THE DEVELOPMENT OF A MORE PRECISE COSMIC RAY SOUNDING BALLOON ELECTROSCOPE AND SOME PRELIMINARY RESULTS

Thesis by

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I. Relation of Sounding Balloon Observations to the Cosmic Ray Problem

Cosmic Rays are penetrating radiations from outside the earth's atmosphere. They were discovered by their ability to discharge an electroscope more rapidly at high altitudes than at the earth's surface. Since their discovery they have been extensively studied to determine their properties. Three types of apparatus have been found useful in these studies: the ionization chamber which measures total intensity from all directions; Geiger-Mueller coincidence counters which give a measure of directional intensities; and Wilson Cloud Chambers, which enable one to study individual rays.

One of the chief functions of the ionization chamber has been and still is the determination of the intensity of cosmic rays at all depths below the top of the earth's atmosphere at every point on the earth's surface. The variation in depth is important for a study of the absorption of the radiation in the earth's atmosphere, while the investigation of the geographical distribution is important in order to determine the effect of perturbing factors such as the earth's magnetic field.

The means by which these studies are made vary both with geographical location and with altitude. Since a large proportion of the earth's surface is covered with water, the level of which is at a practically uniform pressure depth of 30 in. of mercury below the top of the atmosphere, it is natural that observations at a pressure of 30 in. of mercury should have been made by boat. The result was the discovery by Clay[1)], 1927-30, that the sea level intensity of cosmic rays at the equator is less than in the higher latitudes. This Latitude Effect was later confirmed by Compton[2], Hoerlin[3], Millikan and Neher[4], Prins[5] and others, thus showing that there are some charged particles present among the cosmic rays and that the magnetic field of the earth certainly does deflect them appreciably. Later work of Clay[6] and Millikan and Neher[4], resulted in independent discovery by each that the intensity followed the asymmetries of the earth's magnetic field quite closely, giving rise to a so-called Longitude Effect. Further as yet unpublished results of Millikan and Neher have completed a world wide survey of great accuracy which leaves very little doubt as to the intensity distribution of cosmic rays at sea-level.

Observations below sea level have been made by Kolhorster[7], Regener[8], Millikan and Cameron[9] and Clay[10] by means of sinking electroscopes in water or taking them into mines. The results so far have served mainly to show that some of the cosmic rays are extremely penetrating, since there are still some left at 250 meters of water below sea level.

For observations at high altitudes the technique becomes somewhat more complicated. There are at present four possibilities for the elevation of instruments. They are: 1) Mountains; 2) Aeroplanes and Airships; 3) Manned Balloons; and 4) Unmanned or Sounding Balloons. Each of these present certain difficulties. Mountains are limited by their geographical locations, their relatively low altitudes, their inaccessibility, and the variation of their local radiation. Nevertheless almost all observers have done some mountain work and much valuable knowledge has been gained from it. Aeroplanes offer higher altitudes and great freedom of geographical location but have the disadvantages of tilting, and vibration as well as such severe conditions as to tend to make observers somewhat unreliable. The problem has been completely solved by the Neher[11)] vibration and tilt free electroscope with automatic recording. Flights of from 20-30,000 ft. have been made from Comorant Lake, Canada; Spokane, Washington; March Field, California; Panama Canal Zone; Lima, Peru; and in the Philippine Islands.[11),12),13)] Successful flights were also made in the Northern U. S. by Mott-Smith and Howell.[14)] That the latitude effect increased rapidly with increasing altitude was discovered by Compton.[2)] This accurate aeroplane survey checked this discovery, extended it to much higher altitudes and showed that at 3.5 equivalent meters of water below the top of the atmosphere the intensity on the equator was only 40% of that at the same level in the northern United States. This aroused great curiosity as to the extent of the Latitude Effect at the top of the atmosphere.

For the measurement of the intensity of cosmic rays at very high altitudes only manned balloons with enclosed gondolas and sounding balloons with automatic instruments are available. Both methods have been used with considerable success[12),15),16),17),18)] in high latitudes so that our knowledge up to heights corresponding to 0.7 equivalent meters of water in these regions is good though not yet complete enough. The completely unknown quantity is the intensity at these altitudes in equatorial regions. It was this problem which Dr. Millikan and his co-workers in this laboratory set about solving in 1933. Since high altitude manned balloon flights are very expensive and extremely difficult to make in any but the most thickly settled and highly organized regions, it was felt that sounding balloons offered the only chance of success.

II. The Source of Potential Problem

General Considerations

It was almost half a century ago that sounding balloon flights were first made in order to obtain meteorological data. Since then they have been very extensively employed for this purpose. Their use for determining the intensity of cosmic rays at very high altitudes has only been comparatively recent and limited. Bowen and Millikan[19)] were the first in 1922. Regener[17)] and Bowen and Millikan[18)] made flights in 1932. These were all which had been made prior to the start of this problem, though Clay[20)] has since reported flights in Java whose results are probably wrong in view of Neher's recent as yet unpublished aeroplane flights in the Philippines.

