Summary of Results.

Iron.—Tension diminishes the magnetic elongation of iron, and causes contraction to take place with a smaller magnetising force.

Nickel.—In weak fields the magnetic contraction of nickel is diminished by tension. In fields of more than 140 or 150 units, the magnetic contraction is increased by tensional stress up to a certain critical value, depending upon the strength of the field, and diminished by greater tension.

Cobalt.—The magnetic contraction of cobalt is (for magnetic fields up to 500 C.G.S. units and loads up to 772 kilos. per sq. cm.) practically unaffected by tension.

III. "On the Heat of the Moon and Stars." By C. V. Boys, A.R.S.M., F.R.S., Assistant Professor of Physics, Normal School of Science and Royal School of Mines, London. Received April 14, 1890.

Soon after I had completed the radio-micrometer and shown its great superiority over any form of thermopile and galvanometer, I was naturally anxious to carry out some research which would clearly demonstrate the capabilities of the instrument. The determination of the heating powers of the stars seemed most promising, for Dr. Huggins had, in 1869,* made experiments on the heating powers of some of the stars which, though they did not conclusively show that a thermopile was capable of measuring so minute a radiation, yet made it exceedingly probable that the effects observed, if not very exact in quantity, were at any rate real. Dr. Huggins, however, described his experiments and formed his conclusions with the utmost caution. A year later Mr. Stone described experiments which he had made with the great equatorial at Greenwich.† He at first used small thermopiles, but soon found, as we should expect, that a single pair was more sensitive to radiation brought to a point than a pile of many pairs. In attempting to obtain great sensibility by giving the galvanometer a long period he found it almost impossible to use the apparatus on stars at night. Every slight change in the sky, even though quite invisible to the eye, so disturbed the galvanometer that it was impossible to distinguish effects due to the stars from those caused by the varying clearness of the sky. Mr. Stone largely obviated this difficulty by placing in the focal plane of the object glass a couple of thermo-electric pairs so connected that a heating of the exposed face of one would produce an effect opposite

* 'Roy. Soc. Proc.,' vol. 17, p. 309.

† *Ibid.*, vol. 18, p. 159.

in kind to that produced by a heating of the exposed face of the other. Under these conditions a change in the sky which would act on both faces alike, or nearly so, would not disturb the galvanometer, whereas a star made to shine first on one face and then on the other would cause a deflection first in one direction and then in the other. This arrangement had previously been employed by Lord Rosse, in his experiments on the heat of the Moon.* The pairs used by Mr. Stone were about 31 mm. long and had a sectional area of about 4 square mm., that is, each bar was about 31 × 2 × 1 mm. Wires were taken to a distant galvanometer and the telescope was set with the image of the star alternately on the two faces. About 10 minutes were allowed before a reading was taken. The rays of Arcturus concentrated by the $12\frac{3}{4}$-inch object glass produced deviations of from 20—30 divisions of the scale, while a 3-inch cube of boiling water at two feet from the faces produced a deflection of about 150 divisions. Mr. Stone concluded that the face of the pile was heated through about 1/50th of a degree Farenheit. With these figures before me, I had no doubt that the radio-micrometer, which in sensibility vastly exceeds the thermopile, while unlike the thermopile and galvanometer it is free from disturbing effects of magnetism and outside changes of temperature and has the further advantage —and for astronomical work this perhaps is even more important— that a measure can be made in *five seconds* instead of several minutes which are necessary with the older apparatus, would be capable not only of making good and exact measures of the heat of the brighter stars, but I went so far as to hope that even faint stars would produce an appreciable effect and that most interesting results might be derived from an examination of planets, comets, nebulæ, and the red stars.

I therefore determined to put the radio-micrometer to a severe test, and one which promised not only to show its suitability for such delicate work, but at the same time to give much valuable information. The Royal Society gave me, out of the Government Grant, a sum of £50, which, thanks to the advice which I received, especially from Mr. W. H. Massey, and Mr. A. A. Common, in the matter of design and construction, was nearly sufficient to meet all the expenses which I have incurred. I should say also that Mr. Paxman, of Colchester, who made the steel tube, which is a beautiful example of miniature boiler construction, kindly presented this in the cause of science; that I have been able to use some few pieces of apparatus belonging to the Physical Laboratory, at South Kensington, such as the large magnet of about 25 lbs. for the radio-micrometer, some of the lime-light apparatus, and the finder; and,

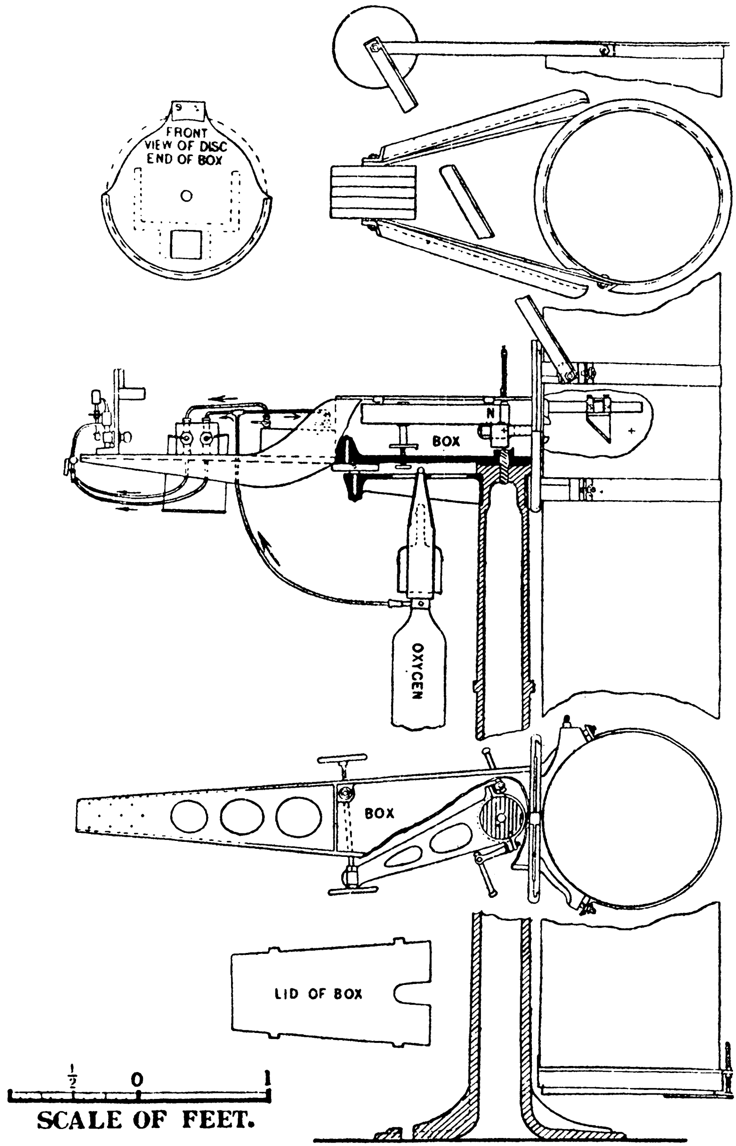
* 'Roy. Soc. Proc.,' vol. 17, p. 436.

finally, that Dr. Huggins has kindly lent me a silver-on-glass mirror of nearly 16 inches aperture and very short focus (67·8 inches) of his own construction, which was exactly what I wanted for this purpose. I accordingly prepared a design for a mounting of this mirror, such that, no matter in what direction the telescope might be pointed, the focus of the rays from a star should be always at one fixed point at which the receiving surface of the radio-micrometer could be suspended. I should say here that in this one particular the radio-micrometer is at a disadvantage by comparison with a thermopile or bolometer; it must not be tilted, it cannot be fixed into an eyepiece and pointed and moved about at will; it must be level, though it may, with its lamp and scale, be carried on a level platform and turned about a vertical axis. This disadvantage, which, however, disappears when the whole apparatus is designed for and is made to suit the radio-micrometer, is more than counterbalanced by the absence of connecting wires with a thermo-electric junction at every binding screw and by the absence of the galvanometer, which is ever ready to give indications on its own account.

In the design of the mounting I have, to a large extent, been obliged to follow certain lines. Thus it is evident, if a large siderostat is not to be had so that the telescope may be fixed, which would be the most convenient plan to adopt, that the mounting must be altazimuth, that the horizontal and vertical axes of motion must intersect, and that the focus, which must be just outside the tube, must be on or close to the vertical axis. With this arrangement trunnions to the telescope would be very inconvenient, as they would necessitate a long and awkward curved arm, which would prevent the telescope from being turned over from east to west, or from north to south. Accordingly, I have adopted the plan suggested by Mr. W. H. Massey, of carrying the telescope by a large disc on its side, resting and turning in an under-cut groove. Fig. 1 shows the chief details of construction finally determined upon, and fig. 2 the finished instrument in position. The radio-micrometer must be protected from stray radiation and from the effects of hot and cold air currents. I therefore arranged that the space at the back of the under-cut disc should be made in the form of a box with a movable lid, all of thick cast-iron, so that the changes of temperature inside should be sufficiently gradual; then the nature of the radio-micrometer would prevent such changes from producing any disturbing effect. The floor of this box continued away from the telescope forms a convenient base for holding the lamp (ether oxygen lime light) and scale.

