bibliography of the pendulum from the standpoint of Physics, accompanied by an historical sketch, and a number of the most important original memoirs, including those of La Condamine, Borda, and Cassini, Capt. Kater, M. de Prony, and F. W. Bessel. To the memoirs and extracts this work contains, and especially to the bibliography, this writer is much indebted. A list (not complete) of works consulted is given below.

"An account of experiments for determining the length of the pendulum vibrating seconds in the latitude of London": H. Kater, *Phil. Trans.*,* 1818.

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"Philosophise naturalis Principia Mathematica": Newton, 1686.

"On the discordancies in the results of the method for determining the length of the simple pendulum": Baily, *Phil. Magaz.*, 5, 1829.

"Experiments to determine the difference in the number of vibrations made by an invariable pendulum in the Royal Observatory at Greenwich, and in the house in London in which Capt. Kater's experiments were made": Sir Edward Sabine, *Phil. Trans.*, 1829.

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*These three volumes of the Philosophical Transactions were kindly loaned by the Parliamentary librarian at Ottawa.

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"Expériences pour connaitre la longuer du pendule qui bat les secondes à Paris": Mm. Borda et Cassini, 1810.

"Méthode pour déterminer la longuer du Pendule simple qui bat les secondes, et pour trouver, en général, les positions des centres de gravité et d'oscillation, et le moment d'inertie d'un corps de forme quel conque": M. de Prony, 1800.

"Recherches sur la longuer du Pendule simple qui bat la seconde": F. W. Bessel, 1826.

THE ACTION OF HEAT ON CEMENT

By J. S. Dobie, Grad. S.P.S.

The modern tendency towards large and high buildings, with a skeleton structure of steel, has made the question of fire prevention and protection one of the most vital importance to architects, manufacturers, engineers, and builders in general. Countless experiments have been made to devise some means of preventing iron and steel columns and girders from buckling under great heat. Plainly, the only way this can be done is by so protecting the metal that heat cannot come into contact with the metal surface.

Experiments have also been made, and numerous schemes introduced, to devise some means of making a floor between the girders that will withstand the action of fire, and so prevent a fire, even if started, from spreading from one flat to another. Hollow tile, brick, and terra cotta have been tried with indifferent success; and at present the tendency is towards using Portland cement or concrete as a protection for ironwork.

It is not the purpose of this paper to propose or condemn any scheme, or to introduce any new one; but rather to show what really may be expected from a mass of concrete or cement when subjected to great heat. The writer has carefully examined all available literature upon the subject, including books and trade catalogues; and, while many give tests which are claimed to be eminently satisfactory, still, in almost every case, the accounts give such meagre information about the tests themselves as to be almost useless, as far as information is concerned, and certainly so as far as comparisons with each other are concerned. The information as to the action of the cement itself is, in every case, very meagre; and all the information given refers only to the conduct of the system tested, and no information whatever is given as to its condition afterwards.

In making these tests three brands of cement were used—two Canadian brands: "Star," manufactured at Deseronto, and "Samson," from Owen Sound; one foreign brand, "Jossen," of Belgium manufacture. These cements were thoroughly tested for fineness, strength, and "blowing" before using, and were found to be first-class. The "Star" and "Jossen" were comparatively slow-setting, and the "Samson" set very rapidly.

Tests were made upon neat briquettes and sand briquettes, mixed in proportions of one sand to one cement, two sand to one cement, and three sand to one cement. The age of the briquettes varied from two months to about four years. Over two hundred briquettes were used in making the tests.

The briquettes were heated in a small assay furnace, heated by a large gas burner, and which gave a range of temperatures of from about 650° Fahrenheit to about 1775° Fahrenheit.

The temperatures were determined by means of a water calorimeter; the temperature of the furnace being obtained from the rise in temperature of a pint of water, in which had been immersed an iron ball of known weight, and which was heated to the temperature of the furnace. The temperatures given are approximately correct. The calorimeter gave good results—different readings, under the same conditions, rarely differing by more than 10° or 15°, which is very close for such high temperatures.

On heating the briquette and removing from the furnace, the first thing noticed was a loss in weight in the briquette; and almost all neat briquettes showed cracks, some large and some small. Some sand briquettes also showed cracks, but not to the same extent as in the neat briquettes; but there was no exception to the change in weight. This loss in weight is due to the fact that the hardened cement is a compound of hydrated crystals of aluminium silicate and calcium silicate, and the heat drives off the water of crystallization, and the loss in weight corresponds to the amount of water of crystallization driven off by the heat. Now, since the crystals depend upon the water for their formation, it is evident that when the water is removed the crystals are destroyed, and the cement ruined. Such was found to be the case in every instance.

The briquettes, after cooling sufficiently to allow handling, were broken in the testing machine, the load being applied at the rate of about 400 pounds per minute. The result showed a marked decrease in the tensile strength, as reference to Tables I. and II. will show; and, under the best conditions, this loss in tensile strength showed an approximate proportion to the loss in weight. Briquettes which lost 19 or 20 per cent. of weight, which is practically the amount of water required for proper crystallization, were practically unable to resist any load whatever, and in very few cases possessed 10 per cent. of their original strength.

The effect of different temperatures was rather peculiar. Briquettes placed immediately into a temperature of about 1775° Fahrenheit showed a very great loss in strength for a very small loss in weight, and did not appear to follow this law of proportion between losses of weight and strength, at least not nearly so well as briquettes treated differently. When the briquettes were gradually heated, however, and allowed to slowly rise to the temperature of the furnace, this law was followed quite closely, as reference to tables will show. The plotted curves give a good idea of the effect of slow and rapid heating on the relation of the cement to this law. This is accounted for by the fact that the cold briquette, on being placed in the furnace at such a great heat, is immediately subjected to very considerable internal stresses, due to the expansion of the outside of the briquette before any water of crystallization at all is driven off. Briquettes, heated gradually, were not subjected to these internal stresses to nearly the same extent as those which were heated very suddenly.

An attempt was made to discover, if possible, a critical temperature to which it would be possible to subject the cement without danger of injury to the crystals, and where the cement would retain its cohesive powers. It was found that, if such a temperature existed, it was below the lowest temperature generated by the furnace, which was considerably below red heat. The lowest temperature had the same effect as the highest, excepting, of course, that with the lower temperature a longer time was required to effect the same reduction of weight than with a higher one. The lowest temperature necessary to destroy the cement was not determined, but is probably quite low. The following quotation from Heath's "Manual of Lime and Cement" is interesting regarding this. "Even good Portland cement concrete, if exposed to the weather during a hot, dry summer, with no supply of moisture available, will probably be found covered with a network of innumerable hair-like cracks, which are signs of the beginning of disintegration, the natural and inevitable result of the loss of the water of hydration of the cement." (Heath's "Manual of Lime and Cement," page 75.)

The only neat briquettes which were not cracked by the heat were some very old ones, about four years old, which were tested. Most of these were uncracked, although some that were very suddenly heated cracked somewhat. All the newer briquettes cracked, the cracks being very large in most cases.

After cooling, the briquettes were found to be quite soft, and could easily be crushed between the fingers. This was especially noticeable in the case of those briquettes which had lost the most water of hydration. These briquettes were like lumps of dry mud or clay, and a very slight pressure was sufficient to crush them completely. Briquettes which had not lost so much water were harder, while those which had lost only about one or two per cent. of water were quite hard. Of this latter class, however, those briquettes which had been heated suddenly were much the softer.

Briquettes which were cracked on removing from the furnace showed the cracks much distended and widened out on cooling, and most of those which had not cracked in the furnace showed cracks on standing for a day or two.

All sand briquettes, whether cracked in the furnace or not, entirely disintegrated on standing in air for a time, the cementing material having been entirely destroyed. Those sand briquettes which had lost most of their water of hydration disintegrated first, and those that had lost less followed soon after. Six briquettes of two sand to one cement were placed suddenly in the most intense heat, and left for about three minutes and removed. They lost from three to four per cent. in weight, and two of them which were broken showed a complete loss of tensile strength; one breaking before any load was applied, and the other going at about eight pounds. The remaining four were set away, and in a couple of days commenced to crumble, the edges going first, and leaving an irregular mass in the centre. This, however, resisted for some time, but in about three weeks they also were completely disintegrated. It was thought that the sudden application of very intense heat would cause the water of hydration to be removed from the outside only, and that the inside would remain solid, and resist weathering. The results, however, showed that although the outer part of the briquettes suffered most, as is shown by the rapid disintegration, still the heat also affected the inner part, and caused its ultimate disintegration. These briquettes were too hot to be handled comfortably when weighed, after removal from the furnace, and it is quite probable that the heat still remaining in the outer portion caused a still further loss of water of hydration in the parts in the centre of the mass. The low tensile strength of these two briquettes was probably caused by the sudden heat causing great internal stresses in the briquettes. The parts of the two broken briquettes also behaved in the same manner as the unbroken ones.