In none of these flights was the accuracy comparable to that obtained under the less stringent conditions in the laboratory, on aeroplanes and in manned balloons. There were two reasons for this: first, inaccuracies of pressure readings, and second, lack of sensitivity of the electroscope itself. It was felt that the first could be improved considerably by use of very high quality but rather expensive bellows made by Julien P. Friez and Sons of Baltimore. This would not be worth while, however, nor perhaps would the whole project be worth while unless the electroscope sensitivity could be increased. The difficulty here was that in all previous flights the electroscope was fully charged before the take off and this one single charging had to last for the whole flight. Thus the sensitivity had to be extremely low. If some means of frequent recharging could be found, the sensitivity could be increased.

This problem then became simply one of finding a source of high potential which was not too heavy to be carried by small balloons. The source of potential should be able to deliver at least 300-400 volts and should not weigh over 100 grams. There seemed to be four possibilities.

1. Tiny storage batteries
2. A dry pile
3. A condenser
4. A voltage multiplier
5. Electrostatic generator

The first was immediately discarded because of weight. The lightest battery of this kind is 1 gram/volt which obviously does not meet the specifications. Dr. Neher built a dry-pile which functioned excellently at room temperature but whose voltage decreased about 0.2% per degree Centigrade drop in temperature. In spite of the fact that Regener had succeeded fairly well in controlling the instrument temperature during flights, great uncertainty was felt about this factor and so for the time being the dry-pile was abandoned. An additional reason for this was that it was questionable whether the dry-pile could be made light enough. The condenser looked reasonably promising as we shall see below, while the voltage multiplier and electrostatic generator actually were not thought of until somewhat later. It was at this point that the author entered the problem.

In order to consider the condenser possibilities a few facts about balloon flights and electroscopes must be considered. They are:

A. A flight lasts about 3 hours.

B. It seemed desirable to recharge the electroscope approximately every 5 minutes. Total number of charges 40.

C. The capacity of the charging switch and electroscope system would probably be about 3 cm. but 5 cm. was assumed to allow a factor of safety.

D. Because of the necessity of maintaining saturation voltage the Neher electroscope system, which we planned to use, is sensitive only in the upper 25 or 30% of its top voltage. For really satisfactory results the top voltage should not vary by more than 1/10 of this or 3% of the total. This 3% was arbitrarily split into two qual parts, one of which was to allow for loss of voltage due to charging, and the other to account for loss due to leakage.

Let a condenser of capacity C be charged to a voltage V_o and charge Q_o. When Q_o is shared with an uncharged condenser of capacity C', the charge Q which is left on C at a voltage V is given by

$$\frac{Q}{C} = \frac{Q_o}{C + C'} = V \quad \text{or} \quad \frac{Q}{Q_o} = (\frac{C}{C + C'})$$

If this is done n times

$$\frac{Q_n}{Q_o} = (\frac{C}{C + C'})^n \quad \text{or } C = \frac{C'}{(\frac{Q_o}{Q_n})^{\frac{1}{n}} - 1}$$

For the problem at hand $\frac{Q_n}{Q_o} = 0.985$, $C' = 5$ cm. and $n = 40$.

$$C \approx 13{,}000 \text{ cm.}$$

With regard to leakage, the allowance is seen from D above to be 1/2 % per hour. The law for condenser leakage is given by

$$\frac{V_t}{V_o} = e^{-t/RC}$$

Putting $t = 3600$ sec. and $\frac{V_t}{V_o} = 0.995$

$$RC = 7.2 \times 10^5 \text{ seconds.}$$

But

$$R = \rho \frac{d}{A} \qquad C = \frac{kA}{4\pi d}$$

$$RC = \frac{\rho k}{4} = 7.2 \times 10^5 \qquad \rho k = 9.05 \times 10^6$$

where ρ is the volume resistivity of the dielectric in static units, k its dielectric constant, A its area, and d its thickness. If ρ is changed to practical units, $\rho k = 8.14 \times 10^{18}$. It is thus obvious that the rate of voltage drop depends not at all on the dimensions of the condenser but only on the resistivity and specific inductive capacity of its dielectric. This treatment neglects surface leakage. It will be shown below how this was taken care of. Table I gives ρ and k for the very best insulators and the expected rate of leakage without extra surface leak

Table I

Material		k	k	Per cent leakage per hour
Fused Quartz	$>5 \times 10^{18}$	5	$>2.5 \times 10^{19}$	<0.16
Special Paraffin	$>5 \times 10^{18}$	2	$>1 \times 10^{19}$	<0.40
Ceresin	$>5 \times 10^{18}$	2	$>1 \times 10^{19}$	<0.40
Parawax	3×10^{18}	2	6×10^{18}	0.68
Hard Rubber	1×10^{18}			
Clear Mica	2×10^{17}	6	1.2×10^{18}	3.4
Sulfur	1×10^{17}	4	4×10^{17}	10.2

It is clear that only the first four of these satisfy the requirements. Mica is the only one of the substances in the above table which seems at all suitable mechanically for a condenser dielectric and it falls far short of the requirements. Just to be sure, however, Dr. Neher had already tested several commerical Mica condensers and found even the best to leak about 8% per hour.