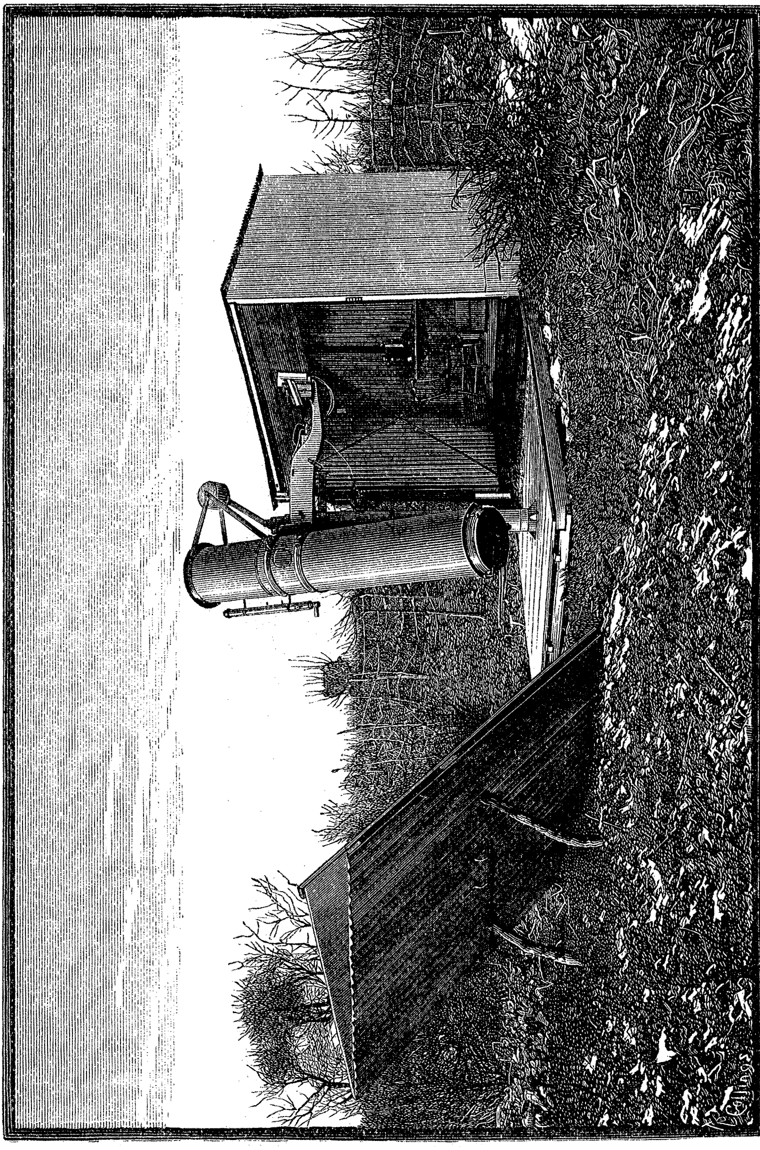
The telescope tube carried by the disc on its side must be balanced about the centre of this disc. The large cast-iron weights carried by four rods are in such a position as to balance it exactly; moreover the

Fig. 1.



box and girder and the apparatus carried are sufficient to nearly balance everything on the head of the vertical column, and so relieve the holding-down bolt of all strain.

Fig. 2.



The vertical column appears too slender. It was, however, important to get the focus as near the side of the telescope as possible, and, as the focus should be on or close to the vertical axis, it is clear

that the column must be as slender as possible so long as its stability is not impaired. The further the focus is outside the telescope, the larger must be the plane mirror. As this is a disadvantage both on account of expense and of obstruction of useful rays, I have materially reduced the size of the flat by placing it well on one side of the axis. The resulting want of definition I found by calculation to be for my purpose of no importance. So long as all the rays fall on the sensitive surface, which in some cases is 1 mm. square, the definition is sufficiently good. With the very low power eyepiece that I use I cannot detect anything more than a point in the image of a star.

The radio-micrometer which I made for this purpose is unusually large and massive, and, as the suspended circuit is hung in a narrow hole drilled in this great mass of solid metal, differences of temperature in different parts of the circuit can hardly be produced by outside influence, as, for instance, the observer's body. Any heat so applied must first unequally warm the massive cast-iron box; it must then be imparted to the solid metal radio-micrometer (nearly 30 lbs.), which is only supported by five points forming a geometrical slide, and then it will only be the difference of temperature in the solid metal between points not much more than a quarter of an inch apart, which by imparting some portion of itself to the suspended circuit will cause any indication of heat or cold. If, however, radiation reaches the sensitive surface through the small hole drilled horizontally in the solid metal, and this can only come from the limited field of view of the telescope, then one portion of the circuit will be independently warmed, and a corresponding deflection will be produced.

The only part of the instrument which reaches outside the cast-iron box is a slender tube which carries the cork and pin from which the circuit is suspended by a very fine quartz fibre. This tube is made of glass ground into the metal of the radio-micrometer. The object of using glass is to prevent any loss of heat from the apparatus into the outer air. This part is also boxed in by an easily removable double box of wood. In making this tube I blew a hole in the side and thickened it with a welt of glass about half an inch above the level of the box lid. To the face of the welt previously ground flat is cemented a piece of a plano-convex spectacle lens, which forms an image of its own on the scale and at the same time brings the light reflected from the plane mirror behind it to a focus on the same scale. The mirror, which consists of a piece of the thinnest microscope cover-glass silvered at the back, produces with this arrangement an image so good that tenths of millimetre can be easily read. To prevent the delicate circuit from being influenced by draughts in the telescope, a tube containing diaphragms is fastened

to it and projects nearly as far as the side of the telescope tube. The diaphragms are of such a size as to limit the view from the sensitive disc to the cone of rays. A tube of this kind was used by Langley to protect his bolometer from the influence of draughts. It is nothing more than the old toy through which you can drop a pencil but not blow out a candle.

The arrangements of lamp and scale are hardly worth describing in detail. I will merely say that I used the ether light to avoid the necessity of having two kinds of compressed gases, which, in the country, would otherwise be necessary. The ether light is exceedingly convenient for this purpose, but I can hardly recommend it, for, though I used the safety burner supplied by the makers, the first thing it did, owing to my inadvertently turning off the oxygen, was to explode with a loud report, and the copper box, after striking the roof of the house, fell close to me, not burst, it is true, but blown into a more or less bulbous form. Moreover, on frosty nights, the ether box is so cold that the gas which comes out requires no more oxygen, so that an explosive gas is being led from the reservoir direct to the burner. Under these circumstances any stoppage of the oxygen supply would at once cause a violent explosion. As it is, it is impossible, on a cold night, to stop the gas without its exploding down to the tap which turns it off.

I provided, between the ether box and the burner, a pair of extra regulating taps, which, by a touch, will turn off the oxygen and turn down the (so-called) hydrogen, or turn them up again for an observation. By this means waste of ether and oxygen is avoided, and the limes last a long time.

I have arranged a slow motion in azimuth, which is more convenient when observing on or near the meridian, but none in altitude, as that would have been troublesome to make. The motion in altitude is rather stiff, but being used to it now I can follow a star in any part of the sky, step by step, without difficulty.

As I have already said, the tube was made by Messrs. Davy, Paxman, and Co., of Colchester; the stand and heavy fittings were made by Messrs. Thomas Horn and Sons, of Gray Street, Waterloo Road, S.E., engineers, and with regard to this part of the work, I must express my great satisfaction at the way the work has been carried out. Nothing is done for show, but every working surface is true, and works freely without shake. I believe this is the first thing of the kind that Mr. Horn has made; if he had done no other class of work but this he could not have done it better. The radio-micrometer and all the odd fittings and adjustments I made myself, and these parts have given no trouble. Into the details of the mounting of the mirror and certain minor adjustments it is not worth while to enter at length. It is sufficient to say that every part is capable of

independent adjustment, so that ultimately the focus is in the vertical axis of rotation, and at the same time the cone of rays from the large mirror is just not sufficient to cover the surface of the flat.