A number of briquettes were heated, and rapidly cooled by being immersed in cold water, after being heated for different lengths of time at different temperatures. In every case the briquettes cracked when immersed; and, if they were red-hot before immersion, they completely disintegrated—in most cases being reduced to a heap of soft mud. The sand briquettes acted precisely similarly to the neat ones; and it appears conclusive that if cement or concrete is allowed to become red-hot, and is then immersed in cold water, the effect would be ruinous in the extreme. Some briquettes were also heated, and, instead of being immersed, were placed under a small stream of water flowing at the rate of about two litres a minute. The results were the same. Red-hot cement completely disintegrated. In some cases, when heat had not been applied sufficiently long to heat the briquette through and through, and where the edges only glowed, the glowing parts chipped off when water was applied, leaving an irregular mass comprising that part of the briquette which had not been heated so much. Even when not completely disintegrated, this central mass showed cracks, was very soft, and had no strength to resist any pressure.

From these experiments, then, the following conclusions may be drawn:

(1) While there is no doubt that a covering of Portland cement concrete will afford some protection to a metal column or girder, still there appears to be no doubt that the concrete itself will be ruined by the action of the fire, and will have to be removed as soon as the fire is subdued. The concrete covering may remain upon the ironwork during a fire, but the heat will damage it to such an extent that it will disintegrate afterwards. The impression appears to exist that the concrete will be in as good condition after a fire as before. This is a mistake, as a fire of ordinary intensity is sufficient to ruin, completely, a very large covering of concrete.

(2) The concrete covering, if heated, will not stand the action of water; all the experiments show that water applied to hot cement is extremely ruinous. This is a very important point, when we come to consider the immense amount of water thrown into a building during a conflagration. The cement is certain to become red-hot; and then, when the hose is turned on, the water strikes the protecting covering, and it breaks off, leaving the ironwork bare and filling the air with a shower of dangerous falling pieces. In a fireproofed building, the usual construction is to have a system of concrete arches and slabs between the joists and girders. The walls are of concrete, and have metal doors to confine the fire, if started, to one spot as much as possible. The floor is usually cemented over, or boarded with thin flooring, and is, therefore, more or less water-tight. The ceiling, or under part, might be exposed to intense heat, while immense quantities of water were being poured upon the upper portion, causing an immense strain on the concrete, not only from the

weight of water, but from the opposite strains which the cement would undergo, on account of one side being expanded by the intense heat, while the other side is kept cold by the water upon it. All this time the fire below is eating away the strength of the cement, and a collapse is the result.

(3) In calculating for the design of the columns and girders, and especially for floors, no allowance should be made for the strength of the concrete, but the cement covering should be considered as so much extra load on the system. It is the usual custom, in designing a fireproof system, to consider that the concrete bears its share of the loads upon the girder; and, in a great many cases, the girder is designed on that assumption. If no fire occurs, this is all right; but, during a severe fire, the concrete loses its cohesive properties, both on account of the loss of its water of hydration, and on account of the great internal strains caused by the expansion of one side under heat, and, consequently, becomes unable to resist any stress anywhere near what it was originally able to bear, and in most cases would not even be self-supporting. The experiments have shown that sudden heating is extremely ruinous, much more so than heat gradually applied; and, in the average fire, a very high temperature is generated in an incredibly short time, and concrete subjected to its action would, in a very short time, be unable to bear any strain whatever.

It appears from this that, in a fireproof building, floors should never be constructed of slabs of cement, forming short spans or arches from girder to girder, without any support. If no load were placed on the floor, and it were subjected to heat, it might possibly retain its form, provided water was not thrown upon it; but if loaded, as any floor would be, a collapse is inevitable. Concrete, then, should always have a metal system of some sort imbedded in it, sufficiently to bear the weight of the concrete itself, and all external loads which may come upon it.

The great advantages possessed by cement or concrete, as a fire-protecting material, are its low heat-conducting power and its very small expansion under heat. These advantages, however, are entirely off-set by the fact that it loses its strength under heat, is ruined by water applied during a fire, and will disintegrate after a fire, if not during the fire itself. These experiments tend to show, therefore, that the value of concrete as a fire-protecting material has been greatly overestimated, and that disastrous results may follow from confidence in a building protected with such a material.

TABLE I.—RESULTS OF 26 BRIQUETTES HEATED SUDDENLY.

Temperature, 1775° Fah. (approx.).

Weight in Grammes.		Per Cent. Lost.	Tensile Strength.		Per Cent. Lost.
Before.	After.		Before.	After.	
*140.02	125.57	10.4	530	190	64.1
*139.28	117.47	15.4	"	200	62.3
*140.06	115.75	17.3	"	185	65.1
*139.07	113.72	18.6	"	53	90.0
*140.46	125.99	11.6	"	180	66.1
*140.98	132.87	5.7	"	235	55.7
*139.23	137.68	1.1	"	330	37.7
*140.11	120.89	13.7	"	140	73.6
*139.47	133.57	2.9	"	280	47.1
*139.21	112.07	19.5	"	15	97.1
*140.17	121.17	13.4	"	155	70.7
*138.71	119.70	13.7	"	145	72.7
*135.77	131.70	2.9	930	0	100.0
†133.75	118.85	11.1	"	20	97.8
†132.75	dropped from tongs, and broke on floor.				
†131.16	123.05	6.2	930	50	94.6
†132.76	118.95	10.4	"	20	97.8
†134.15	128.70	4.8	"	23	97.5
‡139.22	125.72	9.7	515	85	83.5
‡138.41	128.39	7.2	"	75	85.4
‡140.00	134.23	4.1	"	122	76.3
‡137.35	135.54	1.3	"	200	61.2
‡138.47	131.38	5.1	"	97	81.1
‡138.21	135.68	1.8	"	185	64.1
‡140.04	112.87	19.4	"	0	100.0
‡137.26	113.24	17.5	"	10	98.1

* "Star" Brand.

† Briquettes, 4 years old, brand unknown.

‡ "Jossen" Brand.

TABLE II.—RESULTS OF TWENTY-ONE BRIQUETTES HEATED VERY GRADUALLY AND BROKEN.

Maximum Temperature, 1000°—1025° Fah.

Weight in Grammes.		Per Cent. Lost.	Tensile Strength.		Per Cent. Lost.
Before.	After.		Before.	After.	
*139.27	132.34	4.9	530	395	25.6
*138.31	130.16	5.9	"	390	26.3
*140.26	128.32	8.5	"	348	34.3
*140.06	129.16	7.8	"	351	33.7
*138.07	126.23	8.6	"	327	38.2
*138.04	120.13	12.9	"	152	71.2
*139 26	116.27	16.5	"	73	84.3
*138.35	113.31	18.1	"	29	94.5
*140.21	119.26	14.9	"	110	79 0
*139.32	125.34	10.04	"	237	55.2
†137.79	129.32	6.1	515	348	32.3
†139.42	130.26	6.6	"	348	32.3
†140.31	133.58	4.8	"	398	22.6
†139.27	127.31	9.3	"	232	49.0
†140.32	120.15	14.3	"	182	64.7
†138.37	136.32	1.5	"	440	14.7
†139.46	134.31	3.7	"	400	22.3
†140.07	121.31	13.4	"	195	62.3
†139.36	117.26	15.8	"	130	74.6
†140.21	122.31	12.5	"	193	62.4
†139.36	112.60	19.2	"	10	98.0

* "Star" Brand.

† "Jossen" Brand.

ASPECT AND PROSPECT

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In addition to providing the necessary accommodation in a dwelling, an architect must also satisfy the convenience of his client, as far as possible, by a suitable arrangement of halls and rooms, and at the same time make the different rooms in the residence as cheerful and pleasant as possible.

Among the many features which contribute towards making the home cheerful and pleasant, the two most important, and at the same time most neglected in Ontario, are the relation of the building, or rather of the different rooms, to the position of the sun, *i.e*, aspect; and, secondly, the placing of the windows to take advantage of the pleasant views of the landscape, and at the same time obstructing any undesirable views.

The aspect of a room, then, is the relation of its windows to sunlight and weather, while the prospect is simply the view from the windows.

A landscape, as seen from the southern windows, will, with the exception of the early morning and evening of the longer days of summer, appear dark, with the shady side of all objects facing the observer; while the western prospect will be well lighted in the morning, the eastern prospect dark, but lighted in the afternoon, and the northern well lighted at all times, in the morning and evening from the side, thus casting, in most cases, pleasing shadows.