Of the remaining substances, ceresin appeared to be the best possibility because according to Curtis[21] its surface resistivity is very high and is equally as good at 90% humidity as at 50%.

At the suggestion of Dr. Bowen the author's first endeavor was to make a dielectric from filter paper boiled in ceresin in a vacuum. A condenser was then made with aluminum, foil serving as the metal plate and again boiled in ceresin in a vacuum. Several were made and in no case was the leakage less than 6% an hour.

Next, in order to see whether even ceresin itself was good enough a condenser was made by dipping fairly flat pieces of sheet copper into hot ceresin and stacking them up in staggered fashion so that the free ends could be connected together as terminals. This was successful in producing a leakage as low as 0.75% per hour. Its weight was 13 grams but its capacity only 400 cm. This meant a total weight of 300 grams for 10,000 cm which was far too much. It was necessary to reduce the thickness of the dielectric considerably. As a result nearly the whole summer of 1933 was spent in an effort to get sheet metal really flat. The effort was not successful. Whenever the ceresin was made as thin as was necessary, a short circuit always developed.

In discussing this discouraging difficulty with Dr. Neher and Dr. John Strong one day, it was suggested by them that possibly flat metal plates could be made by evaporating metal on mica. This seemed to the author to be a very powerful suggestion for not only would the plates be flat but they would be far cleaner than it was possible to make any metal plates. After several months of research, the following technique proved successful.

Preparation of the Plates

By the method of Wright[30] large sheets of clear mica with very few imperfections were split to a thickness of from 0.03 mm to 0.05 mm with great care being taken not to leave steps in the surfaces. The modification by Strong[31] of using water as a help in splitting mica was not used because even the purest distilled water leaves a thin layer of contamination when it evaporates. The plates were then examined for flatness and the undesirables eliminated. By use of templets and with a needle as a scratching tool, these large sheets were marked into rectangles of two sizes and then cut out along these marks with a paper cutter thoroughly scrubbed with xylene. The sizes of these rectangles can be seen from Fig. 1a and b which are drawn to scale. Sometimes as many as ten could be obtained from one sheet of mica. It was then necessary to fasten leads on to both sides of each plate. The leads used were 0.002" nickel wire which could be easily straightened by pulling and then cut into 1 inch lengths with scissors. They were fastened at a particular spot near the edge of the mica plates by means of Aladdin Cement. This cement polymerizes on continued heating and forms a hard smooth surface practically tangent to both the mica and the wire.

Aladdin cement is a powder. When a quartz or glass fiber was moistened and dipped in the powder, enough powder stuck to make a little bead when heated with a hot wire. By touching the end of each nickel wire to this hot bead, a little cement adhered to each one. In this way all leads were tipped with cement. To cement to the mica one end of the wire was supported on the mica and the other on a horizontal needle at the same level. A hot wire was then applied until the cement melted, ran out on the mica, and was finally polymerized. As a factor of safety two leads were fastened on each side of the mica. In order to avoid having the cement lumps from alternate plates fall on top of one another and so separate these plates too much, the points at which the leads were fastened on opposite sides of the plates were displaced about 1/2 an inch from each other. Mica masks were then made with an opening having the shape and dimensions of the shaded portion of Fig. 1a and b. The mica plates were thoroughly dusted with a clean rabbit's hair brush, sandwiched between two masks, and then exposed on both sides to aluminum being evaporated by the process devised by John Strong[22)] of this laboratory. Fig 1a and b shows the plates after this process. The shaded area represents the aluminum coat. In order that the leads might be soldered later, they were bent in under the mask to prevent their being aluminized except at their contact with the cement. The coat was made opaque on each side in order to insure good conduction. The 1/10 inch wide lanes out to the edge were of course for the purpose of making contact with the leads. Each lead connection was tested for about 20 microamperes which is about twice the initial charging current per lead with 2 megohms in series with a 17 plate condenser and 500 volts applied. It was very early found that the leads burned out in time if no gridleak was in series. Now if all the leads on one of these plates be connected together the result is the equivalent of an extremely flat metal plate. The edges were left uncoated to eliminate trouble due to their inevitable raggedness.

Construction of Condenser

In order to minimize surface leakage, the condensers were always made with an odd number of plates, there being one more wide plate than narrow. The proper number of plates was selected, again thoroughly dusted with a rabbit's hair brush, and almost completely dipped in ceresin at a temperature of 80° C. In being dipped they were hung by spring clips from the lead end and all but the last 0.02" was covered. They were withdrawn fairly slowly so that the excess ceresin ran off. As the bottom edge came out it was slightly inclined to allow the remaining ceresin to drain. In this way only a very slight excess thickness of ceresin was left at the bottom and this was where one wanted it anyway. The layer was quite uniform and about 0.0008" thick on each side. The wide and narrow plates were then stacked alternately, staggered by about 1/10 of an inch, with the leads coming out in opposite directions. Two clear wide mica plates were put on the outside to prevent sticking and the whole clamped between two flat plates, heated to about 45° C, and squeezed enough to iron out irregularities and make the condenser hang together. While still in the clamp, the two sets of lead wires were soldered together with hot wire and needle, great care being taken not to get flux on the condenser proper, and to clean all flux from the leads when through. The finished condenser is shown in Fig. 1c. One very significant fact about the design is that the narrow end is the insulated side and so the only critical place for surface leakage is the line AB, Fig. 1c. This line is completely covered with ceresin which has good surface properties.