Though there is a finder, it would hardly be safe to trust to this to know when a star had just come on to the sensitive surface, and so I have arranged a 1-inch total reflecting prism behind the metal block under the magnet, which can be turned round so as to view the sensitive surface, and any image in the small space round it, from either side of the box. There is an oblong hole in each side for this purpose, through which a low power eyepiece, carried by a bracket on the metal block, projects; the space round the eyepiece is covered in by a separate shield, to prevent hot or cold air from entering the box. The dark radiations from the pupil of the eye are entirely prevented, by the three glasses, from reaching the sensitive surface, so that it is possible to watch the image of any heavenly body quietly transit across the disc or sensitive surface without disturbing the indications by the heat of the eye. I have arranged a temporary small telescope with a diagonal eyepiece, immediately above that of the chief telescope. The small telescope shows that part of the scale to which the spot of light is brought, magnified, so that without moving the body or any part of the apparatus it is possible to watch a star come on to the disc, and to see the effect on the scale, and thus to avoid every source of error at once. If in any case a star is observed to transit over the disc time after time, and the index is not moved through one-quarter of a millimetre (and I find on a perfectly clear and quiet night there can be no doubt whether this is so or not,—I should even have little doubt of a tenth of a millimetre), then it is certain that the heat received was not sufficient to produce such a deflection. An equatorial star takes about 20 seconds to cross the disc, while practically the whole deflection due to any source of heat is produced in 5 seconds, and so, no matter how long the star might be kept on there would be no gain, while, on the other hand, the longer that it is necessary to leave the star on before practically the whole deflection is produced, the greater is the uncertainty of the zero of the instrument. The advantage of the short time constant, if I may use this expression, is fully proportional to its smallness, if it is not proportional to some higher power of its smallness.

I determined not to put up the apparatus in the doubtful atmosphere of London, and I am fortunate in having been able to fix it in my father's garden at Wing, in Rutland. The position is certainly good, the altitude is about 400 feet, the climate is as dry as in any part of England. The subsoil is oolitic limestone, containing a large quantity of iron, and very firm (the foundations of many of the old walls in the village are from 1 to 2 feet above the present level of the ground, and are perfectly secure). There is not a house or building

within 100 yards, and these are screened off by trees. The only objection is the rather long railway journey, which prevents isolated observations at odd times, and makes special observations of temporary phenomena very inconvenient.

The column is bolted down to a mass of about 2 tons of concrete, bedded upon the rock. The protecting house is made of wood and galvanised iron, and rests by four grooved wheels on rails made of gas-pipe, so that it can, when its own holding-down bolt is unscrewed, be pushed away so as to leave the telescope clear. The door lifts off and rests against two posts in one of the borders near. The figure will be sufficient to make every part, except minor details, perfectly clear. I think it will be best to describe the observations in the order in which they were made.

I began observations on the 6th September, 1888. The night was clear, but there was a gentle wind from the S.W., which produced an uncertainty in the position of the zero of a few millimetres. To keep off the wind I pulled the house over the telescope and looked east. Capella and Regulus gave no indication (11 20 P.M.), certainly not $\frac{1}{2}$ mm. There were several good negative observations. An earwig then began to climb up the delicate circuit of the radio-micrometer, and as it was windy I left, after first removing the circuit.

September 7th. To keep earwigs, of which there were an enormous number this year, and spiders from coming into the radio-micrometer, I placed in the diaphragm tube some cotton-wool which had been soaked in creosote. Thunderstorm in the day; at night wind north and cold. Dew on the telescope. Observed many stars up to 3 A.M., including Altair, Arcturus, α, β, and θ Orionis, and Capella. No deflection of as much as 1 mm. Wind prevented greater accuracy. About 3 A.M. some fleecy clouds passing produced strong effects of heat long before the star showed any diminution of brightness to the eye. A few leaves on the top of a distant tree produced an effect of about 60 mm. of heat.

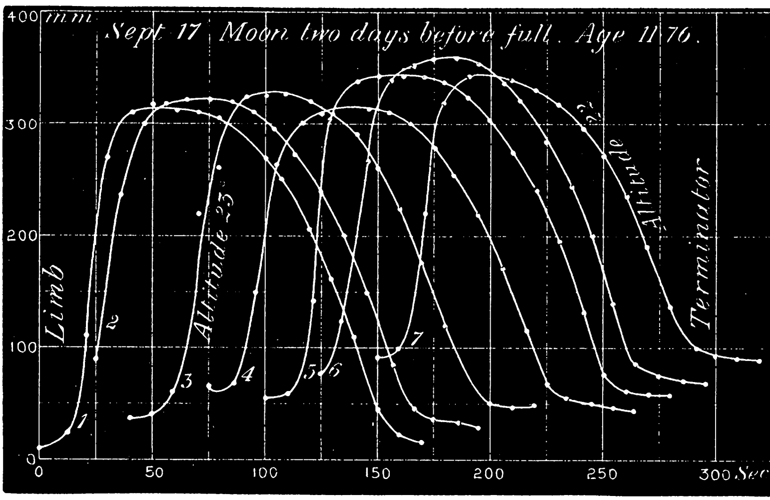
September 11th. A new moon, just above the horizon (about 4°), produced, the instant the image of the limb met the disc, a rapid movement of about 30 mm., which gradually declined to about half when the terminator was reached, after which the deflection at once fell to nothing. There was no indication of heat from the dark part of the moon. The night being good, I was out till dawn, and tried all the bright stars in Pegasus, Andromeda, and Orion, as well as Aldebaran, Castor, Capella, and Saturn. No result from any of them, certainly not $\frac{1}{2}$ mm.

September 12th. At 5 30 P.M. the moon, first quarter, was low down in the south. Observations at 5 50 and at 5 54 in the daylight showed deflections at the limb of 125 and 120 mm., which, as before, became less towards the terminator, where they vanished.

September 13th. No effect from Jupiter, but he was badly placed near the S.W. horizon, and it was too windy for a satisfactory negative observation. Several observations of the moon showed the greatest heat to be close to the limb; the deflection for this part ranged from 175 to 200 mm.

September 17th. Night clear and quiet; heavy dew. I now determined more accurately the variation of heat from point to point across the moon by arranging that it should transit centrally (or near a pole if desired) over the disc and taking readings every ten seconds. The first five curves in fig. 3 are the results of five consecutive transits

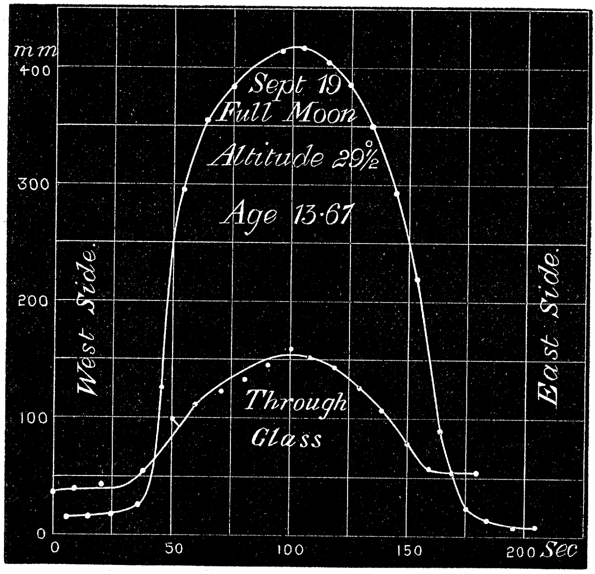
FIG. 3.



over the central part of the moon. The sixth curve was taken over the south end of the moon and the seventh curve over the north end. They were all taken when the moon was not far from the meridian. These are the first observations which clearly indicated a maximum of heat within the disc and not on the limb. This is evidently about the position at which the Sun is vertical, being approximately at right angles to the terminator. To save space and confusion, these curves are all drawn superposed but separated in time to a small extent.

September 19th. Full moon. The first curve, fig. 4, was taken when the heat of the moon was largely absorbed by a piece of clean window glass fixed across the mouth of the radio-micrometer. The second curve was taken immediately afterwards (9 5 P.M.) without glass. The heat transmitted by the glass, almost exactly 25 per cent., is somewhat different from the proportion, 17·3 per cent., which

FIG. 4.



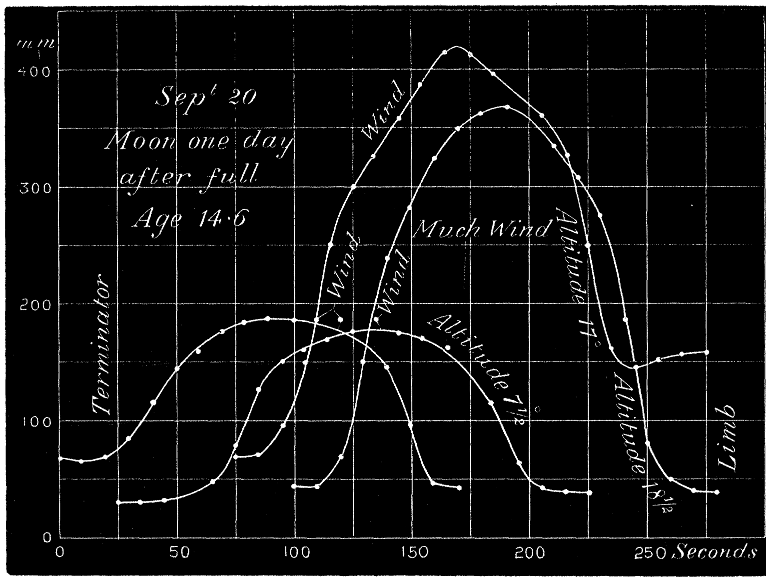
Lord Rosse obtained for the full moon, using the 3-foot telescope and a very superior thermo-junction of his own design.* The remarkable symmetry of the curve for the full moon at first rather puzzled me, as I expected that the side that had been baked by the sun for from 7 to 14 days would be hotter than the side that had only been lighted up for from 0 to 7 days. However, if we consider that soil of any kind is so bad a conductor that it would acquire its final surface temperature in perhaps an hour or less, that is, if not protected by an atmospheric blanket, the symmetry of temperature is nothing more than should be expected. Lord Rosse's experiments† on the heat of the eclipsed moon fully bear this out.