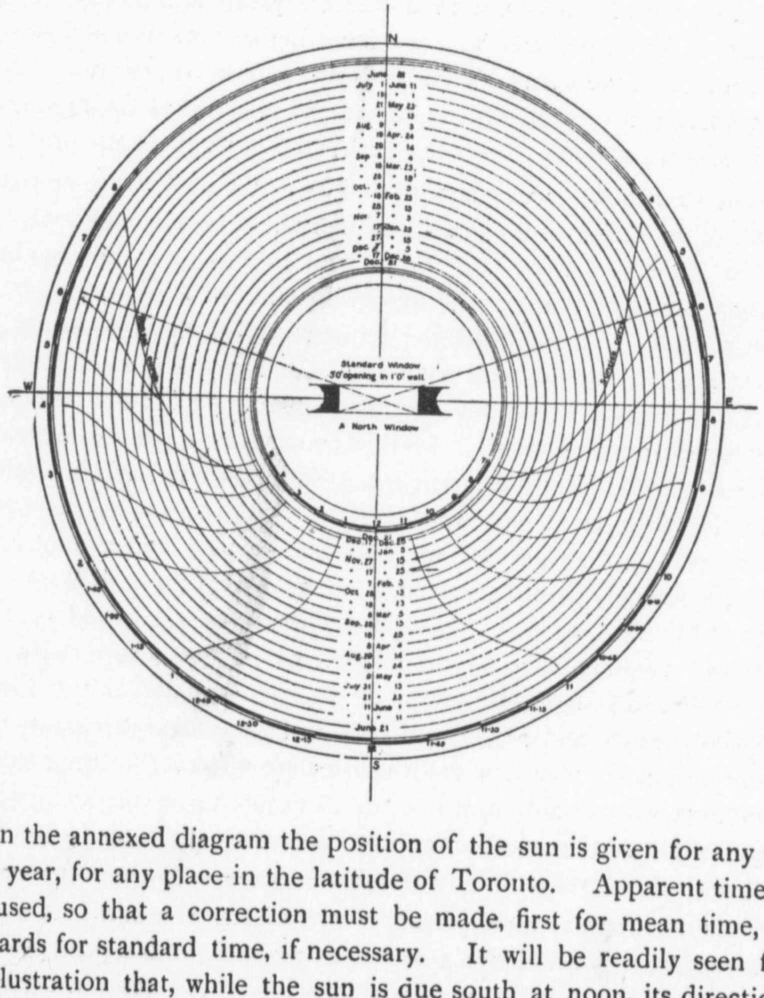
In the cities and larger towns our architects say, "If we only had the landscapes"; while in our rural districts we meet with the fact, if not with the statement, that our dwellings must have their axes parallel, or at right angles to the road, with the parlor on one side of the entrance and a sitting-room on the other, irrespective of what the landscape may be, or whether the road runs north, east, or towards any other possible point of the compass. If the planning of our townships had depended upon the contour of the country, so that the roads would wind back and forth up the valleys, the result of placing the rural residences in some regular way from them might have been satisfactory. But, unfortunately, surveyors, acting upon instructions from the Government, have followed the straight and narrow way over hill after hill. Even on many of the through roads the principle of laying out seems to have been Euclid's definition of a straight line, with an utter forgetfulness of the conservation of energy. In this connection there is no doubt that a vast improvement is possible throughout the country, and much might be accomplished, even in the cities and towns, by obstructing an undesirable view with a group of cool spruce trees, or a cluster of birch with their delicate green foliage and brilliant white bark, assisted, perhaps, by a few flowering shrubs. While there may be some reason in the excuse that a landscape cannot be provided in every case, yet there is no reason for the haphazard position of the rooms and windows, relative to sunlight, which is exhibited throughout the province.

In the cities and towns it is not possible to have all of the houses on the north or west sides of the streets. Much might be accomplished, however, if a little judicious care were taken in providing bay windows, prejections, etc., and even in the proper location of the windows in the different rooms. In many cases the living rooms of the house might be placed at the garden front with good results. There are, throughout the province, a few cases where this has been done, much to the satisfaction of the occupants of the house. In this connection might be mentioned the residence, in Galt, of the Hon. James Young, which is placed with its back to the main street, thus throwing the living rooms on the garden front, and giving at the same time a southern aspect, and a magnificent view up the ravine. On the other hand, there are, in the neighborhood of the Bay of Quinte, a number of pleasant country villas, situated on rising ground and commanding magnificent views of the beautiful Napanee valley and the blue, sparkling waters of the bay, bounded in the distance by the shady groves and bright meadows of Prince Edward County; and yet these houses are placed without any feeling whatever for their sublime surroundings, without any regard to the daily journey of the sun, and the direction of the cutting winds or pelting rains, but simply situated on rising ground, with their axes parallel with concession—— of township————.

A guest at such a house, where everything might and should be properly arranged, is ushered into a cool drawing-room, with a poor prospect, and partakes of an evening meal with the bright sunlight glaring in his face; when, if the plan had been reversed, he would have enjoyed the dining-room because of its coolness, and the drawing-room would have been cheerful on account of the sunlight.

Why, in Ontario, we have been careless of such matters, when a little trouble would conduce to such comfort, it is difficult to say; but, judging from the results of our best architects, there will be a decided improvement in the near future.

Professor Kerr, in his exhaustive treatise on house planning, publishes, among the many illustrations, a diagram which he called an aspect compass, and which simplifies very materially the question of determining the available sunlight.



In the annexed diagram the position of the sun is given for any time of the year, for any place in the latitude of Toronto. Apparent time has been used, so that a correction must be made, first for mean time, and afterwards for standard time, if necessary. It will be readily seen from this illustration that, while the sun is due south at noon, its direction is not constant for any other hour during the year, but varies, moving along a reversed curve as indicated. Thus, while the sun may be slightly north of east at 7 a.m., on the 22nd of June, it is considerably south of east in midwinter, and for intermediate dates the direction of the sun may be

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found by observing the intersection of the 7 o'clock curve, and the circle representing the required date.

The 5 o'clock curve ends when the sun rises at 5 o'clock, the 6 o'clock curve when it rises at 6 o'clock; so that, in joining these points, we get a curve of sunrise, and similarly for sunset. On the diagram the azimuth of the sun is not shown before sunrise or after sunset, as it would not be of any value for the purposes of aspect.

In using this diagram, all that is necessary is to apply a tracing of it to the plan of the proposed house, shifting it from one window to the next, being careful in each case to keep the centre of the diagram in the centre of the opening, and the proper direction of the meridian.

The diagram indicates the application to a window in a north wall, and shows that the sun's rays may enter such a window from April 25th until August 18th, and that there will be one-and-a-half hours of sunlight entering in the morning of June 21st, the same in the evening, while from August 18th to April 25th there will not be any direct light coming through the window.

Considering this window to be in a south wall, sunlight would enter from 9.15 until 2.45 on June 21st, or from 8 o'clock until 4 o'clock on April 2nd. Similarly, the amount of available sunlight may be determined for any given position of the window. It will thus be seen that a southerly window gives a maximum of sunlight, when the rays have their greatest hygienic value.

For the benefit of those members of the society who have not given much thought to the aspect of different rooms, it might briefly be stated that a dining-room should have a north or northeast aspect. A combined dining and sitting room must have a compromised aspect, depending largely upon the character of the household, the style of living, and local peculiarities. In the case of a morning room (used for breakfast and a morning sitting-room), the morning sunlight will always be welcome, and an eastern or southeast aspect is desirable.

The drawing-room should have such an aspect as will give a maximum of sunshine and mild weather, avoiding, as far as possible, the disagreeable east winds, wet weather, and sultry sunshine.

LAING'S PLANETARIUM

By A. T. Laing, B.A.Sc.

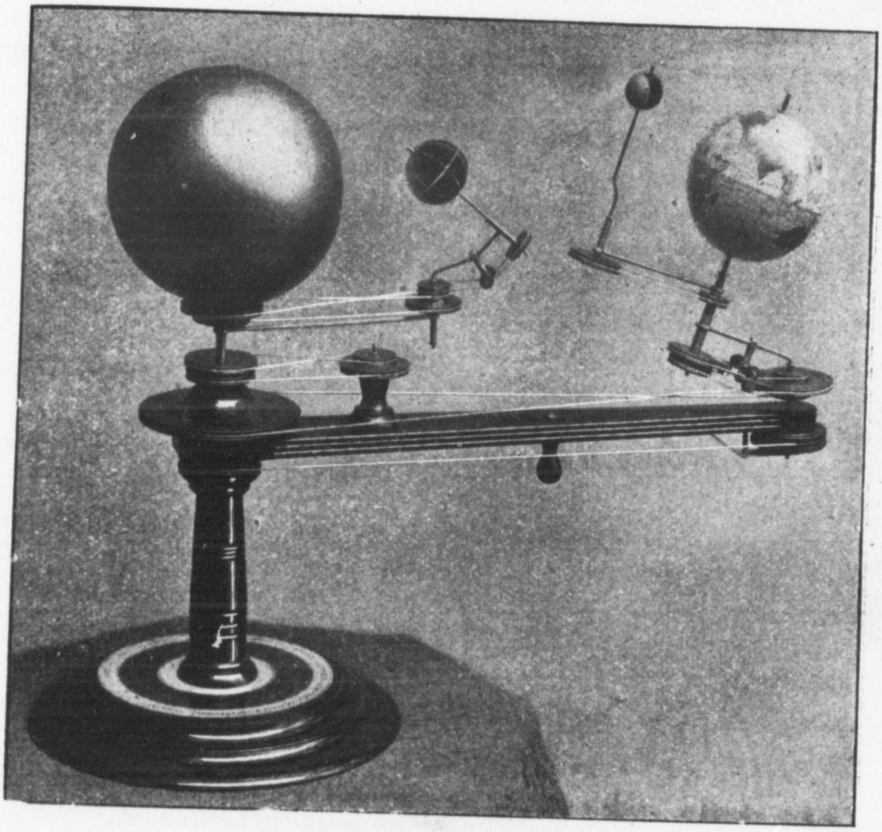
It is with pleasure that I present to you, this afternoon, this recent invention, called after the inventor, Mr. A. Laing, of Essex, Ont., "Laing's Planetarium." It is one of the most complete of astronomical instruments that has ever been constructed for the elucidation of planetary phenomena.