Tests of Condensers

Leakage at Room Temperature. Between Jan. 1934 and June 1935 about 20 of these condensers were made and tested for leakage at room temperature. None of them showed leakages of more than 1.5% per hour. More than 90% of them were under 1% per hour. The best run was on June 20, 1935 when over a period of 9 hours a 7700 cm condenser leaked only 0.2% per hour. These tests were made at about 450 volts, but the condensers proved able to stand 750 volts and still give small leakage. The general testing procedure was to charge the condenser and read the voltage with an electrostatic voltmeter which was a Neher electroscope system suitably mounted and calibrated. After some hours another reading would be taken and the average leakage calculated. In most of the tests the condensers were in an enclosed box with a siccative. However, in the one or two early cases where tests were made in the open, very little difference was noted. Later when condensers for the electroscopes were being made, some trouble with humidity was encountered. The explanation for this will be given later.

Leakage and Capacity at Low Temperatures. Because all previous flights by this institution had encountered low temperatures, the behavior of the condensers at these temperatures had be be investigated. Several runs were made which proved without question that the leakage was less at low temperatures than at room temperature. The exact amount of this change was not measured for it appeared from the results that the capacity increased with falling temperature. This caused relatively large potential drops as the temperature fell and in some cases small potential rises as the temperature rose again. The change of capacity between 22° C and -50° C was therefore measured. The resulting curve is shown in Fig. 2. The total increase in capacity was 7.4% of its capacity at 21° C. There could have been but three possible causes for this change. They were increase in dielectric constant, thermal contraction, and possible mechanical changes. That the dielectric constant of ceresin increases as the temperature falls is shown in U. Retzow[23]. Although Retzow's curve stops at 20° C, a reasonable extrapolation to lower temperatures would indicate an increase of about 1.5% and certainly not more than 2% between 20° C and -50° C. One also infers that most of the change would occur before -20° C. The coefficient of thermal expansion of ceresin was measured between 0° C and 20° C and found to be $= 2.3 \times 10^{-4}$ per Centigrade degree. This would give a capacity increase of about 1.6% between 20° C and -50° C. Thus only 3-4% or about half of the 7.4% capacity change has been accounted for by the first two factors. At Bowen's suggestion a condenser was subjected to pressure by means of two metal plates padded with 1/8 inch of cork and clamped together. This accomplished two things: first, it increased the capacity about 6% in such a way that when the pressure was released the condenser returned to within 1% of its original capacity, thus indicating that the pressure was taking up mechanical slack; second, it reduced the capacity change from 7.4% to 5%. This curve is also shown in Fig. 2.

This increase in capacity was annoying for it meant that much more apparent leakage if the temperature went down very much during a flight. This was one reason why an attempt was made to follow Regener's procedure and prevent the temperature from falling very low.

Behavior of Condensers in Electroscopes

Eleven condensers were made for the five electroscopes. In order to eliminate mechanical difficulties, they were pressed between 0.081" Dow Metal plates with 1/32" rubber pads to distribute the pressure. Light copper wires were used to connect the insulated side to an amber binding post and to the lead into the electroscope. During the calibrations and tests the condensers behaved reasonably well. Although most of the calibrations were run without condensers, 30 films were taken with them functioning. Table II shows the number of films for each electroscope on which the condenser discharged a certain per cent of the distance across the film in 3 1/2 hours.

Table II

Elect No	No of Films 0-20%	No of Films 20-40%	No of Films 40-60%	No of Films 80-100%	No of Films over 100%	Voltage
0	3	4	0	1	0	440
1	2	2	1	1	0	740
2	4	1	1	0	0	620
3	2	2	0	0	0	380
4	5	1	0	0	0	440
	16	10	2	2	0	

Since 40% of the distance across the film represents not more than 12% of the total voltage, we see that 87% of the runs gave less than 3 1/2% per hour, which, while not as good as hoped for, was considered satisfactory. The four which were worse were all early films taken before the condensers had become really dried out. These leakage figures also include loss of charge to the charging switch.

On the flights at Fort Sam Houston many things went wrong. Probably the worst failure was that of the condensers. As soon as San Antonio was reached the leakage on all condensers increased somewhat and that of the 740 volt and 620 volt condensers so much that they were worthless. New condensers were tried but were no better. These two electroscopes were therefore used at low pressure without condensers. Electroscope No. 4 was the only one to be returned while Dr. Neher was at San Antonio and its condenser had functioned excellently for the hour before its clock stopped. No. 0 and No. 4 on its second flight each showed a rapid increase of leakage as soon as the flight started so that the electroscope trace left the film after an hour to an hour and a half. No. 3's record was so faint that it didn't indicate how the condenser worked. Upon their return, the condensers of No. 0, No. 3, and No. 4's second flight were all short circuited. In order to explain these difficulties and to make the condensers foolproof, further research was carried on in the school year 1935-36.