September 20th. Observed the moon on the horizon, deep red, limb ill defined, and cloud bands across, looking like the belts of Jupiter. A series of readings were taken, but not at exactly ten-second intervals. The deflection was greatest about the middle, where it reached 15 mm. Four minutes later (7 2 P.M.) the deflection in the middle amounted to 40 mm. The four curves, fig. 5, were taken later, as indicated by the increasing altitude. Though the later ones especially were much disturbed by wind, they well show the diminishing absorption by the atmosphere as the moon rose in the sky. Observations later

* 'Phil. Trans.,' vol. 163 (1873), p. 615.

† 'Roy. Dublin Soc. Trans.,' 1885, p. 321.

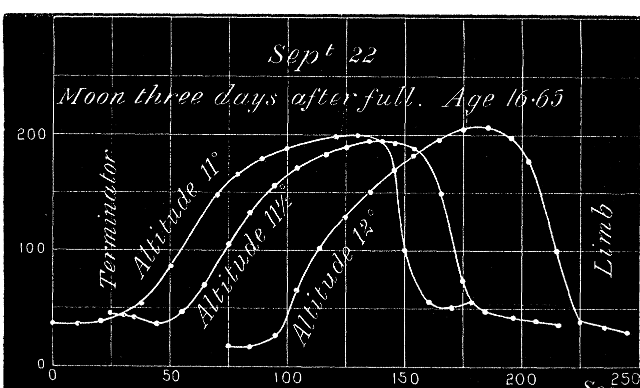
Fig. 5.



were prevented by light fleecy clouds which, as they flitted across the moon, so disturbed the deflections in the direction of cold (not as heat as when they pass over the sky or over stars) that the curves obtained later were a mass of teeth, like a comb or a saw, and ceased to have any value.

September 22nd. This was at first a perfect night, not a breath of

Fig. 6.



wind, but what there was was east. There was much dew, and by 9 P.M. the air had become filled with mist. Three observations on the moon (three days past full) were taken, but owing to the low altitude the actual amount of deflection was small. The regularity of the curves, which were the only ones taken that night, is an indication of the perfect quiet of the night. I examined also Vega, Altair, and α Cygni; there was certainly not a deflection of $\frac{1}{4}$ mm. The index often did not move $\frac{1}{10}$ mm. for several seconds. As this was a perfect night and the dark side of the moon was the advancing side, I made special observations to see if any heat could be obtained from the dark part on which the Sun had just ceased to shine, but not the slightest effect was observed until the terminator itself met the disc of the radio-micrometer.

I had at this time to go to London, and so no more observations were made until the early morning of October 31st, when I had returned to observe the moon a few days before conjunction. The night was windy after a wet day. At 3 30 A.M. the deflection on the limb was 25 mm. where it was greatest. As usual, it gradually decreased up to the terminator. At 6 30 A.M., when the moon was higher and better placed for a delicate test, I tried to observe the heat due to the earth-shine, which was then very marked. Owing to the wind I was unable to make a satisfactory negative observation, but it was certainly not 2 mm., and I believe not 1 mm. I have no record of the deflection on the bright part.

December 26th (5 30 P.M.). Venus very bright, but not at her best. An equal area in the moon would produce an effect easily observable, whereas a part of the moon large enough to produce the same light would still more produce a measurable deflection. There was not a deflection of $\frac{1}{2}$ mm. I had, however, another circuit in the instrument at this time which did not seem very sensitive.

April 19th, 1889 (7 30 P.M.). Made many observations on Venus, which had passed the position of greatest brilliancy. Sky clear, but the night was windy, which kept the index moving over a space of from 10 to 40 mm. Out of many transits observed, only two were not interrupted by a violent gust of wind. In each of these cases there was a deflection of from 2 to 3 mm. in the direction of heat, which may have been real, but I do not place any reliance upon it.

I was not satisfied with the delicacy of the apparatus and so made a new circuit, and suspended it by a fibre 1/5750 inch in diameter. With an exposed surface of about 4 square mm., illuminated by a candle flame at a distance of 60 inches, this gave a deflection of 60 mm.

As Mercury was unusually well placed in May, I took this circuit to Wing, hoping to obtain effects from the most promising of planets. Clouds, however, prevented Mercury from once being seen. Later on

I was able to observe Arcturus on the meridian in a clear sky, but I obtained no effect.

On March 25th, 1890, I tested the delicacy of this circuit with a 4-inch Leslie cube (black face) filled with boiling water at a distance of $81\frac{3}{4}$ inches. The deflection due to the exposed area of about 4 square mm. was 50 mm. A new circuit, made of better materials, gave a deflection of 180 mm. under the same conditions, and as this seemed a good circuit, I used it last Easter with freshly silvered mirrors.

April 2nd, 1890. A perfect night, except a slight but persistent east wind. The moon, not yet full, sent the index right off the scale at once. It was too windy to make satisfactory observations on stars.

April 3rd. Perfect night. Wind very slight, N.N.E. Very dry. Telescope tube and ether box covered with frost. Ether box so cold that no separate oxygen was necessary. I balanced a paraffin candle on a hedge at a distance of 93 yards, and found on moving the telescope on and off with the slow motion screw a very constant deflection of 55 mm. In this case the candle was so much out of focus that only about one-sixth of the rays fell on the disc, all the rest passing by. I could not alter the focus sufficiently, nor could I put the candle further away that night, but this was done the following day. About 1 30 A.M. Arcturus was on the meridian. There was not a deflection of $\frac{1}{4}$ mm. in any of a number of very satisfactory observations. Mars, low down in S.E., and Regulus and Saturn produced no effect either.

On Good Friday, April 4th, I prepared a clear view up to a mound 250 yards away, where a mill once stood, by cutting off the tops of the hedges that were in the line. On this mound I placed a stand carrying a beehive with a hole in the side. A candle placed in the beehive would send its rays into the distant telescope without any intermediate obstruction. A light paper shutter inside the beehive was arranged so that it could be pulled to one side by a long piece of cotton, and be allowed to fall back again so as to obstruct the radiation. The exact distance from the candle to the mirror (obtained by chaining) was 250·7 yards. During the day I moved the radio-micrometer on its geometrical slide until the disc and the image of the beehive were in the same plane. The image of the candle-flame, which was about $\frac{1}{4}$ mm. long, was then focussed upon the disc, and all the rays which entered the telescope from any part of the flame were received upon the disc.

About 8 30 P.M., when it was fairly quiet, there being a slight east wind, my niece, L. Wintle, kindly stationed herself on the mound out of sight of the telescope, and at a signal either pulled or let go the cotton. The telescope was pointed towards the candle the whole

time. It was not moved, nor was anything moved, except the shutter in the beehive. At each movement of the shutter the image of the light started off violently, and came to rest in about five seconds, showing a deflection of 38 mm. This was repeated about twenty times. This is an absolutely trustworthy measure of the sensibility of the whole apparatus, including imperfect reflection by the silver and every factor, except the absorption by the air. This, as the night was dry and clear, must have been very small. It shows if $\frac{1}{4}$ mm. is taken as the smallest deflection which can be observed with certainty, that a candle-flame at 250·7 yards produces an effect which is 152 times as great as the least which can be certainly observed; or that, if the candle-flame had been taken $\sqrt{152}$ or 12·3 times as far away, that is, 3084 yards, or 1·71 miles, it would have sent into the telescope sufficient heat to be observed, and therefore more than Arcturus.

After these measures were made, the stand, &c., were removed, and my niece stood with her face exactly in the line of the telescope. The image of her face must have been entirely within the disc. When she moved on one side the clear sky was in view instead. The deflection, observed many times, was 48 mm. This large effect was no doubt chiefly due to the screening of the sky, but I mention the experiment to show that the mirrors reflect dark heat satisfactorily.