It is designed principally for use in the various educational institutions of the country, and has already met with the highest approval by many who stand at the head of these institutions. It will prove of invaluable assistance to the teacher in attempting to teach mathematical geography, as well as primary and advanced astronomy. Besides its use in schools, this instrument will make a handsome and instructive addition to the equipment of any private library. It will stimulate in the minds of those who study it a taste for the subjects which it illustrates and explains; and when an interest in the subject of astronomy is awakened, it develops a spirit of research and enquiry, which the instrument will do much to gratify.

The following are a few of the many facts plainly illustrated by the instrument:

I. It shows that the diurnal rotation of the earth is the cause of the apparent diurnal motion of the sun, moon, and stars from east to west, in consequence of which they rise in the east and set in the west, thus clearly showing the cause of day and night.

II. It shows also, very clearly, that the sun crosses the equator from south to north on the 20th of March, or the time of the vernal equinox; and that, as the sun moves along the great circle of the ecliptic, it reaches its greatest northern declination perpendicular to the tropic of Cancer, 23½° from the equator, about the 21st of June, or the time of the summer solstice; and that then the great day star inclines its course toward the south, crossing the equator at a point diametrically opposite the vernal equinox. The sun still continues its course toward the south, till it reaches its greatest southern declination about the 21st of December, or winter solstice, perpendicular to the tropic of Capricorn, 23½° south of the equator; and that the sun then begins to incline its course toward the north, reaching the vernal equinox again on the 20th of March in the following year, having made a complete circle of the ecliptic.



Laing's Planetarium.

This illustrates the four cardinal points of the earth's annual motion—the vernal equinox, summer solstice, autumnal equinox, and winter solstice.

III. The axis of the earth is very plainly shown to be pointing in the same direction, or toward the pole star, at an inclination of 23½° from the perpendicular, during its complete circuit around the sun, thus illustrating the vicissitudes of the seasons and the changing of the climate.

IV. The instrument clearly illustrates the fact that the circle of illumination is perpendicular to the ecliptic, and at right angles to the radius vector; and that, at the time of the equinoxes, the circle of illumination cuts all the parallels of latitude as well as the equator in two points, diametrically opposite, thus giving equal day and night all over the world; while, at the time of the summer solstice, this circle cuts the earth at a point 23½° beyond the north pole, and falls short of the south pole by the same distance. This produces a difference in the duration of day and night, which depends upon the parallel of latitude in which we live.

The pamphlet of instructions which accompanies the instrument gives a table of the lengths of day and night, according to the latitude, from the equator to the polar circles.

V. The fact that the ebb and flow of the tides does not occur at the same time each day is very clearly explained by using this instrument. It shows the cause of the difference to be the motion of the moon around the earth, in the same direction that the earth rotates.

VI. The planetarium plainly shows that the orbit of the moon cuts the plane of the ecliptic at an acute angle, thus illustrating what is meant by the ascending and descending nodes.

VII. The apogee and perigee of the lunar orb are shown so clearly that a child can understand it.

VIII. It clearly demonstrates the fact that an eclipse of the sun can take place only at the epoch of the new moon, and that the moon can undergo an eclipse only when it is full. It also shows that on account of the inclination of the moon's orbit, already referred to, it is only when at the moment of new or full moon, the moon is at or near its node, that an eclipse can occur. This gives the reason why we do not have eclipses every month.

IX. The three principal motions of the planets are nicely illustrated. They are as follows: When a planet is seen to be travelling from west to east among the stars in the celestial sphere, it is said at this time to have its direct motion; but when its motion appears to be from east to west, it is called its retrograde motion. And during the periods between the direct and retrograde motions the planets will for a short time appear stationary. The instrument shows the cause of these apparent motions to be due to the manner in which the orbital motions of the planets are combined with ours.

X. The instrument illustrates the fact that the orbital motion of Venus is so combined with ours that it causes her to return to the point of inferior conjunction every 584 days, and when she passes this point out to the right or west of the sun she is called our morning star. She wings her flight out to the point of greatest western elongation, and passes round to the point of superior conjunction, when she ceases to be morning star and assumes the office of evening star; she now passes out to the left or east of the sun and becomes higher and brighter till she passes her greatest eastern elongation. She now pursues her course till she reaches again the point of inferior conjunction, and becomes our morning star once more; and, as is shown by the instrument, these aspects are repeated in never-ending succession.

Very nearly the same remarks will apply to Mercury, the periods of time being different. The transits of Venus and Mercury are explained at the same time.

XI. The perihelion and aphelion of the Earth, Jupiter, or any of the primary planets, is made very clear by the use of the instrument.

XII. It also shows in a comprehensive manner what is meant by the zodiac; how it is divided into twelve signs, and the names of the signs. What is meant by right ascension; how that the sun, for instance, is said to be in six hours right ascension at the moment of the June solstice (or the moment when the sun enters the sign of the zodiac called Cancer), the sun at the same time having reached its greatest northern declination; how the sun is said to be in twelve hours right ascension at the time of the autumnal equinox, its declination at the same time being zero; also how the sun is in eighteen hours right ascension at the time of the winter solstice, and how, at the same time, the sun enters the sign Capricornus, 23½° south declination. Also that the sun's right ascension and declination are both zero at the vernal equinox.

Besides what is contained in the brief remarks above, the following are some additional facts illustrated by the instrument: The ecliptic, the zones, the midnight sun, the seasons of Venus, leap year, the procession of the equinoxes, parallelism of the axis of Venus, the continual changing of the amplitude of the sun, etc.

One of the main features of this instrument is its simplicity of construction; there is nothing to get out of order, and all the various motions go on simultaneously.

ROENTGEN RADIATION

By J. C. McLennan, B.A.,
Assistant Demonstrator in Physics at Toronto University.

When the announcement was recently made by Professor Roentgen, of Wurzburg, Germany, that he had discovered a new kind of radiation, it excited so much popular interest, and seemed to have such a far-reaching influence in the development of physical science, that we considered it advisable to verify in Toronto at once, as far as practicable, the results obtained by the original investigator

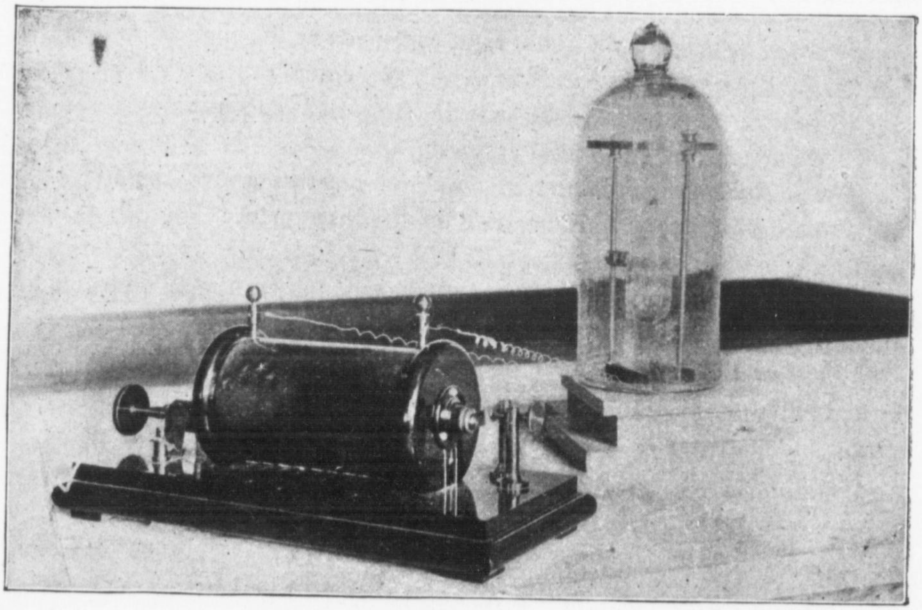


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FIG. 1.

Apparatus showing Induction Coil and Bell-Jar containing Crookes' Tube.

Together with Mr. C. H. C. Wright, B.A.Sc., Lecturer in the School of Practical Science, and Mr. J. Keele, B.A.Sc., of the same institution, the writer repeated these experiments, and found the results exactly as described, and even more wonderful than we had anticipated.

As shown in the accompanying illustration, the apparatus used by us consisted of an induction coil of moderate size and a Crookes' tube of special form.

