ART. XVIII.—*Determination of the Acceleration due to the Force of Gravity, at Tokio, Japan;* by T. C. MENDENHALL.

THE following series of determinations of the value of the acceleration due to the force of gravity at this place was begun in the month of February of the present year and continued throughout the succeeding two or three months. A considerable time was devoted to preliminary experiments, and there was some delay on account of the non-arrival of one of the pendulums used, from the maker in Europe. Throughout the whole series and in all of the labor connected with it I have had the intelligent and faithful assistance of Messrs. Tanaka and Tanakadate, two special students in the department of Physics of the Imperial University of Japan.

There is probably nothing new in the method employed, except, perhaps, the mode of determining the time of vibration of the pendulum. As far as I am aware this method has not been previously described, and it seems to possess many advantages over the ordinary method of coincidences. It simply involves the use of a good chronograph and a break-circuit clock or chronometer, together with an arrangement by means of which the experimental pendulum can be made to record its own beats upon the chronograph at any time. In the beginning the whole number of vibrations which the pendulum will make in a given time may be determined by letting it break the circuit at every vibration, or, better, at every sixtieth or hundredth vibration, which can easily be accomplished by counting and raising the break-circuit apparatus to its proper position underneath the pendulum at the right moment. In our arrangement this apparatus consisted of a very small and light "trip-hammer" made of fine wire, which was so adjusted that by pressing upon a button it was brought up to such a point that it would be just "thrown" by the pendulum in its passage through the lowest point of its arc. Although the resistance offered to the pendulum can be made extremely small, yet it is so great as to interfere quite perceptibly with its motion if the pendulum is obliged to operate the break-circuit at each beat, as experiment has proved. But it may be rejected after the first two or three trials, not only on account of the resistance which it introduces but also because it is not necessary to continue its use. The whole number of seconds required for a given number of vibrations being known, it only remains to determine the fractional part of a second as accurately as possible. It is therefore only necessary to cause the pendulum to break the circuit twice, once at the beginning of the period and once again at the end. By this means all objection to the process on account of resistance is removed. Indeed it is in the possibility of determining these fractional parts of a second at the beginning and at the end, that the merit of this method consists. The chronograph used in these determinations is by Alvan Clark and Sons, and for uniformity of speed it is everything that could be desired. The line made by the pen is sharp and clear. The length of one second on the sheet is about 8 mm., so that it can be easily measured with a microscope of low power with a micrometer eye piece. It will easily be seen that even if the total time during which the pendulum is made to swing be not great, its value can be ascertained within a very small fraction of itself. By this process, therefore, it becomes possible to make the duration of the experiment extremely short compared with that required in the method of coincidences and yet to reach the same degree of accuracy. As a proof of this it may be stated that in numerous instances in which the duration of the experiment was only twenty minutes, three independent measurements of the total time, made from the chronograph sheet, did not differ among themselves by more than one sixty-thousandth part of the whole. The advantages in thus reducing the whole duration of the experiment from hours to minutes are many. All of the conditions may be maintained nearly constant during the whole time of the swing, and this is especially important in regard to temperature and arc of vibration, the latter being also made much smaller to begin with than would otherwise be possible. Again, the method eliminates "judgment" to a great extent as the pendulum marks for itself the beginning and the end of the period of time. Another important gain is that the use of the clock may be dispensed with, and, without loss of accuracy, the break-circuit chronometer substituted, thus rendering the whole apparatus for such a determination easily portable.

The first series of experiments undertaken was with a Kater's pendulum. The adjustment of the sliding weights, for the purpose of making the times of vibration when suspended from either knife edges equal to each other, is a matter of considerable difficulty and involves much labor. In the present instance no attempt was made to secure a closer agreement between the two periods than that of those recorded below, for the reason that the difficulties in the way of obtaining the exact length of the pendulum were such as to make any greater degree of accuracy as far as time is concerned unnecessary. There being at that time no standard of length in the possession of the University, recourse was had to a standard bar three meters in length, made by Troughton, belonging to the Department of Public works. As the pendulum was approximately one meter long it was necessary to "triplicate" it in order to compare it with this bar. The latter is provided with reading microscopes, and by repeating the comparison two or three times the result given below was obtained and it is believed to be not more than a few hundredths of a millimeter from the truth. The results of six determinations of the time of vibration are given below in columns A and B, the first set being made upon one of the knife edges, and the second upon the other. The values of "*g*" corresponding to these are also given.