Later on in the evening, at about 10 P.M., I turned the telescope on to the moon, now about full. The index was, of course, driven right off the scale, but with a 6-inch stop over the mouth, the deflection, due to the centre of the moon's disc, was 150 mm. With a 12-inch stop the index again went off the scale. Now the area of the mirror, which is 15·625 inches in diameter, is 192 square inches, the area of the 6-inch stop is 28·25 square inches, and the area obstructed by the flat is 4·67 square inches. The effective area of the mirror is therefore 187·3 square inches, and of the 6-inch aperture 23·58 square inches, and the ratio of these numbers, 7·95, is the number by which the deflection with the 6-inch stop must be multiplied in order to obtain the deflection that would be obtained from the whole mirror if all could still go in proportion. The centre of the full moon, then, would give, on this supposition, a deflection of 1192·5 mm., or about 4800 times as great an effect as could be observed with certainty. Since the disc is in the particular instance about one-sixth of the diameter of the image of the moon, it is clear that it covers about 1/36 or receives, in round numbers, 1/30 of the total heat, if it is at the centre. It is evident, then, that an amount of heat equal to about 1/150,000 of that sent by the full moon would produce an effect which could be observed with certainty, and Arcturus does not send this.

Dr. Huggins did not obtain any effect from the moon, but, as he used an object glass which would absorb nearly all the moon's heat,

or, at any rate, a large proportion, and as it had a smaller aperture and a greater focal length than the mirror which he lent me, so that the image of the moon was larger, and on this account also had a smaller heating power, it is not surprising that he did not obtain a large effect from the moon; but that he obtained none is, in the face of these measures, strong evidence that the deflections observed, and which he attributed to the stars, were spurious.

Mr. Stone did not observe the moon at all, but he concluded from his observations that Arcturus was in heating power equivalent to a 3-inch cube of boiling water at 400 yards. A direct comparison of the candle that I used with a 4-inch cube of boiling water gave as the ratio of the heating powers on a radio-micrometer, of which the temperature was 15° C., a figure varying slightly with the state of the flame, but generally slightly over one half. Had the comparison been made with the radio-micrometer at 0° C., as it was when the observations on Arcturus were made, a slightly lower number would, of course, have been obtained. As the face of a 3-inch cube is slightly over one-half the face of a 4-inch cube, the candle and the 3-inch cube may be taken for the purpose of comparison as sensibly equal. Now, the direct and conclusive experiment already described has shown that Arcturus on the meridian is not equal to a candle, and therefore to a 3-inch cube at 3084 yards, so that Arcturus must have a heating power nearly sixty times less than that given by Mr. Stone. I cannot clearly follow the whole of Mr. Stone's reasoning, more especially that part which relates to the rise of temperature due to the star. By the use of thermometers in contact with the face of his pairs when subject to a much stronger radiation, Mr. Stone determined the amount by which the face must be raised in temperature to produce a deflection of one division, and he concluded from the deflection observed that the face was raised through 1/90° C. Now, ou comparing his arrangement with mine, it is evident that, as I have a larger aperture, I have more heat to begin with; as I have no glass for the rays to pass through, I am free from the large absorption of glass for heat; and, finally, as my thermo-electric bars have a sectional area about one-twentieth of that of the bars used by Mr. Stone, the heat for a given difference of temperature would have been conducted away at about one-twentieth of the rate; or, as this is the chief cause of the cooling of the hot junction, for a given rate of radiation my junction would have been heated to nearly twenty times the amount to which Mr. Stone's would be heated. Taking all these things into consideration—if Mr. Stone's figures are correct—my junction should have been warmed through about one-third or one-fourth of a degree Centigrade. Now, I have already shown*

* Cantor Lectures, 1889.

that the radio-micrometer will respond to a temperature rise of less than one-millionth of a degree. Even if my figure of one-millionth is not absolutely correct—and I believe that the figure should be still smaller—the discrepancy is so enormous as to require explanation. Knowing how completely every thermopile that I have examined fails to approach in delicacy the radio-micrometer, I cannot quite understand how Mr. Stone was able to observe deflections which correspond to a rate of radiation which is only a few times as great as the least that I can observe. As an instance of the delicacy of the radio-micrometer, I may state that, compared with the usual form of thermopile, made by Messrs. Elliot Brothers, connected with a low resistance reflecting galvanometer by the same makers, it is fully one thousand times as sensitive, besides being far more stable or insensitive to disturbing causes.

I think my observations go to show that the heat of Arcturus has not as yet been observed (unless the refracting telescope possesses some mysterious power which the reflector does not); and the same conclusion may be drawn with respect to the other stars. I have by no means reached what I believe to be a practical limit to the delicacy of the radio-micrometer, and it is possible that with a more delicate instrument, or with a larger telescope—and Mr. Common has promised to allow me to use for the purpose his 5-foot reflector—I may yet be able to observe some definite and real effect; but that I should have so far failed is not surprising, in view of the following argument. When anything is heated and gives out light an increment of temperature causes a relatively greater increment of light; so that, light for light, less heat is radiated by the hotter body. Thus, among stars that are equally bright, the coolest will send the most heat. Now, a candle-flame is, in all probability, far cooler than any white or yellow star; and so if a candle-flame is removed until it appears of the same brightness as some star—for instance, Arcturus—then it should radiate far more heat. Now, I do not know at what distance a candle would appear as bright as Arcturus, but supposing that it is as much as 1·71 miles, then, since at this distance I can only just detect the heat of the candle, it is evident that I should not be able to detect the heat of Arcturus.

With regard to the observations on the moon, they are, I know, exceedingly fragmentary and incomplete. They were not, however, undertaken for their own sake, though they may have some value, but in order to test the apparatus and to serve as a standard by which other observations might be compared with mine. For work on the moon the apparatus is not exactly the most convenient, though it does well. For systematic work I should employ the most delicate circuit I could make with a very small sensitive surface and a quick period, and then, to obtain curves of heat which should give the deflections

from moment to moment and not only at ten seconds intervals, I should record the movement of the index photographically. It would also be an advantage to have the telescope under cover, owing to the annoyance caused by wind. I did not adopt this plan, as I did not wish the apparatus to be influenced by any unnecessary source of heat. Now that I have found how insensitive it is to everything in the nature of radiation, except that which is focussed by the telescope upon the disc, I know that this precaution was unnecessary as well as inconvenient. The apparatus, as designed, is not suitable for spectroscopic examination of the moon's heat, and so I have not attempted to repeat any of Langley's observations. I may state that, though neither the properties of the circuit employed nor the ten seconds readings were suitable for detecting local variations of the moon's heat, due to bright or dark spots, yet I did expect to find indications of local differences. None, however, were apparent, but I can hardly imagine that with a much smaller disc, a more rapid period, and a photographic record local variations could not be determined.

As my observations on the moon's heat are so small in number and were made at such irregular intervals, I have not applied Lord Rosse's correction* for its absorption by the air.

The curves of the moon's heat require a slight correction for the lag of the apparatus, which I have not attempted to make and which it would be useless to make unless the curves were drawn by a more perfect method. The correction could be reduced to a great extent by causing the telescope or a siderostat to so move as not quite to keep pace with the earth's rotation, so that a greater time would be occupied by a transit; but it would be unsafe to prolong this time to a great extent, for, besides the greater uncertainty in the position of the zero, a slight change would be produced by the varying altitude of the part of the sky under observation, and this would change the atmospheric absorption. As the telescope is moved from the horizon to the zenith the index moves over the scale to an extent which is very variable, but in the direction of cold.

I should have stated that very thin clear mica absorbs a large proportion of the moon's heat. On the night that I tried this I was unable to measure the proportion, as while with the mica I obtained a deflection (not recorded in my notes) of roughly 200 mm., without the mica the index went off the scale and I had no stop to limit the radiation.

Anticipating trouble owing to the varying state of the sky, such as was found by Mr. Stone, I devised a form of radio-micrometer circuit which should be differential. Thus, calling the alloys used A and B,

* 'Phil. Trans.,' 1873, p. 598.

two A bars are soldered to the lower ends of the copper wire arch, two B bars are soldered to these and joined below. The A, B junctions are then exposed in the radio-micrometer side by side, and so are unaffected by general atmospheric influences, while as a star passes from one to the other the effect of the star will be reversed. On a quiet night the single junction works so well that I have not at present tried the differential form.

It is my intention to make observations from time to time when I am able, but these, owing to my duties in London, can only be made at uncertain intervals.

[*Note*.—At the time that this paper was sent in I did not realise that the larger figure that I obtained for the percentage transmission of the moon's heat through glass, viz., 25, instead of about 17, as found by Lord Rosse, far from being of the nature of a discrepancy, is in reality a difference which should be expected. Lord Rosse made observations on the total heat only; he did not localise parts of the moon, as I have done. He found a greater proportionate transmission at the full moon than at the quarters, viz., 17 at the full and 8 at the quarters. Now, taking elements in the moon, I cannot think that this proportion can to any appreciable extent depend on the altitude of the earth at the element, but it should depend on the altitude of the sun there, for upon this the heat received by the element depends. Now, in taking the full moon as a whole, there are elements with the sun at all altitudes from 0° to 90°; but those parts with the sun at the zenith are most favourably situated to produce their effect on the earth, and thus the observed effect, which is the result of all the parts, should show a percentage transmission between that due to a zenith sun and a sun near the horizon, which should also be the case with the moon at the quarters or between the quarters and the full; but there is this difference, as the moon approaches the quarters the effect of the parts on which the sun is shining vertically becomes proportionately less and less, and so while with a full moon the percentage transmission should approach that due to an element with a vertical sun, with a half moon the percentage should be more nearly that due to an element with a rising or setting sun.