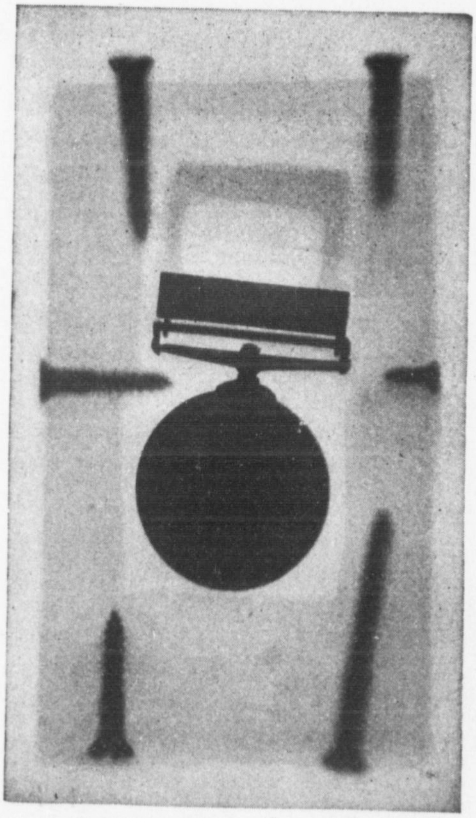

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FIG. II.

Northwest Medal Photographed through a Block of Wood.

Crookes' tubes are made of glass, and were originally designed to exhibit properties of the electric discharge when passed through air at different pressures. The electric current is led into and from them by means of platinum electrodes sealed into the glass and carrying discs or caps of aluminium of different shapes to give variation to the form of the discharge.

When an electric spark is passed through a tube from which the air is being gradually exhausted, it presents a variety of appearances, each being characteristic of the vacuum obtained. At first it consists of a single line of light. It then breaks up into a number of irregular streaks, and this appearance, as the exhaustion goes on, gives place to a bluish colored halo between the electrodes. This halo then breaks up into a series of parallel discs, and, on pushing the exhaustion still further, these disappear entirely from the negative, but remain in the region of the positive electrode. Here a new appearance is presented. When the air has been exhausted to this degree, the part of the tube directly opposite the negative electrode begins to glow with a beautiful fluorescence, which indicates that some peculiar invisible discharge, causing this effect, is emanating from the negative electrode. It is the cathode rays, so called, which are said to form this discharge, and the tube in this condition constitutes a Crookes' tube.

These cathode rays have been very fully investigated by Lenard, Hertz, Thomson, and others. It is found that, like ordinary light rays, they travel in straight lines, are capable of producing intense heat, and, unless especial care is taken, there is great danger of melting the tube if the discharge is continued without interruption, even for a few minutes.

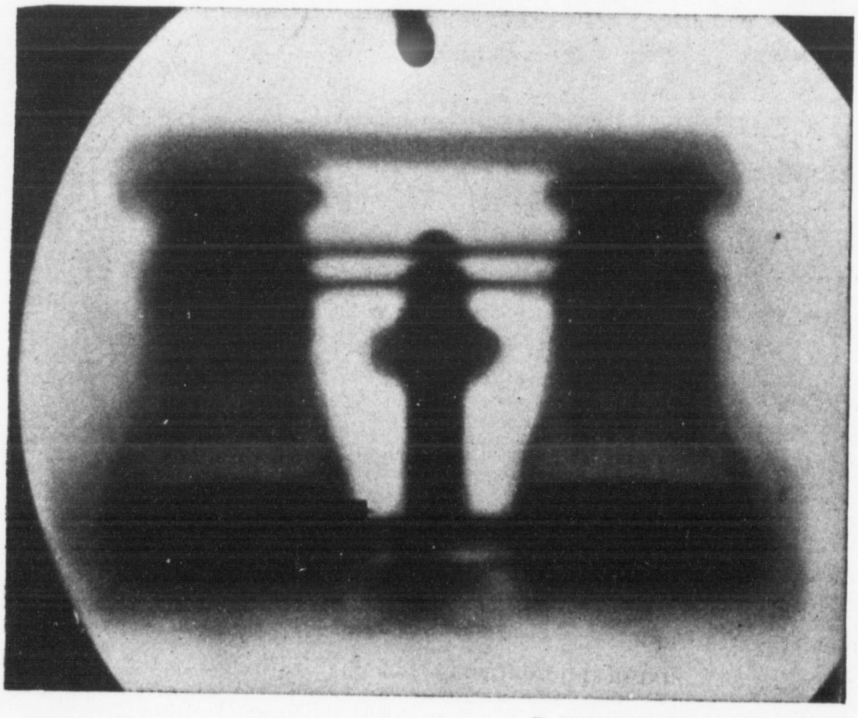

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FIG. III.

Opera Glasses Photographed Through Case.

If a magnet is brought up to the tube when these rays are passing, they can be readily deflected from their direct path, and, in fact, a finger presented to the tube is sufficient to deflect them towards the point of contact.

In his early experiments, Professor Roentgen found that on surrounding a Crookes' tube while in action with a close-fitting black paper cover, it was possible to see in a completely darkened room a brilliant fluorescence upon paper covered with barium platino-cyanide held near the tube. This appearance he found to be still visible, though faint, at a distance of two metres. From this experiment he concluded that the Crookes' tube was the origin of the action causing the fluorescence, and that this action, whatever it was, passed through paper which was impervious to ordinary light. Extending his experiments by placing other substances between the Crookes' tube and the fluorescent screen, he found that all bodies allowed this new kind of radiation to pass through them in a greater or less degree. Wood, paper, and water were very transparent, aluminium and ebonite fairly so, while copper, lead, gold, platinum, and even glass were quite opaque unless made in very thin plates. Different thicknesses of various materials were tried, and the results obtained showed that as the thickness increased the hindrance offered to the new rays by all bodies also increased.

The density of bodies seems to be the only property which affects their permeability, and yet their densities alone do not determine completely their transparency, as plates of aluminium, glass, quartz and Iceland spar of equal thickness were interposed between the tube and the screen, and it was quite evident that, although their densities are about the same, the resistances they offered to the passage of the rays were quite different. Although these new rays can be passed through many substances which are opaque to sunlight, no evidence has yet been obtained which would show that they can be refracted. On passing them through prisms, or lenses, of water, carbon bisulphide, ebonite, aluminium or wood, there is no indication, or, if any, but slight, of refraction at these surfaces.

Since many metals and glass of ordinary thickness are found to be impermeable to these rays, it is but natural to expect that these substances would reflect them; but all experiments so far seem to show that the ordinary law of reflection does not hold for these new rays, and that if they can be reflected at all it is only in a very general and irregular manner.

Although this unknown radiation was at first detected and studied by means of a fluorescent screen, it was soon found that ordinary photographic dry plates were sensitive to it, and it is owing to the developments in this direction that such intense interest has been aroused.

The reproductions illustrating this article are from photographs taken in the course of our own investigations, and they will indicate some of the possibilities of the new discovery. Fig. II. was obtained by placing on the cardboard box containing the sensitized plate a silver medal, over which was placed a block of wood one inch thick. The screws also shown in the picture were driven into the wood, and from the appearance of the cut it is quite evident that while the wood offered but little resistance to the passage of the rays the metals were quite opaque. Fig. III. represents a pair of opera glasses taken in their case. The metal parts were of aluminium, and it can be seen that, while the rays passed through the tubes at all points, they did so to a much greater extent where there was only one thickness of the metal. The object lenses are clearly defined, showing that the glass is quite opaque, and the outlines of the

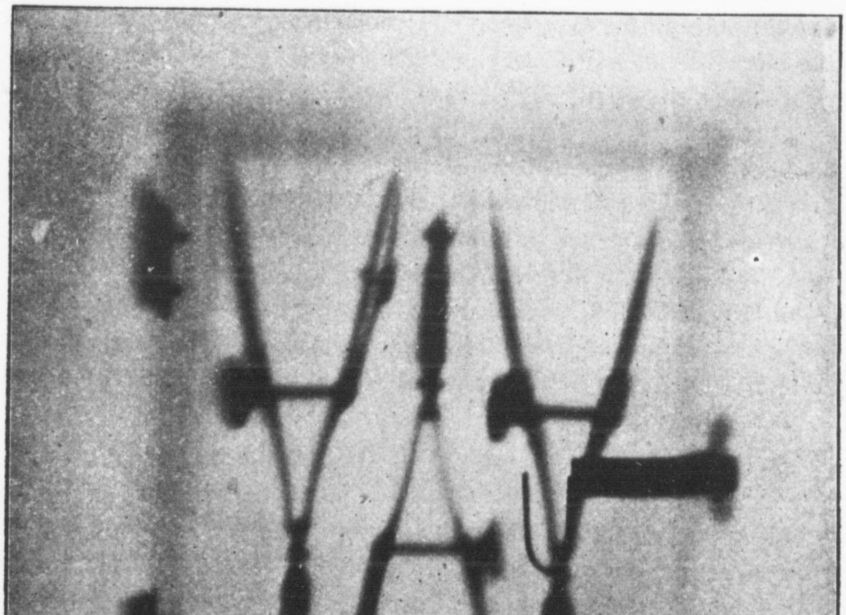

FIG. IV.