The facts to be explained before proceeding with new condensers were:

1. The change from the early condensers which were insensitive to humidity to the later ones which were very sensitive.
2. Failure of high voltage condensers before the others.
3. The rapid increase of leakage immediately after the take-off for a flight.
4. The bad condition of the condensers upon return of the electroscopes.

Number one was explained by the discovery that there is more than one kind of ceresin. The product which has good surface qualities is distilled from the natural mineral ozokerite and has a single melting region of 65 - 70° C. The author was informed by D. D. Taylor that there was an artificial ceresin on the market which had extremely poor surface properties and a double melting point. It turned out that before making the electroscope condensers a new stock of ceresin had been ordered from Braun Corporation and the artificial product instead of the natural was received. Therefore great difficulty was encountered with humidity. Numbers 2, 3 and 4 are all explainable on a combination of a decrease in the resistivity of this artificial wax with temperature or a gradual decrease of plate separation due to the softness of the wax and the high attraction between the charged plates, or both. Since the lower of the two melting points of the artificial wax was about 50° C, and the attraction between the plates varied from 75 gmwt/cm² at 400 volts to 230 gmwt/cm² at 700 volts, it was indeed surprising that any of the condensers worked at all at the 35° - 40° C temperatures encountered at San Antonio. Naturally the high voltage condensers went bad first due to the greater attraction between the plates. As will be shown later, on most of the flights the temperature increased slightly for the first half hour. Either because of additional softening due to this increase in temperature or because of some phenomenon connected with the decrease of air pressure around the condenser, the plates tended to approach each other in the early portion of the flights of Nos. 0 and 4 (second flight). This increased the capacity of the condensers and caused a very rapid apparent leak. That the condensers were totally bad when the electroscopes were returned was due to an increase in this phenomenon as the electroscope lay on the ground in the sun. That the condenser of No. 4 was not ruined on its first flight was due to the fact that the electroscope was found on the day of the flight.

It was found also that the rubber pressure pads in addition to losing their quality at these high temperatures, also absorbed ceresin. Moreover, the copper wire which was painted with this wax disintegrated in time. This may have been due to vapors from the lacquer with which the interior of the recording chamber was painted.

New Condensers

Although natural ceresin might have come through the flights in good shape, it was decided that some mechanically strong spacer should be used to prevent all possibility of capacity change. In order to completely eliminate humidity effects and effects due to decreasing air pressure it seemed necessary to dip the final product in ceresin in a vacuum. Mica strips, quartz fibers, and ground fused quartz were all tried as spacers. In no case was it possible to dip the condenser in hot ceresin without producing a short circuit in it. This led Bowen to suggest that mica and spaced ceresin be tried in series. Thus even if contact was made at one or two points through the ceresin, the mica would prevent shorting and the leakage through the mica would be small because of the small area of contact. This of course requires that each piece of mica be coated on only one side. A large number of plates coated on both sides were already made, so the leads and lanes were merely scratched off one side, leaving the rectangular aluminum coat isolated. The condenser was made by dipping the plates in ceresin the conventional way and ten 40 u quartz fibers were used as spacers between each pair of plates. The whole was clamped between the usual Dow Metal plates, soldered up and dipped in ceresin after 12 hours in a vacuum. The resulting condenser leaked only 0.5% per hour but showed three times the capacity that was expected. Since ceresin has a dielectric constant of about 2 and mica about 6, it looked very much as if short circuits had as usual occurred between most of the plates, but due to the isolated aluminum coat on the back side of the mica, as soon as a short developed through a layer of ceresin, that layer was completely eliminated as a dielectric. To test this a plain mica condenser was made. Its leakage was but 0.5% per hour. Thus there is a surprising discrepancy between the 8% per hour of commerical mica condensers, the 3.4% per hour calculated limit for mica and the 0.5% per hour obtained here. Several of these condensers have been made since and have been put through quite severe tests. Since they have proved their worth, a fairly detailed description of their construction is given here.

Inasmuch as mica is the dielectric, the exact kind becomes extremely important. The mica used in these condensers was purchased from Eugene Munsell Company, 200 Varick Street, New York City. The order read: 1 lb. Mica, uncut, transparent, and as clear as possible, in sheets that will cut 4 inches square. Mica in sheets that will cut 6 inches square was also used. A recent letter from Munsell Company indicates that this was a high quality of Ruby Brazilian Mica. Since good condensers have been made from two different lots of this mica, ordered a year apart, it is assumed that they can be reproduced. The ceresin used is obtained from J. T. Baker Chemical Company of Phillipsburg, New Jersey. There are two brands which have good surface qualities and single melting points. They are: Ceresin Wax, White, a domestic product and Natural Ceresin Wax, which is imported from Austria. The second kind is now being used.