Now my method of observation of the moon at once gives the means of finding the percentage transmission due to an element with the sun at any altitude. The curve, fig. 4, shows that the centre of the full moon does send a larger proportion through glass than either limb, and, as it should do, a greater proportion than the integral result found by Lord Rosse. A single transit of the full moon should, therefore, give data from which the integral result of the moon in any phase could be calculated, and corresponding observa-

tions upon the moon in other phases would show experimentally whether the percentage transmission by glass of the heat from an element of the moon does depend on the altitude of the sun only, or whether the altitude of the earth also has any influence, as, indeed, is suggested by Lord Rosse in a note to his Bakerian Lecture ('Phil. Trans.,' vol. 163, p. 626). It is unfortunate that I have not at present made any other observations on the transmission by glass of the moon's heat.—April 21, 1890.]

IV. "Observations on the Secretion of Bile in a case of Biliary Fistula." By A. W. Mayo Robson, F.R.C.S., Hon. Surgeon, Leeds General Infirmary, Lecturer on Practical Surgery at the Yorkshire College, and Examiner in the Victoria University. Communicated by Dr. Clifford Allbutt, F.R.S. Received April 3, 1890.*

There are few physiological questions on which so much doubt and disagreement prevail as on that of the secretion and uses of bile, this being especially marked when we come to compare the apparently contradictory observations of various experimenters relating to the action of drugs on the biliary secretion.

As the well known experiments of Dr. Rutherford and Messrs. Prévost and Binet were conducted on the lower animals, it may possibly account for the differences between their observations and those recorded in this paper. From the rarity of cases of biliary fistula in healthy human subjects, the opportunity has rarely occurred for a careful analysis of fresh bile in sufficient quantity, or for a complete analysis of the *whole* twenty-four hours' secretion; and in all previous analyses no notice has been taken of the gall-bladder secretion.

In the following cases, the fistulæ remained open for long periods after the initial operations, the total flow of bile or gall-bladder secretion was carefully collected and accurately measured at different times and for many consecutive hours at a time, and the general good health of the patients was maintained throughout.

Method of Collecting.

The fluid was caught in a light glass flask, into the mouth of which it was guided by means of a celluloid cannula, a substance chosen after several trials with metal ones, on account of its lightness and non-irritating qualities.

* This paper is a revision of that read on January 16, under the title "Observations regarding the Secretion and Uses of Bile" (see p. 129, *supra*).

Fig. 1.

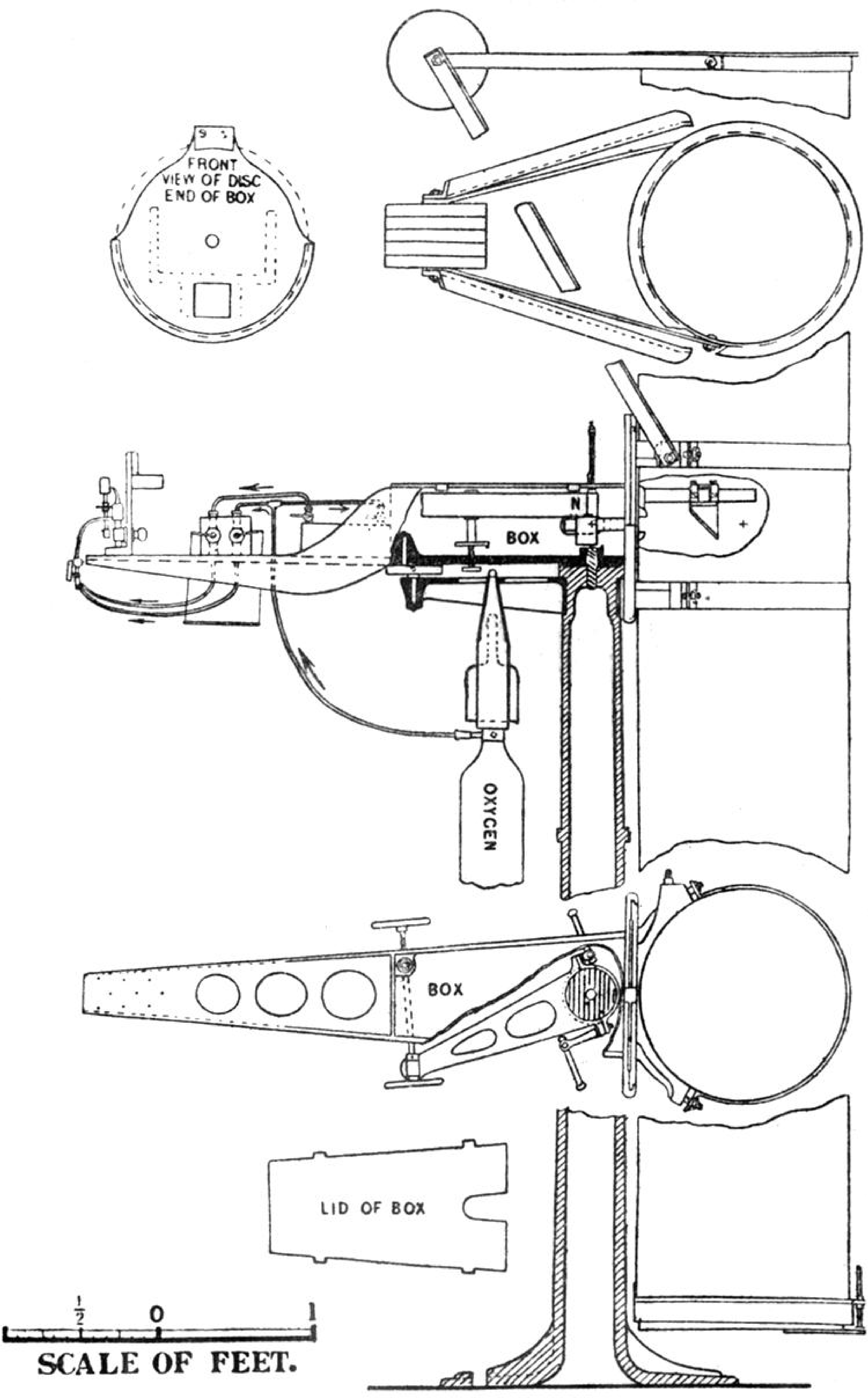

FRONT
VIEW OF DISC
END OF BOX
N
BOX
OXYGEN
BOX
LID OF BOX
½
0
1
SCALE OF FEET.

Fig. 2.