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case are indicated but faintly. Fig. IV. shows a set of drawing instruments inclosed in a thick leather case.

The action upon the sensitized film of the dry plate seems to be the same as that due to the light of the sun. The developers used were metol, hydroquinone, pyrogallic acid, and oxalate of potash and iron. Pyro' developer seemed to give best results. The images came up rather more slowly than with ordinary light, and the density as seen before fixing the plate was somewhat misleading, as the chemical action seems to be confined to the surface of the film only. The color of the deposit upon the plate by the various developers is the same as that given by sunlight Various types of dry plates were tested, but, though we were unable to detect any difference in the action upon them, it may be possible, when more properties of the new radiation are known, to produce a more sensitive film than those now in use.

During our early experiments we found that the time required to obtain good impressions on a plate was so long that the utility of the new discovery seemed to be very limited, even if not doubtful.

We therefore directed our efforts to reducing, if possible, the time of exposure, and this we succeeded in doing to a very marked degree.

On making a careful test of all the tubes in the Physical Laboratory, we found one which gave a much stronger radiation than any of the others. This tube, constructed by Seguy, of Paris, was pear-shaped, and as it had one electrode inserted in the smaller end, and the other in the side, we were able, by making the former the negative terminal, to obtain a large glass surface exposed to the action of the cathode rays. This tube was employed in all our later experiments. Thinking that probably the action would vary with different sensitized films, we conducted a series of tests to determine the relative sensitiveness to the rays of various types of plates, but observed no marked difference, and concluded that any reduction in the time of exposure must be otherwise obtained. As experiments made with prisms and lenses of wood, pitch, and other materials, gave no indication of refraction at their surfaces, the only remaining method for the concentration of the rays seemed to be an application of the principle of reflection. In order to determine whether the rays could be reflected, a surface of clean mercury was prepared, *and it was found that when the rays were directed towards this surface sensitized films protected from direct radiation were fogged by some action coming from the mercury. To test this apparent reflection still further, a sensitized film, protected by a plate-holder, was placed at a distance of about twenty centimetres below the Crookes' tube. A thick plate of glass was then inserted midway between the* *tube and the film, parallel with the latter, with the intention of screening the plate in part from the action of the rays. The tube was then excited for some time, and on developing the film it was found that the rays evidently travelled in straight lines, since the part of the film protected by the glass plate was well defined and entirely unaffected by them. This experiment was repeated, the arrangement of apparatus being identical, with the sole exception that a glass bell-jar was placed over the whole. Development of the film in this case showed (1) no action on the film outside the jar; (2) no indication that the interposed plate glass acted as a screen; (3) the action much more intense than in the previous experiment, proving conclusively the reflection of the rays from the surface of the jar.*

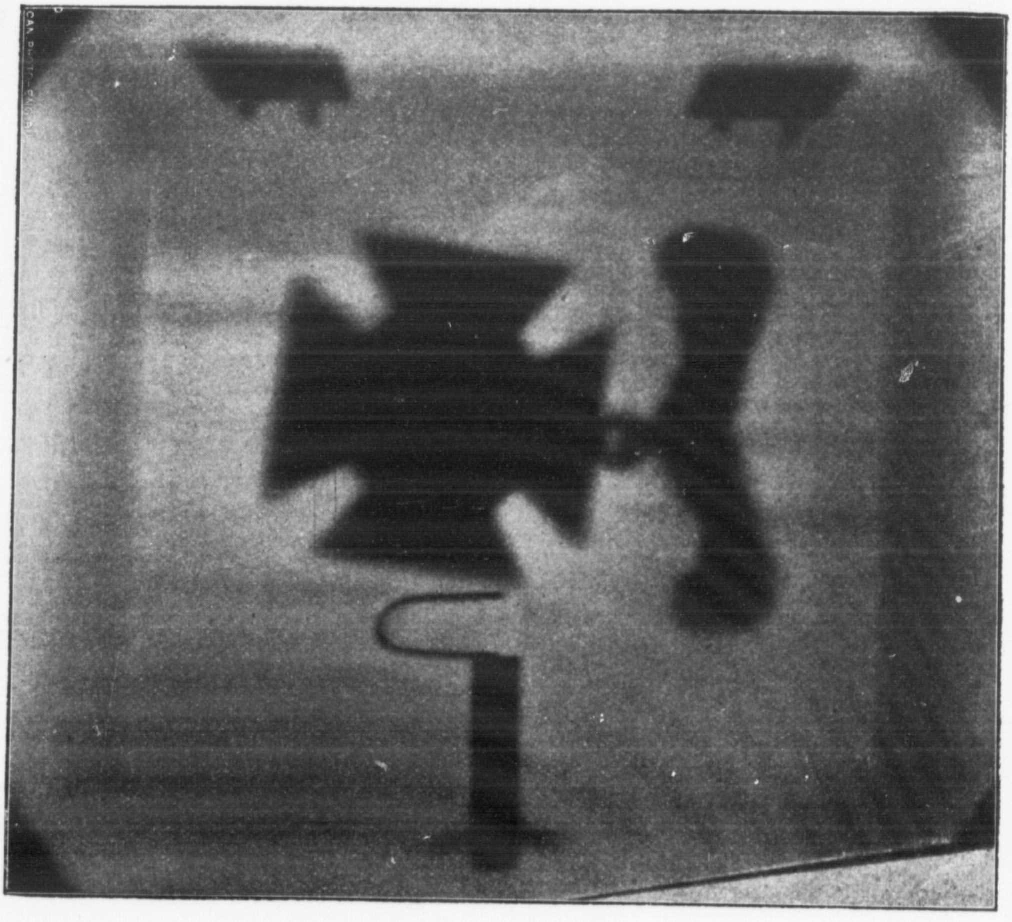


FIG. V.
Medal Photographed in its Case.

By the employment of this method we reduced on the 11th February the time of exposure almost to instantaneousness. The picture here given (Fig. V.) was taken with the bell-jar over the apparatus, and was obtained by an exposure of four and a half seconds, the object being a medal placed within a leather-covered wooden jewel case. Very good results were similarly obtained by an exposure of one second through five folds of black paper.

The importance of the new discovery in its application to surgery appears to be somewhat exaggerated. In detecting foreign bodies imbedded in animal tissue, much depends on the character of these bodies, and upon their particular location relative to the bones. In this connection it may be interesting to state that from a photograph of a patient's foot taken by us we located the point of a needle in it so accurately that the surgeon stated he was able by a single incision to remove it. As the needle in this case was situated between two of the bones, the conditions for obtaining a good shadow were rather favorable.

There have been many conjectures regarding the nature of this new radiation, but up to the present its true character remains quite uncertain. As already indicated, it does not pass through glass, and from this it has been concluded that although the cathode rays produce it, it is a form of radiation quite distinct from these rays. This conclusion is further confirmed by the fact that while cathode rays can be deflected from their direct course by a magnet, the latter has no effect on the radiation outside of the tube. It is generally conceded now that when the cathode rays strike upon the glass of the Crookes' tube, vibrations are set up in it which, on being communicated to the space outside, produce what we may now call the Roentgen rays. In fact, it seems to be proven conclusively that this view is the correct one, as Professor J. J. Thomson in his recent experiments found that a sensitized photographic plate placed *inside* a Crookes' tube in the path of the cathode rays was quite unaffected by them. Whether these rays are merely ultra-ultra violet rays, or whether they are due to vortex motions or to the longitudinal vibrations which are supposed to accompany the ordinary light vibrations in the ether, is a problem which has yet to be solved.

I cannot close this article without referring to the assistance given us by President Loudon, of the University of Toronto, and Professor Galbraith, Principal of the School of Practical Science. Much of the success which accompanied our experiments was due to the many valuable suggestions offered by them, and to their kindly placing at our disposal every facility which their laboratories could afford.

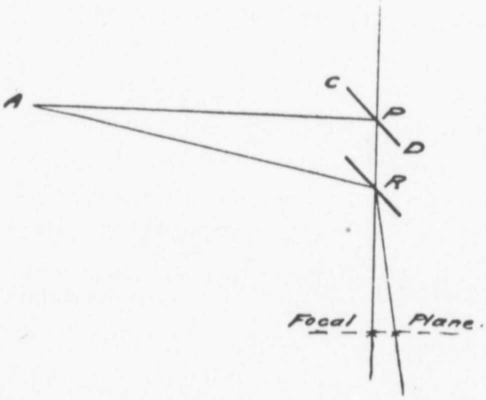
A SIMPLE FORM OF TELEMETER

By L. B. Stewart, D.T.S.,
Lecturer in Surveying, School of Practical Science.

The principle of the following simple form of telemeter occurred to the writer a few years ago, and an instrument constructed on that principle, in a preliminary trial, gave promise of such good results that a description of it is here given for the benefit of any who may have use for such an instrument. In facility of use it may not compare favorably with some telemeters used for military purposes, but in accuracy it is probably superior to some, and in portability it is all that could be desired.