The mica for these condensers was split as before to a thickness of 0.03 to 0.05 mm. It was marked out and cut in the same way into the same sized plates. The wires were again 0.002 in, nickel but were now fastened to only one side of the mica and De Khotinsky cement was used instead of Aladdin. The reason for changing cement was that with only one side of the mica coated there were only half as many leads. Moreover, there were only a third as many plates per condenser due to the higher dielectric constant of mica. Thus each lead had to stand six times as much current as before. The De Khotinsky proved to be much better than the Aladdin Cement for this. As a factor of safety the protective resistance was raised from 2 megohms to 20 megohms. After the usual dusting process one side of the mica was coated with aluminum. In order to better control the thickness of the coat on the lead connections more evaporating filaments were used and only three plates were coated by each filament, as shown in Fig. 3a. Each lead connection was tested with 500 volts with 20 megohms in series which allows a factor of safety of 10 on normal use. An odd number of plates were again selected, dusted, and stacked exactly as before with care being taken that the coated sides be all either face down or face up. Two clear mica plates were put on each side of the condenser and the whole clamped reasonably firmly between the Dow Metal plates by which it was to be mounted in the electroscope. The leads were tied together with 0.002 in. nickel wire, and soldered together.

The insulated side was connected by larger nickel wire to the amber plug (A), Fig. 3b. About 4 inches of this wire (D) was left free for later connection to the charging switch of the electroscope. The uninsulated side was connected through the 20 megohm gridleak C to the Dow Metal plates. Fig. 3b shows the condenser all ready for dipping. The two cylindrical plugs B are the means by which the condenser is to be fastened to the electroscope. The condenser was then hung from the boom of the dipping mechanism shown in Fig. 4 and the whole enclosed and evacuated to about 10^{-1} mm Hg for 12 or more hours. The ceresin was then heated to 110° C by an electric heater and the condenser immersed therein for from 15 minutes to half an hour. After this the condenser was raised and both ceresin and condenser were allowed to cool, the latter cooling much more rapidly. When the ceresin was between 80° and 70° C the condenser was dipped in and immediately out again several times until covered with a uniform coat of ceresin at least 1/16" thick. As soon as the condenser was cold, air was let in and the finished product removed from the boom. It should be mentioned that all metal parts were thoroughly wiped with xylene before the condenser was put together. All solder flux was also carefully wiped off. The bottom of the amber plug and one or two other places including the S. S. White gridleak bubbled furiously in the hot ceresin. The condensers sometimes bubbled a little but soon stopped. It was considered advisable to keep the condenser immersed until all but the few chronic portions stopped bubbling.

New Condenser Tests

Several condensers made this way were dipped in different substances. The substances were domestic ceresin, Austrian ceresin, artificial ceresin, sealing wax, and Victron. The first two were good even at high humidities. The third was good when dry. The last two were no good at all. They were only tried in an effort to find a really high melting point substance which would work. One condenser with ceresin spaced with quartz fibers in series with mica was also made, as well as one in which aluminum foil was used instead of evaporated aluminum. Tests of varying number and severity were tried on several of these. They are enumerated in Table III.

Table III

	Domestic Ceresin	Austrian Ceresin	Austrian Ceresin Aluminum Foil	Quartz spaced Austrian Ceresin	Sealing wax Aluminum Foil
Time to reach 1% per hr after first change	8 hrs	6 hrs		2 hrs	∞
Percent leakage per hr at 23°C 60% humidity	0.5	0.1	0.2	0.4	1.2
Percent leakage per hr at 45°C 20% humidity	3.5	0.7	1.6	-	>5
Percent leakage 23 C, 70% hum.	0.6	0.2	0.3	-	-
Percent leakage 45°C, 70% hum.	-	0.7	1.6	-	-
Capacity per Plate	1400 cm	1400 cm	1050 cm	600 cm	1050 cm
$\frac{\text{Capacity } 45^\circ\text{C}}{\text{Capacity } 23^\circ\text{C}}$	-	0.998	1.022	-	-
Breakdown Voltage	>540	>1250	>540	>540	>540

High leakages at 45° C eliminate all but the Austrian ceresin condenser and possibly the quartz spaced one whose Aladdin cement leads burned out before this test could be made. The only real advantage of the mica and quartz spaced ceresin condenser was its small soak in. This is altogether offset by the tremendous amount of work to make this kind of condenser as compared with the straight mica. In justice to the domestic ceresin it should be said that this test was made on the last remnants of a 2 1/2 year old supply and that the fresh domestic product probably would have fared better. Since the mica condenser dipped in Austrian ceresin performed so well, and since the time was very limited, it was decided not to completely finish the investigation of quartz-spaced condensers and one or two other possibilities but to see if the excellent qualities of this condenser could be duplicated in others. If so they were certainly good enough for the electroscopes. The small change of capacity of these condensers between 23 and 45° is significant in that it means that no apparent leaks due to capacity change with temperature should be expected. This effect and the possible effect of a change of soak in with temperature have been further tested by running the temperature up and down at about 6° C per hour with no disastrous effects. The soak in seems to be slightly lower at high temperatures.