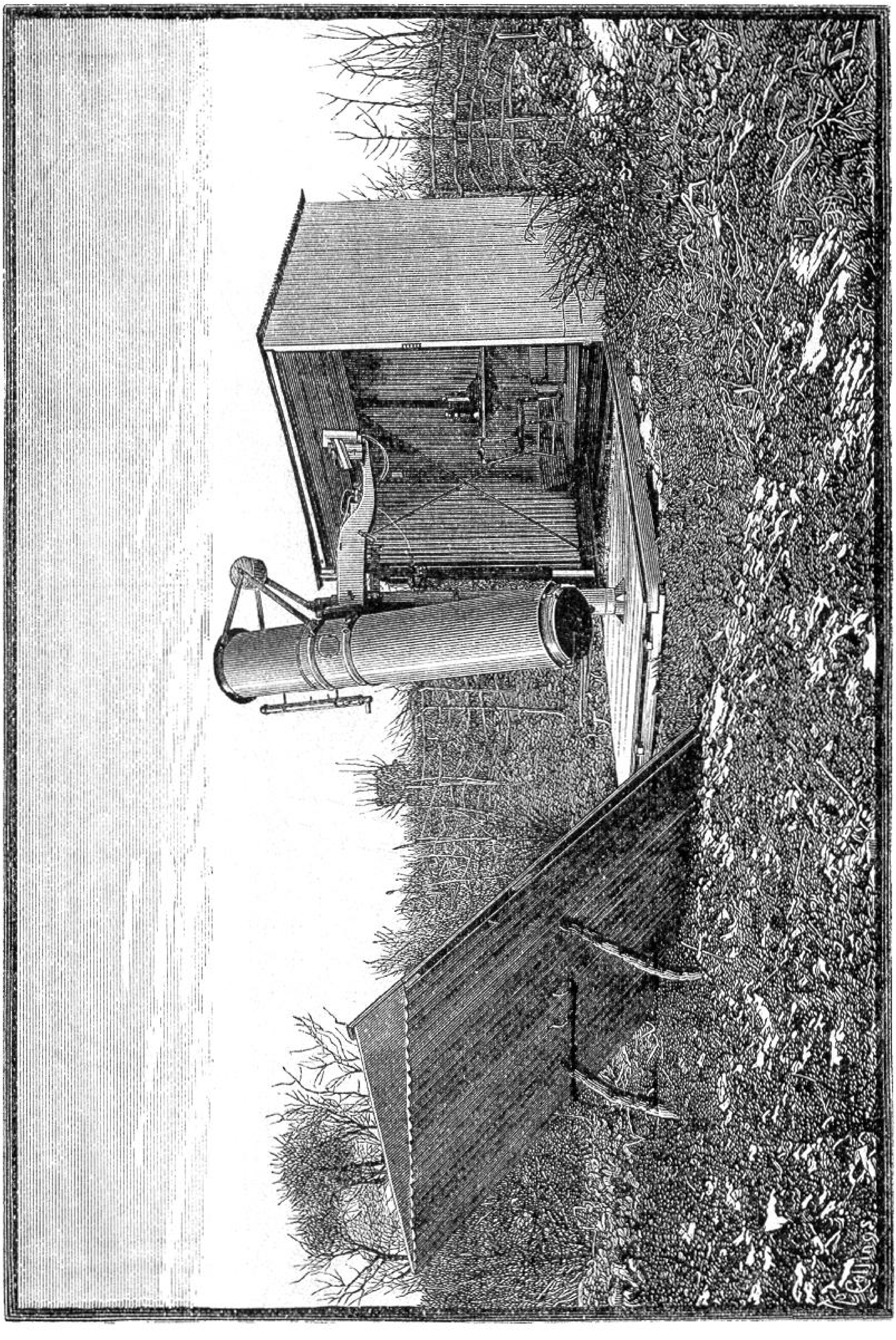


FIG. 3.

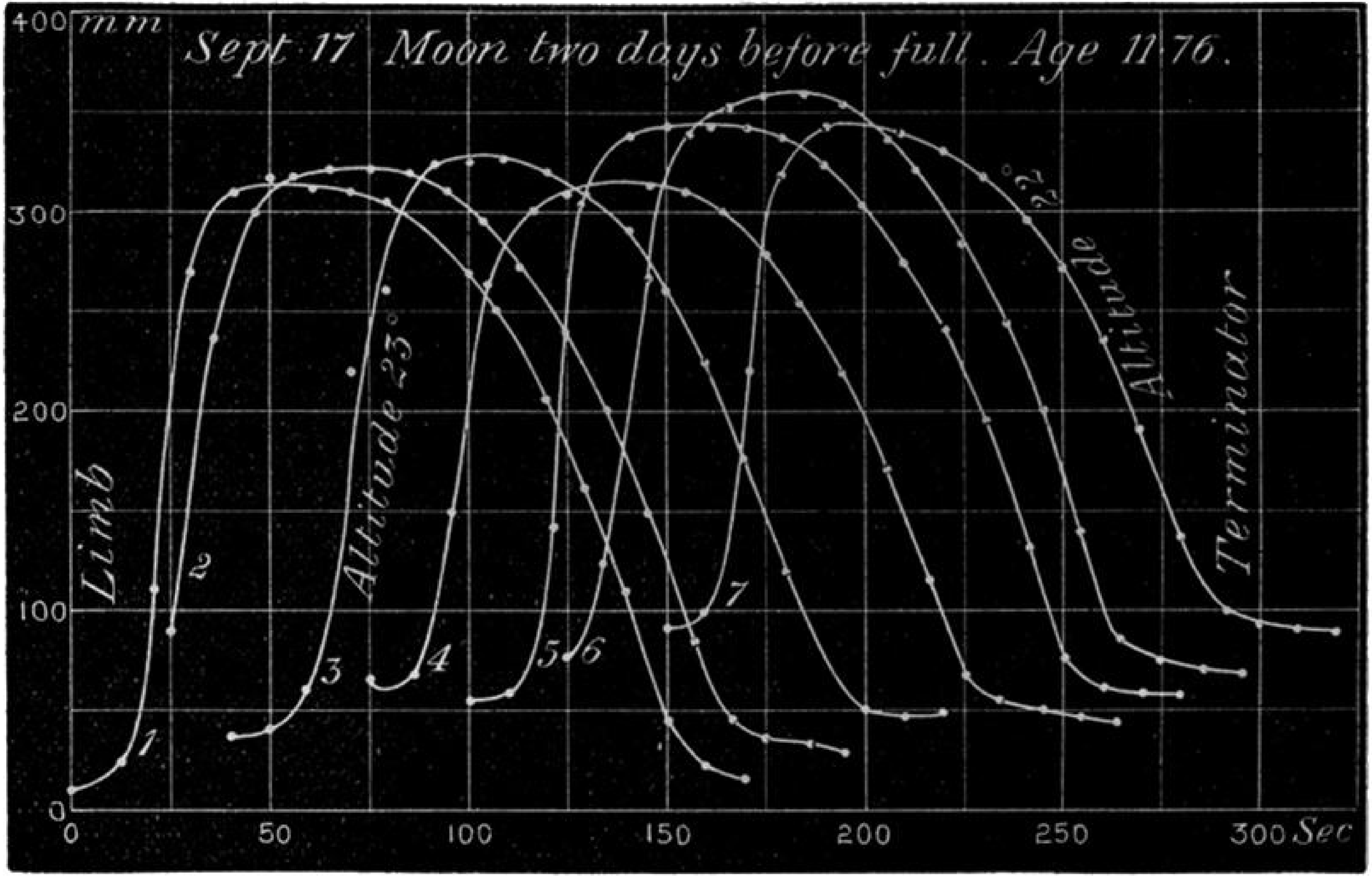

Sept 17 Moon two days before full. Age 11·76.
400 mm
300
200
100
0
0
50
100
150
200
250
300 Sec
Limb
Altitude 23°
Altitude 22°
Terminator
1
2
3
4
5
6
7

FIG. 4.