The instrument consists of a small telescope, in front of the objective of which a triangular reflecting prism is mounted, with the reflecting surface at an angle of 45° with the axis of collimation of the telescope. The thickness of the prism is half the breadth of the objective, so that two images are formed at its focus, one that of an object situated on the axis of collimation produced, and the other formed by a pencil of rays which is bent through a right angle by the prism. It can be shown that if the two acute angles of the prism are equal, the direction of a ray of light after reflection and refraction by the prism is the same as if it had suffered no refraction. At the focus of the telescope are placed a number of equidistant parallel lines, like transit threads, and also a line cutting them at right angles; and by its intersection with the middle line defining the axis of collimation. Preferably, these should be lines etched on glass, on account of their permanency and the constancy of their intervals. They are placed so as to be parallel to the reflecting surface of the prism, and, moreover, they are adjusted so that when the instrument is set up at the vertex of a right angle, subtended by two points, they can both be made to appear on the middle line, one being seen directly and the other by reflection.

This completes the description of the instrument. In order to use it to obtain the distance to a distant point, such as A, set up the instrument on a tripod or other firm support at P, and turn the telescope until the point seen by reflection appears on the middle line; at the same time observe directly some point, as B, that appears also in the centre line, or plant a picket at that point. The point P is marked on the ground. The observer then moves the instrument back along the line BP until some



point R is reached, at which the point A appears by reflection on the first side thread, the telescope being directed along the line PB. The point R is also marked, and the distance PR measured. The distance AP may then be found by multiplying RP by a constant whose value depends on the focal length of the objective and the distance from the centre line to the line employed. A value of the distance AP may thus be found by means of each of the lines in the focus of the telescope.

It will be observed that the constants for the various lines diminish as their distances from the centre increase, and. therefore, as the corresponding points on the ground may be found with equal accuracy, the probable errors of the different determinations of the distance will be proportional to those constants, and their weights proportional to their inverse squares. In combining the measurements of the distance, each should therefore be multiplied by its weight and the sum of the products so formed divided by the sum of the weights. If the lines are sensibly equidistant, the weights of the determinations of a distance by the lines taken in order from the centre may be assumed to be the squares of the natural numbers 1, 2, 3, etc.

The constants of the instrument may be determined best by actual trial on measured distances. A good way to do this is to attach the instrument temporarily to a block which slides along a straight edge placed horizontally; the axis of collimation is adjusted parallel to the straight edge by so placing the instrument that some distant point remains on the central line, while the instrument is slid from one end of the straight edge to the other. Some well-defined object is then placed at a convenient distance off, at right angles to the direction of and in the same horizontal plane as the straight edge. A screen with a narrow vertical slit, behind which an illuminated white surface is placed, will answer this purpose well; the flame of a lantern is not as good on account of irradiation. The instrument is then shifted, bringing by reflection the image of the slit on each line in succession, and marking the corresponding point on the straight edge. The distance of the slit from the outer focus of the objective of the telescope is then measured, when the image of the slit is on the middle line, and the observer is then in possession of all the data necessary for the calculation of the constants. This operation may be carried on in any good-sized room, and a few determinations will give the values of the constants as accurately as can be desired.

Another necessary adjustment is to insure the parallelism of the lines and the reflecting surface of the prism. This may be done by placing the instrument in a horizontal position, bringing some point which is in the horizontal plane of the axis of collimation to the centre of the field of view by reflection, and then turning the diaphragm until the middle line coincides with a plumb line seen directly.

A few trials made with an instrument constructed for the writer leads to the conclusion that distances may easily be found with an error of less than one per cent., and possibly with a much smaller percentage of error by combining the results of all the lines.

BRICKWORK MASONRY

Results of tests made in the Laboratory of the School of Practical Science, Toronto, during the session of 1895-6, by Messrs. Wright and Keele.

By Jos. Keele, B.A.Sc.

BRICKWORK PIERS.

The following tests were made with the object of determining the resistance to crushing offered by piers of ordinary brick, constructed in the same manner and of the materials as those most commonly used in practice in Toronto. These materials will fairly represent those in use throughout the Province of Ontario.

For this purpose a bricklayer and his assistant were engaged to procure from four different brickyards a quantity of each of their grade of bricks, the bricks being taken from the kiln as they came to hand.

The different bricks used were Kingston Road, first, second, and third quality; Humber, first and second; Yorkville, first and second; Carleton clinker and first, and Don Valley pressed brick, buff and red.

An individual test of each class of brick was made to determine its crushing strength and absorption. The absorption test was made as follows: The dry brick was carefully weighed, then immersed in water, and at the end of twenty minutes the brick was taken out, the surface water dried off, and again weighed. The brick was again immersed until the total time of immersion was thirty minutes, and again weighed.

This was the longest time allowed in water, having found in former tests of the same nature that the absorption of water by the brick was practically complete in thirty minutes.

The table of absorption is given below:

Kind of Brick.	Weight Dry.		Weight after 20 Minutes in Water.		Weight after 30 Minutes in Water.		Absorption in Ounces.	Absorption in °/°
	lbs.	ozs.	lbs.	ozs.	lbs.	ozs.	ozs.	
Kingston Road, 1st class..	5	5¾	6	0	6	0	10¼	11 9
" " 2nd "	5	5½	6	2¼	6	2¼	12¾	14.9
" " 3rd "	5	1¾	6	2¼	6	2¼	14¼	17.1
Carleton Clinker.........	5	0¼	5	10	5	10½	10¼	12.7
" 1st class .	5	0½	5	13½	5	13¾	13¼	16.4
Yorkville ..... 1st class..	4	10½	5	11¾	5	11¾	17	22.7
" .....2nd "	4	11¾	6	0	6	0	20¼	26.7
Humber........1st "	5	7½	6	2½	6	2½	11	12.6
" ....2nd "	5	8	6	6¾	6	4¾	14¾	16.7
Don Valley Pressed, red..	5	13¾	6	6	6	6½	8¾	9.3
" " buff..	5	0	5	15½	5	15¾	15¾	9.7

To ascertain the crushing strength of each quality of brick, two fair and sound samples were selected and bedded between thin layers of Portland cement, thus giving two parallel planes without injury of any kind to the brick.

The following table shows the crushing strength of the different bricks used:

ULTIMATE CRUSHING STRENGTH OF COMMON AND PRESSED BRICK.

Class of Brick.	Height.	Area Exposed to Crushing in Inches.	Ultimate Load in Pounds.	Crushing Strength in lbs. per sq. inch.
Kingston Road, 1st class...	2½	8¾ x 4 35	132400	3783
" 2nd "	2½	9 x 4⅛ 37	62000	1670
	2½	8⅞ x 4⅛ 36.6	63000	1721
" 3rd "	2⅝	9 x 4⅛ 37	67600	1821
	2⅝	8⅞ x 4⅛ 36.6	68000	1857
Carleton Clinker..........	2½	8⅝ x 3⅞ 33.4	190000	5685
" 1st class...	2½	8¾ x 4 35	112000	3200
Yorkville.... .. 1st "	2⅝	8⅝ x 4 34.5	160000	4637
" ....2nd "	2½	8¾ x 4 35	107000	3057
	2½	8¾ x 4 35	112000	3200
Humber........1st "	2¼	8⅞ x 4 35.5	43400	1222
	2½	8⅝ x 4 34.5	50000	1449
" ....2nd "	2½	8⅞ x 4¼ 36.6	72000	1966
	2½	8⅞ x 4¼ 36.6	64000	1748
Don Valley........red....	2⅜	8⅜ x 4 34.3	184000	5372
" ....buff		8½ x 4⅛ 35	125000	3571

The piers were built by a skilled bricklayer, who also provided the lime mortar, which consisted of 4½ yards of Bloor street coarse sand to ten barrels of lime, this being about the proportion of two parts sand to one part lime. The cement mortar was mixed in the proportion of three parts sand to one part of good Portland cement. While the piers were being built, two cubes of each class of mortar were prepared and set aside for the purpose of ascertaining their resistance to crushing, thus giving a complete record of all the materials used.

ULTIMATE CRUSHING STRENGTH OF MORTAR 2½ MONTHS OLD.

Class.	Height. Inches.	Area Exposed to Crushing in Inches.	Ultimate Load in Pounds.	Crushing Strength in lbs. per sq. Inch.
Lime mortar—2 to 1.......	5	4⅝ x 4⅞ 22.5	1200	53
Lime mortar—2 to 1.......	4⅞	4⅝ x 4¾ 22	1700	77
Cement mortar—3 to 1	5	5 x 5 25	33800	1352

The piers were built and laid aside to harden in the mechanical laboratory of the School of Practical Science, in a temperature which averaged about 60° Fahrenheit, and were prepared for the test as follows: A thin mortar of neat cement was spread on a smooth cast-iron plate, and the pier placed upon the mortar and left until the cement hardened. The bottom bed was then trimmed off flush with the sides, the pier placed on the testing machine, and a layer of neat Portland cement mortar was placed on top, the pier was slid under the head of machine, and the head was brought to its bearing while the mortar was yet soft.