Length of pendulum at 0°		1·00069 meters.
" "	at 11°·5 (vibrating temp.)	1·00090

Time of vibration, corrected for arc and chronometer.

A.	B.
1·00410	1·00412
1·00412	1·00417
1·00410	1·00417

Values "*g*."

A.	B.
9·7980	9·7976
9·7976	9·7966
9·7980	9·7966

These give a mean value of 9·7974.

It must be remembered, however, that these results are not corrected for buoyancy, in other words they are still to be reduced to a vacuum. This correction has not been applied for two reasons: first, because the result cannot be considered as anything more than a first approximation, and, second, because I am by no means certain as to just what it ought to be. There is no doubt but the correction must be different in the two cases of suspension, when the heavy ball of the pendulum is above the knife edge and when it is below, as was experimentally demonstrated by Sabine many years ago. I believe Sabine also investigated the actual correction to be applied to Kater's pendulum, but owing to the very limited library facilities afforded residents of this country I am unable to refer to his results. When corrected, the above mean will undoubtedly somewhat exceed the true value, but no great degree of accuracy was attempted either in the adjustment of the pendulum or in the measurement of its length.

A "Borda's Pendulum," made by Salleron, was received soon after the conclusion of the above experiments, and as it was accompanied by a standard measuring apparatus it was determined to undertake a more accurate and a more extended series of observations for the determination of the value of "*g*."

The pendulum was of the well known form used by Borda and by many others since his time. The ball was of brass, and upon examination was found to be very closely spherical and homogeneous. It could be suspended by means of a small concave cup to which the ball would cling when it was rubbed with a little tallow. To this cup a suspending wire was fastened, the other end of which was secured to the end of a small cylinder projecting down from the knife edge. In a line with this and above the knife edge was a set of adjusting screws, by means of which this part of the apparatus, independent of the wire and ball, was made to vibrate in approximately the same period as that of the pendulum. Experiment proved that this adjustment might be very considerably disturbed without sensibly affecting the time of vibration of the pendulum, but it was, nevertheless, carefully attended to. After a number of trials the cup was rejected as a useless and somewhat uncertain part of the apparatus, and in its stead, in the final series of experiments, the wire was securely fastened to the ball by means of a small drop of solder which was fused on the end of the wire and afterward brought in contact with the ball while the latter was heated. The rejection of the cup greatly simplifies the calculation of the reduced length of the pendulum and lessens the probability of error in the measurements. A number of trials were made to determine the most desirable suspending wire. Theoretically an exceedingly fine wire is best, but practically it was found that much trouble was occasioned by the almost constant stretching and final breaking of very fine wires, so that it was found best to employ a wire of sufficient strength to assure, after a few days' suspension, a pendulum of invariable length during the time of the experiment, subject, of course, to slight changes due to variable temperature. The wire used at last was of platinum, which is desirable on account of its low coefficient of expansion. The measuring apparatus was similar to that used by Borda. The great advantage of this method is that the pendulum can be measured *in place*, or at least it must only be removed to give place to the measuring rod, the actual adjustment for length having been made before. This pendulum, as in the previous case, was suspended from a strong iron support secured to the top of a stone pier about six feet in height and two feet square. The knife edge of the pendulum rested on a pair of agate plates which were accurately leveled by means of four leveling screws and then firmly secured, so that motion was impossible. At the base of the stone pier and firmly secured to it was a strong wooden beam, upon which could be placed the apparatus for breaking the circuit, and also the small circular plane table which was elevated by means of a screw until it was tangent to the sphere at its lowest point. The measuring rod which could be substituted for the pendulum was of iron, and the length of the rod was read by means of a vernier and microscope to hundredths of a millimeter. There was also a metallic thermometer attached for giving the temperature of the bar. The reading of the length of the rod was always taken while the rod was in position upon the agate plates, and its temperature was read at the same time. The deflection of the support of the pendulum due to the addition of the weight of the measuring rod was determined to be about ·02 mm. and correction was made for it. As the apparatus was arranged it was very easy to make a measurement of the length of the pendulum, and measures were taken at very short intervals, always both before and after a series of vibrations. It was found, however, that when the temperature was constant the length remains sensibly the same. A number of comparisons were made between the meter upon the measuring rod and a standard meter from the Finance Department, made by Deleuil, both of which ought to be correct at 0°. The result of these comparisons was such that a correction of $-$·04 mm. was made upon the length of the measuring rod at 0°. The pendulum was suspended in a small room favorably situated as regards all disturbances from air currents and sudden changes in temperature. From this room wires were carried to the chronograph, which was in a room near by. The time was taken from a break-circuit chronometer in the transit room of the Astronomical Observatory. The chronometer was not moved from its place, the observatory being connected with the physical laboratory, from which it is distant nearly two miles, by a telegraph line, so that the beats of the chronometer were recorded upon the chronograph in the laboratory. The rate of the chronometer was determined by star transits observed for several nights in succession before and after the date of the observations recorded below. The results given are the periods of vibration in mean solar time, corrected for chronometer rate and also for arc of vibration, the latter being observed by means of a scale and a telescope about fifteen feet away. The mean arcs varied, in the different experiments, from 40′ to 70′.