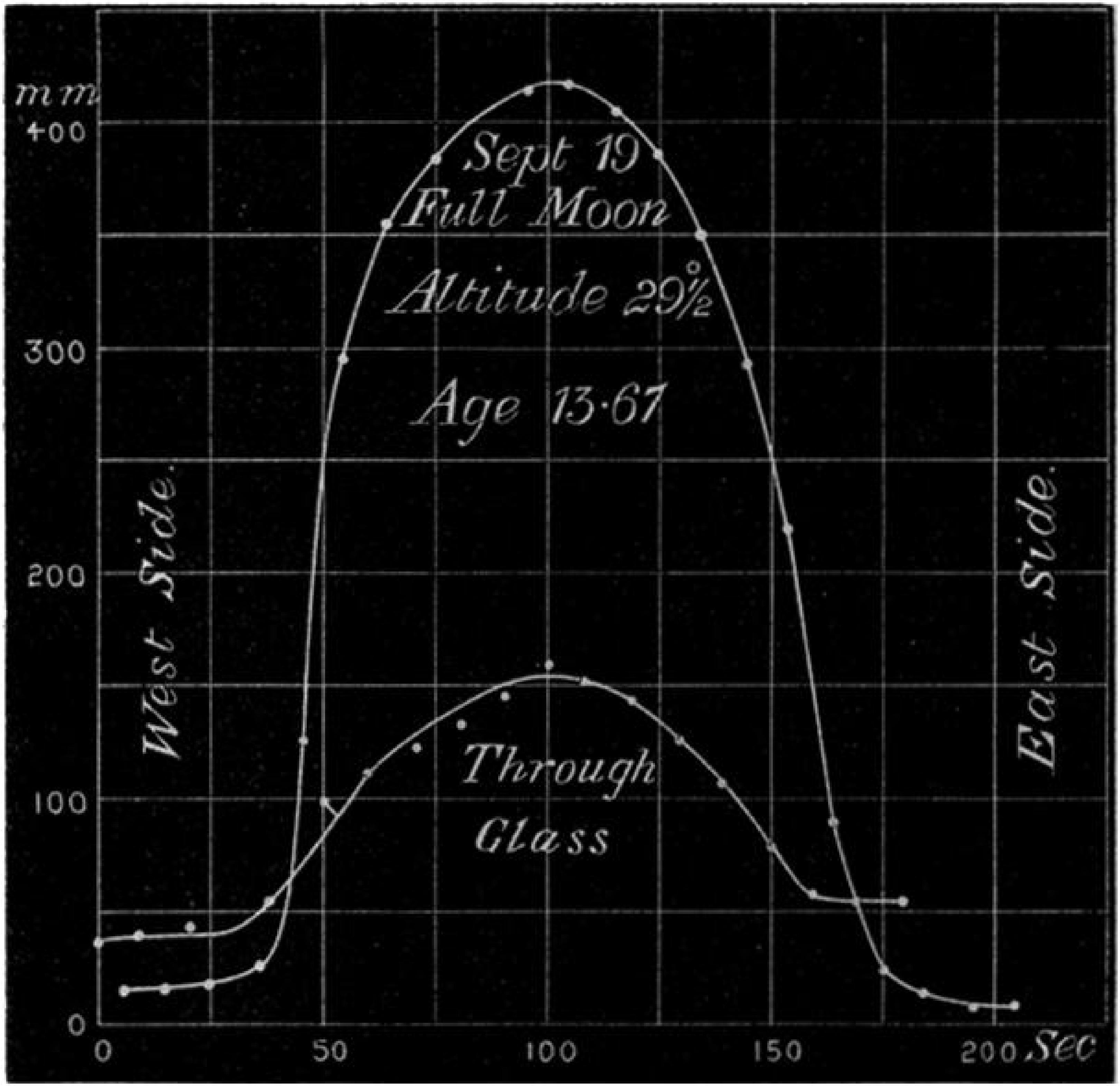
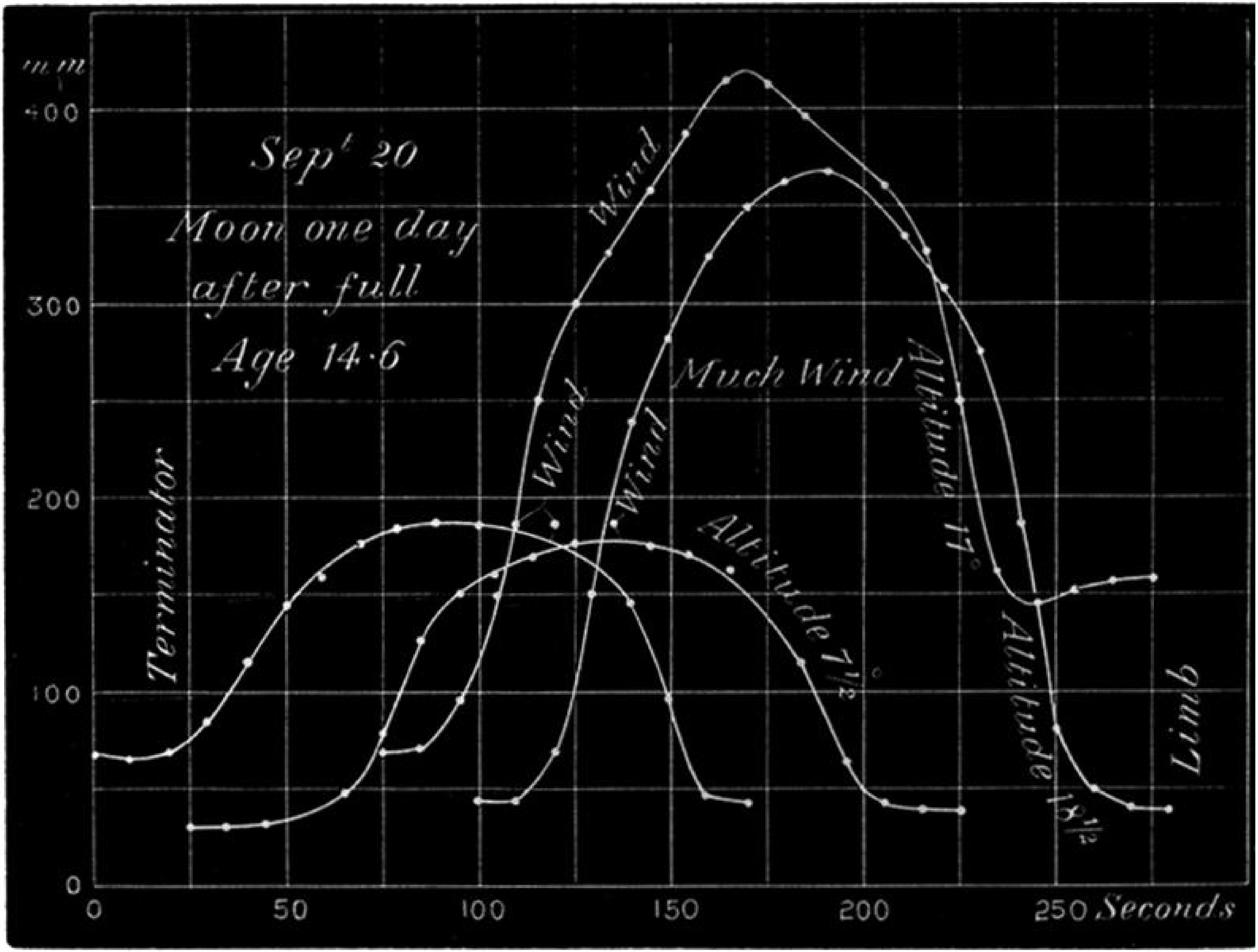
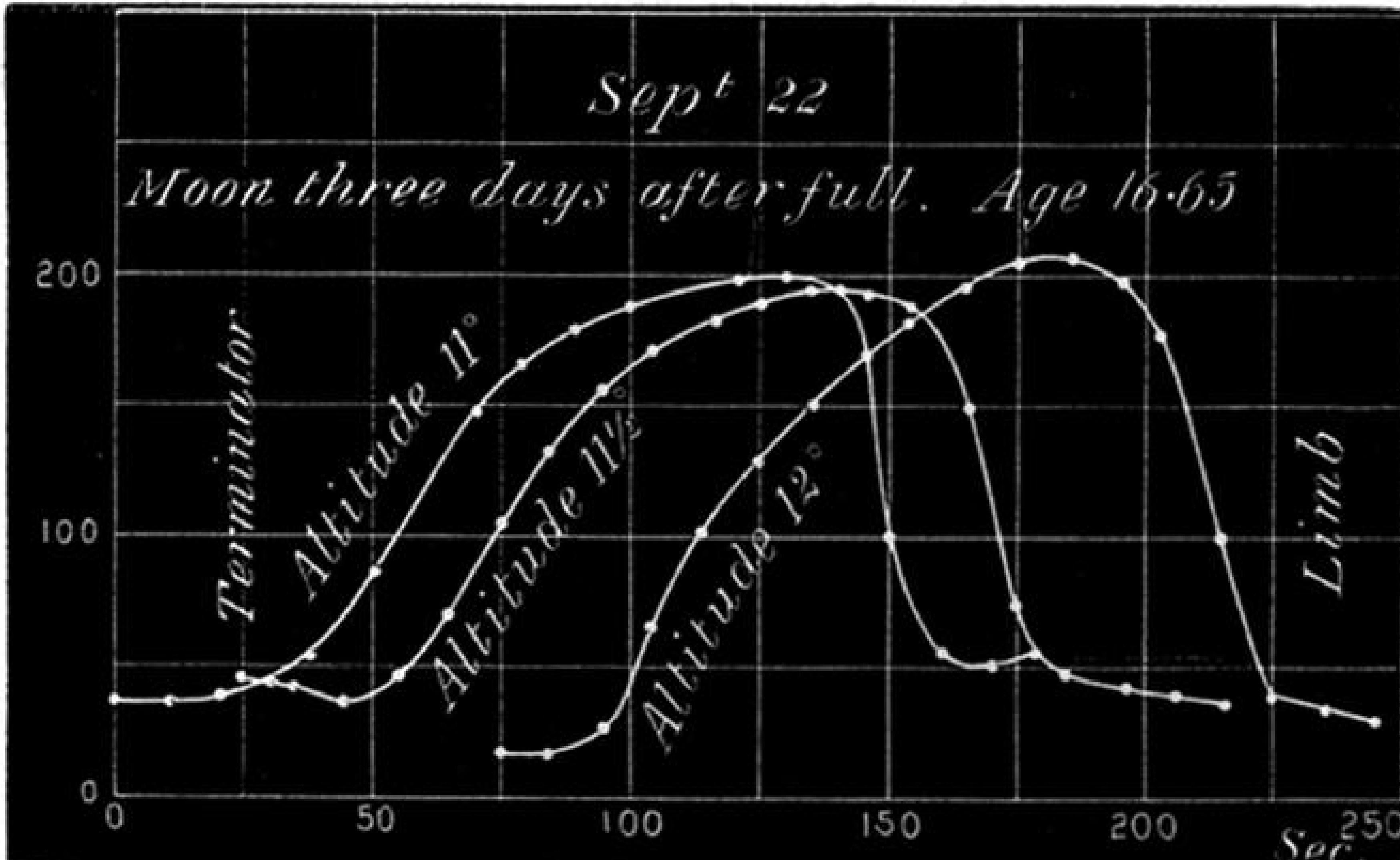

mm
400
300
200
100
0
0
50
100
150
200
Sec
Sept 19
Full Moon
Altitude 29½°
Age 13·67
West Side.
East Side.
Through
Glass

FIG. 5.



m.m
400
300
200
100
0
Sept 20
Moon one day
after full
Age 14·6
Wind
Wind
Wind
Much Wind
Terminator
Altitude 7½°
Altitude 17°
Altitude 18½
Limb
0
50
100
150
200
250 Seconds

FIG. 6.



Sep^t 22
Moon three days after full. Age 16·65
200
100
0
0
50
100
150
200
250
Sec.
Terminator
Altitude 11°
Altitude 11½°
Altitude 12°
Limb