This method ensured two parallel beds and gave a uniformly distributed stress on the pier. The load was applied slowly and continuously, until complete failure of the pier occurred.

Pier No. 1:

Description.... Humber, 1st class, laid in lime mortar ⅜″ joints.	
Size of pier, 9″ × 9″	area, 81 square inches.
Length, 24 courses	73 inches.
Age	10 days.
Ultimate load	23,600 pounds.
" strength per sq. inch	291 pounds.
" " " foot	20.9 tons.

This pier was built on the testing machine; with lime mortar on top and bottom bed, the head of machine was brought down to a level bearing, and pier allowed to harden in position for ten days.

The pier failed by spreading a little at the head, a wide crack running down the centre to about half the height of the pier.

Pier No. 2:

Description.... Kingston Road, 1st class, laid in lime mortar with ⅜″ joints.	
Size of pier, 8⅞″ × 8⅞″	area, 78.75 square inches.
Length, 8 courses	23 inches.
Weight	114 pounds.
Age	2½ months.
Ultimate load	44,000 pounds.
Crushing strength per square inch	558 pounds.
" " " foot	40.2 tons.

The pier sustained a high load without sign of fracture, but was completely destroyed under the ultimate load.

Pier No. 3:

Description.... Kingston Road, 2nd class, laid in lime mortar with ⅜″ joints.	
Size of pier, 9″ × 9″	area, 81 square inches.

Length, 8 courses..24 inches.
Weight...114 pounds.
Age..2½ months.
Ultimate load..22,000 pounds.
Crushing strength per square inch...... } Not determined by
" " " foot....... } experiment.

The bottom bed used in this case was the one-inch board upon which the pier was originally built; the board appeared to be slightly warped, and split under the application of the load, causing a variation in the stress, to which is due the early failure of the pier.

Pier No. 4.

Description..Kingston Road, 3rd class, laid in lime mortar with ⅜″ joints.
Size of pier, 9″ x 9″....................area, 81 square inches.
Length, 8 courses..24 inches.
Weight...110 pounds.
Age..2½ months.
Ultimate load..24,000 pounds.
Crushing strength per square inch...............296 pounds.
" " " foot...................21.3 tons.

Failure occurred by splitting of the bricks in the upper courses, then wide vertical cracks opened throughout the whole length, and under highest load every brick in the pier was shattered.

Pier No. 5:

Description..Humber, 1st class, laid in lime mortar.
Size of pier, 9″ x 9″....................area, 81 square inches.
Length, 8 courses..24 inches.
Weight...122 pounds.
Age..2½ months.
Ultimate load..28,000 pounds.
Crushing strength per square inch...............346 pounds.
" " " foot...................24.8 tons.

The pier held well together until near the ultimate load, then long continuous cracks appeared, with final rupture of the whole pier.

Pier No. 6:

Description..Humber, 2nd class, laid in lime mortar.
Size of pier, 8¾″ x 8¾″..............area, 76.5 square inches.
Length, 8 courses..23½ inches.
Weight...118 pounds.

Age	2½ months days.
Ultimate load	22,400 pounds.
Crushing strength per square inch	293 pounds.
" " " foot	21 tons.

All the bricks in the upper portion were completely shattered, the principal failure occurring along one corner of pier.

Pier No. 7:

Description	Carleton Clinker, laid in lime mortar with ⅜" joints.
Size of pier, 8⅝" x 8⅜"	area, 72 square inches.
Length, 8 courses	22 inches.
Weight	114 pounds.
Age	2½ months.
Ultimate load	44,000 pounds.
Crushing strength per square inch	609 pounds.
" " " foot	43.8 tons.

The pier failed, with continuous lines of fracture up and down the four sides, only one brick on the lower bed being uninjured after the test.

Pier No. 8:

Description	Carleton, 1st class, laid in mortar with ⅜" joints.
Size of pier, 8¾" x 8¾"	area, 76.5 square inches.
Length, 8 courses	23½ inches.
Weight	110 pounds.
Age	2½ months.
Ultimate load	41,000 pounds.
Crushing strength per square inch	535 pounds.
" " " foot	38.5 tons.

The pier was completely shattered under the highest load. The mortar crumbled out like sand, and had very little effect in holding any portions of the pier together.

Pier No. 9:

Description	Yorkville, No. 1, white brick, laid in lime mortar with ⅜" joints.
Size of pier, 8¾" x 8¾"	area, 76.5 square inches.
Length, 5 courses	14½ inches.
Weight	65 pounds.
Age	2½ months.
Ultimate load	39,000 pounds.
Crushing strength per square inch	509 pounds.
" " " foot	36.6 tons.

Small cracks appeared as the load was put on. As the highest load was approached portions of the side of the pier spalled off, and finally shattered to fragments under the highest load.

Pier No. 10:

Description.. Yorkville white brick No. 2, with pinkish shade, laid in lime mortar with ⅜″ joints.

Size of pier, 8¾″ x 8¾″	area, 76.5 square inches.
Length, 8 courses	23½ inches.
Weight	105 pounds.
Age	2½ months days.
Ultimate load	30,000 pounds.
Crushing strength per square inch	392 pounds.
" " " foot	28.2 tons.

Fine cracks appeared early in the test, which increased to long vertical cracks, running the length of the pier, portions of the brick spalled off, and under the highest load given above the pier was totally destroyed.

Pier No. 11:

Description.. Don Valley pressed brick, buff color, laid in lime mortar.

Size of pier, 8⅝″ × 8⅝″	area, 74.4 square inches.
Length, 8 courses	21 inches.
Weight	97 pounds.
Age	2½ months.
Ultimate load	51,000 pounds.
Crushing strength per square inch	686 pounds.
" " " foot	49.4 tons.

As the highest load was approached, fine cracks appeared, which were confined to individual bricks, and were not continuous down the length of the pier; the fracture was rather of a crumbling nature.

Pier No. 12:

Description.. Don Valley pressed brick, red color, laid in lime mortar.

Size of pier, 8½″ × 8½″	area, 72.25 square inches.
Length, 8 courses	21½ inches.
Weight	110 pounds.
Age	2½ months.
Ultimate load	88,000 pounds.
Crushing strength per square inch	1,218 pounds.
" " " foot	87.7 tons.

The failure of this pier was of somewhat the same nature as that of the last, but the brickwork held together better under the ultimate load.

CEMENT PIERS.

Pier No. 13:

Description	Yorkville, 1st class, white color, laid in cement mortar.
Size of pier, 8⅝″ × 8⅝″	area, 74.4 square inches.
Length, 8 courses	24 inches.
Weight	110 pounds.
Age	2½ months.
Ultimate load	79,000 pounds.
" " per square inch	1,062 pounds.
" " " foot	76.5 tons.

The pier held together well, and did not show much sign of failure until the highest load was reached; the pier was destroyed in the test, probably owing to the brittle nature of this brick.

Pier No. 14:

Description	Yorkville, 2nd class, color white with pink tint, laid in cement mortar.
Size of pier, 8¾ × 8¾	area, 76.5 square inches.
Length, 8 courses	24 inches.
Weight	111 pounds.
Age	2½ months.
Ultimate load	78,000 pounds.
" strength per square inch	1,018 pounds.
" " " foot	73.3 tons.

This pier was well built, and shows the value of a cement mortar for laying brickwork, as its binding qualities allows the brick to develop nearly its full strength.

Pier No. 15.

Description	Humber, 2nd class, laid in cement mortar.
Size of pier, 9″ × 9″	area, 81 square inches.
Length, 8 courses	24 inches.
Weight	124 pounds.
Age	2½ months.
Ultimate load	91,600 pounds.
" strength per square inch	1,131 "
" " " foot	81.4 tons.

Fine cracks occurred in some of the bricks only under nearly the highest load, but total destruction of the pier took place under the ultimate load, but did not shatter so badly as in the case of those laid in lime mortar.

Pier No. 16 :

Description	Kingston Road, 2nd class, laid in cement mortar.
Size of pier, 9″ + 9″	area, 81 square inches.
Length, 8 courses	23½ “
Age	2½ months.
Ultimate load	69,000 pounds.
“ strength per square inch	852 “
“ “ “ foot	61.3 tons.

This pier held together even under the ultimate load, the failure occurring through actual crushing of some of the upper bricks. After pier was removed from the machine, only small portions of it could be forced away from the mass.

Pier No 17:

Description	Carleton Clinker, laid in cement mortar.
Size of pier, 8½ × 8½	area, 72.25 square inches.
Length, 8 courses	22¾ inches.
Weight	115 pounds.
Age	2½ months.
Ultimate load	174,000 pounds
“ strength per square inch	2408 “
“ “ “ foot	173.4 tons.

Fine cracks appeared toward the end of test ; these cracks were not continuous down the length of pier, nor did they increase much in width under the highest load.

Pier No. 18 :

While working on this pier the friction clutch of the machine gave way, and the tests were discontinued for the present.