After having found the total length of the pendulum, as well as the dimensions and masses of its various parts, the "reduced length," or the length of the equivalent simple pendulum, is computed by means of a well-known formula. This with the time of vibration gives the value of "g" in air, and to this must be added the correction for "buoyancy." In most of the earlier determinations by this method this correction was found by simply comparing the density of the pendulum with that of the air in which it vibrated. Although it was shown, before Borda made his experiments, that this correction was too small, he seems to have been ignorant of the fact, and not until Bessel again investigated the question a good many years later, was general attention called to the fact. About the year 1830, Baily made an extensive series of experiments with a variety of pendulums, for the purpose of determining the true value of this correction. Unfortunately I am unable to refer to his results directly, but I believe that his conclusion was that for such a pendulum as was used in these determinations the ordinary correction should be multiplied by 1·5, and this factor has been used in correcting the results given.

Below will be found the results of eleven different determinations, the first five of which were made on May 26, and the others on May 27. On both days during the time of vibration, all of the conditions were sensibly constant and the same, and in addition to this the nights were favorable for the determination of the chronometer rate. Each of the results is based upon an experiment of twenty minutes' duration, the time of vibration in each case being the mean of two or three independent measurements of the chronograph record made by different persons.

The value of "*g*" is calculated for each time of vibration determined, and each is corrected for buoyancy. These include all of the determinations made upon those two days, none having been rejected.

Determination of the value of "*g*" at Tokio, Japan.

Latitude N. 35° 41′, long. E. 139° 44′. Ht. above sea level, 5 meters.

Total length of pendulum,	1014·18 mm.
Distance from knife edge to wire,	46·50 "
Length of wire,	931·62 "
Radius of ball,	18·03 "
Weight of ball,	198·951 grm.
" wire,	1·913 "
Density of ball,	8·0
Length of equivalent simple pendulum,	994·59 mm.

Time of vibration.	Corresponding value of "*g*."
1·00103	9·7982 meters.
1·00100	9·7988 "
1·00103	9·7982 "
1·00104	9·7980 "
1·00103	9·7982 "
1·00101	9·7986 "
1·00102	9·7984 "
1·00101	9·7986 "
1·00103	9·7982 "
1·00101	9.7986 "
1·00100	9·7988 "

Mean of all results, $g = 9·7984$.

This result is slightly greater than that given by many of the formulas for computing the value of "*g*" in any latitude.

An excellent opportunity is offered in Japan for measuring the force of gravity at a considerable height above the sea level, in the great extinct volcano, Fujiyama, which reaches a height of between 12,000 and 13,000 feet. An excursion is being arranged for the purpose of making this determination during the coming summer. For this purpose what may be called an "invariable" pendulum is now being vibrated in the place at which the above result was obtained. Its period will be carefully ascertained here before carrying it to the mountain, then on the top of the mountain, and again here after it has been brought back.

Nearly all of the labor of the above determination had been concluded when the April number of the Philosophical Magazine reached Japan. It was found to contain a paper by Messrs. Ayrton and Perry, on a "Determination of the Acceleration of Gravity for Tokio, Japan," which was based on experiments made by the authors, at the College of Engineering in this city, in 1878. Before beginning the series of experiments described above, I had learned that some work in the same direction had been done in the College of Engineering. Messrs. Ayrton and Perry being no longer in Japan, I was unable to ascertain, although inquiry was made, anything very definite concerning their methods or results. I was led to believe, however, that they had made no attempt at a very precise determination. Their paper, as now published, puts the matter in a somewhat different light, and, as their result differs from that deduced above, I desire to call attention to some points in connection with their method and their calculation. The data furnished in the paper are by no means as complete as would be desirable for a thorough discussion of its value, but much may be learned by an examination of what it does contain.