In order to test whether this condenser could be duplicated, three more were made and tested simultaneously with it for leakage at 3°C, 22.5°C, 37°C and 47°C. The results of these tests are shown in Table III A. An idea of the smallness of these leakages is given by the voltage drops in the 44 hour run at 22°C. They were respectively 14.6, 11.9, 14.1, and 11.3 volts out of an average of 500 volts. A contact failure prevented the leakage of No. 2 at 47°C from being obtained. The value of 1.7%/hr. was obtained before it was thoroughly soaked in.

Table III A. <u>Values in per cent loss of voltage per hour</u>

	No. 0	No. 1	No. 2	No. 3	Length of run
3°C	0.01	0.01	0.02	0.01	145 hours
22.5°C	0.05	0.07	0.06	0.05	44 hours
27°C	0.22	0.27	0.26	0.19	39 hours
47°C	0.7	0.8	<1.7	0.8	3.5 hours

Although Table III A seems to accentuate the discrepancy between the leakage predicted by Curtis' value for mica and the leakage measured, this is not necessarily the case. All these later condensers were coated with aluminum on only one side so that a portion of the dielectric may have been a thin layer of ceresin or of partial vacuum between the mica on the other side and the next coat of aluminum. The lowest leakage obtained with mica coated on both sides was 0.5% per hour. Since this, however, represents a discrepancy of a factor of 7 it might be well to consider briefly its possible causes. Curtis[21]), who made all but one of the resistivity measurements quoted, seems to have taken a great deal of care both in the preparation and environment of his specimens and in his measurements. Errors due to surface leakage were supposedly completely eliminated by use of a guard ring. However, he states himself that very few of the materials were so uniform that two samples would give values of the resistivity as close as 10 per cent, while a factor of 10 or even more was not infrequent. Thus a variation in the mica itself might account for the discrepancy. Another possible cause might be his failure to soak the mica in long enough although he says he watched this carefully. It might also be that Ohms law fails for these thin sheets. Still another possibility is that the small leaks measured here were due to the feeding back of soaked in charge. The author is inclined to doubt this, however. It seems that more accurate measurements of the resistivity of mica should be made.

It is the opinion of the writer that the problem of a source of high potential for sounding balloon electroscopes has been satisfactorily solved. The actual test of the solution of course must await more flights.

III. The Design and Construction of the Electroscopes

When in August 1934 it was finally decided that the original ceresin condensers would be satisfactory, the problem of designing the electroscopes was begun. Since the Neher electroscopes had been so successful, it was decided to attempt to adapt them to balloon work rather than to follow the design of previous balloon electroscopes too closely. However, it seemed likely that the 4 inch cylindrical ionization chambers from Bowen and Millikan's 1932 flights could be converted to the Neher type without too much trouble.

The first problem to be solved was that of the position of the condenser and the construction and position of the charging switch. It was decided to have an auxiliary chamber above the ion chamber connected to it as far as gas flow was concerned but electrically shielded therefrom. The charging switch was a toroid wound with wire with a short section cut out and mounted as an armature. One side of this armature was in the ion chamber and the other in the auxiliary chamber. An insulated wire ran through the armature at right angles to its axis of rotation thus making contact possible between the electroscope system and the condenser. The whole switch was gravitationally balanced so that very little power was required to run it. The condenser was then in a perfectly dry chamber and functioning under the conditions for which it has been tested. Contact for charging the condenser from outside was made by means of a gravity switch.

The recording mechanism including clock, film drum, slit, barometer, and thermometer was fastened in somewhat makeshift fashion to the outside of the chamber. Because of the necessity for more power, it was decided to use small clocks rather than watches which were used in all previous instruments. The clock used in this electroscope was taken from a cheap fad clock sold by Sears and Roebuck. The actual works were made by the Lux Clock Manufacturing Company of Waterbury, Connecticut. The remaining mechanisms were identical with those used on the final design and so will not be discussed further here.

The electroscope took much longer to make than had been anticipated and was not finished until January 1935. Condensers and electroscope system were installed and a few films run. In most respects the electroscope behaved quite well. The photographic recording was good and the condenser functioned so well that it was hard to tell that it discharged at all in 3 1/2 hours. There were, however, several difficulties, namely:

1. It was too expensive to construct and not very adaptable to mass production.
2. Calibration would be somewhat difficult due to lack of symmetry.
3. The clock proved to be quite irregular, having fluctuations of 5 - 10% over periods of two minutes.
4. It was difficult to load in the dark.

Dr. Bowen, Dr. Neher, Mr. Pearson and the author drew many new designs and had many discussions as to possible changes or even radically new designs. The chief difficulty was that if the condenser were to be in a chamber connected to the ion chamber for gas flow but electrically shielded from it, there must be practically three separate chambers provided: the ion chamber, the condenser chamber, and the recording chamber. It was at this time that one of the condensers was tried to see if it would work at 60 - 70% humidity. Since it worked well, it was decided to put the condenser in the recording chamber and lead the high potential into the ion chamber through a pyrex or quartz plug. This reduced the number of chambers to two and soon Dr. Neher evolved a design which met objections 1 and 2 completely and in addition promised to reduce the weight of the instrument a little.