The pendulum used by Messrs. Ayrton and Perry was nearly ten meters in length. There are serious objections to the use of a long pendulum. Borda, in his celebrated determinations made at Paris, used a pendulum about four meters long, but one which approximates in length to a seconds pendulum has been almost universally made use of since. The great objection to the use of a long pendulum is the difficulty of measuring it *in place.* Messrs. Ayrton and Perry measured their pendulum by placing it in a horizontal position, and stretching it by allowing the end near the ball to hang over a wheel with very little friction. The length was obtained by comparison with a bar one meter long, and as this bar must be placed ten times to cover the whole length, it is plain that any great degree of accuracy must have been difficult to obtain, and this is especially true when the measurement of that portion of the wire which hangs over the wheel is considered. Their 26th experiment was made on the 25th of January, and the 53d on the 21st of February, from which we may infer that the entire time of suspension was at least two months. As only one measurement is spoken of, it is probable that it was measured at the conclusion of the series of experiments, and it seems hardly likely that its length would have remained constant during that length of time. In getting the time of vibration the first method used was what might be termed the method of coincidences by electricity, and which, so far as I know, was first described by Professor Pickering, in his excellent "Physical Manipulations." This was afterward rejected, however, and the vibrations were counted by means of a Morse instrument. The authors speak of measuring the fraction of a vibration, but evidently this could not be done with accuracy by the use of such an arrangement, and there is also the objection that the pendulum was obliged to do the work of breaking the circuit at every vibration. Messrs. Ayrton and Perry give the time of vibration of their pendulum for only three experiments, and it is a little difficult to understand exactly how these were obtained. The time, taken from the chronometer, is given and also the number of vibrations. Any one who will take the trouble to divide one by the other, will obtain results differing very materially from those given in the paper. The only way out of this difficulty that occurs to me, is to consider the times given as the apparent times corrected for clock error, and this I shall do, although it involves the somewhat violent assumption that the chronometer employed had, in the first experiment, a "losing rate" of about 1 minute and 15 seconds per day, which, at the time of the second experiment had changed to a slight "gaining rate," and had fallen again to a "losing rate" at the third. Supposing, however, that this was the case, it will be observed that the periodic time used in the calculation is considerably greater than that of either of the three experiments given. This time, 3·0748 seconds, is doubtless the mean time, and in the absence of the other results it may be fairly assumed that there was at least one result which was as much greater than the mean as the least of those quoted in the paper is less. On this supposition, the extreme times of vibration of their pendulum will be 3·0741 seconds and 3·0755 seconds. Substituting these numbers in their formula, the values of "g" obtained would be, respectively, 9·7997 meters and 9·7907 meters. These results are not corrected for buoyancy, but the same wide range would exist after that correction is made. But as nothing is known concerning the experiments not quoted, the most favorable case imaginable may be assumed. Suppose that in addition to the three results given, they were in possession of an infinite number of others, all agreeing exactly with that which is used in their computation. The extreme results would still cover a wide range, from 9·7997 to 9·7952. Instead of the latter number they give 9·7958, but they have made a miscalculation in reducing the formula, the true value being as given above.

Although making some elaborate calculations concerning corrections which may be rejected, they reject, apparently without calculation, the correction for the arc of vibration. The arc through which their pendulum swung was nearly 2°, and if a correction for this be applied it will materially alter the last figure of their result. In applying the correction for buoyancy they seem to have corrected only for the ordinary density of the ball and not for its "vibrating density." The correction which they apply is ·0016 meters, and if this be multiplied by 1·5 and the correction for arc also made, their result will be 9·7979 instead of 9·7974, as given in their paper. They refer to the close agreement of their result with that deduced from Clairault's formula, which they make to be 9·797. But they have misquoted Clairault's formula, putting γ where there should be 2γ. This is doubtless a typographical error, but they have also miscalculated it, for it gives 9·7980 meters as the value of "g" at this latitude, and not 9·797 meters, as stated in their paper.

Taking all of these facts into consideration, it does not seem that great weight can be attached to their result, notwithstanding its close agreement with the calculated value for this place, and it is doubtful whether the "bounds of existing knowledge" have been advanced, in any great degree, by this investigation.

Tokio, Japan, June 2, 1880.*