The electroscope which was finally built is shown diagrammatically in Fig. 5 and photographically in Fig. 6. It will be desirable to discuss first the general mode of operation and later the experimental details of construction.

Recording Mechanism. Light, usually daylight, enters the ionization chamber (Z) Fig. 5 by the window (A), passes through the lens (B), out through the window (C), is reflected by a mirror (D) and exposes the film through the slit (E). The moving arm of the Neher electroscope system (F) which is mounted on a quartz rod (W) casts a shadow beam (G) which is focused by the lens on the slit (E). Both the barometer bellows (H) and the bi-metallic thermometer (I) have arms which lead to the slit and which are tipped with 0.0015 in. tungsten. These fine wires also cast a shadow on the slit so that the film has simultaneously shadows due to the electroscope system, the barometer, and the thermometer. It was necessary to have a fiducial mark as well. This was a fine tungsten wire mounted on a brass arm leading from the fixed end of the barometer. The cylindrical drum (J) on the outside of which is wrapped 12 1/2 inches of either S. S. Panchromatic or Plenichrome film is rotated once around by the clock in 3 1/2 hours, thus recording the three moving shadow traces and the fiducial trace. After this one rotation it is automatically stopped and the record is complete. The drum and clock are enclosed in a closely fitting spun aluminum pill box which protects the film in the event of the outer case being opened or broken. Fig. 16 shows a barometer calibration film C, and the flight films of No. 0 and No. 4 (1st flight). Fig. 17 shows No. 4 (2nd flight) and No. 1.

Automatic Charging Mechanism. If one is to frequently recharge the electroscope system he must have some kind of an automatic charging switch. The source of high potential is of course the condenser (I), which is charged to 300 - 750 volts, depending on the particular electroscopic system. It is connected to the amber post (V) and also to a tungsten wire which goes through a pyrex plug into the device (R) in the ionization chamber. There is a switch on the clock which connects the solenoid (N) to the flashlight batteries (M) every four minutes. When this happens the iron rod (O) is pulled into the solenoid against a conical spring. The motion of this rod causes the small brass block (P) to rotate through an angle of about 45 degrees. A 3 in. phosphor bronze rod (Q) fastened rigidly to this block but insulated from it by quartz tubing swings out through this same angle and makes contact with the electroscope system. Simultaneously the other end of (Q) pushes in the disc (S) and through it makes contact with the high potential tungsten wire. The disc (S) is of amber with a button of platinum passing through its center. Thus when it is pressed in as in the diagram, the platinum is insulated from the walls of the ion chamber, whereas when it is out against the opening in (R) it is insulated from the high potential and connected to the walls of the ion chamber, in this way screening the chamber from the high potential during operation. After one or two seconds of contact the clock switch breaks the current, and the conical spring causes the arm (Q) to swing back to where it is grounded to the case. Meanwhile a little spiral spring pushes the button S into its screening position. The rate of discharge of the electroscope is then continuously recorded for 4 minutes until it is again recharged.

Other Accessories. (K) is a piece of brass mounted on a pivot in such a way that it is gravitationally unbalanced. When the ionization chamber is below the recording chamber gravitationally speaking, the arm of this piece of brass rests against the balance wheel of the clock and stops the clock. When the ion chamber is above, the arm moves away from the balance wheel, allowing the clock to turn. This furnishes a very convenient way of keeping the clock stopped until the balloon is ready to take off. (U) is an opening into which a mirror and eyepiece can be inserted to ascertain whether the instrument is working properly. The condenser can also be charged at the post V through this opening. (T) is a chamber in which the siccative is placed. (Y) is the needle valve by which the ion chamber can be evacuated or filled with gas.

Construction Details. The ionization chamber is composed of two hollow spun steel hemispheres about 0.018 in. thick and of 6 in. diameter which were silver soldered together at their equators. The window holders, valve seat, gasket seat, and the truncated hemisphere of the recording chamber were also silver soldered on. This was accomplished all at once by a process devised by Neher. All the spun parts were heated to 900°C in a hydrogen furnace, thus being de-oxidized and annealed. The two hemispheres were then fastened together by three pins at their equators. A steel plug with a hole and set screw in it was put over each window holder. A tungsten wire spring stretched between the two held the windows in place. The valve and gasket seals were held in the same fashion by tungsten wire springs which hooked onto the middle of the window spring. A bar with hole and set screw was set across the equatorial plane of the truncated hemisphere. By means of this and another tungsten spring this hemisphere was held in place. Silver solder in suitable forms and amounts and paste flux were placed between all the parts to be soldered and the whole put in the hydrogen furnace and heated to 900°C. When removed, all joints were well soldered in about half the cases. In the other half the process was repeated or repairs by hand were made. This gave a silver soldering job without oxidation or deformation.

The other half of the recording chamber was a hemisphere with a flange, which was fastened by machine screws to the equatorial flange of the truncated hemisphere. Both of these hollow hemispheres were but 0.01 in. thick. Another addition to the general framework of the instrument was a Dow Metal cylinder which was fastened by screws to the truncated hemisphere right next to the ionization chamber. This in turn held on its other end a circular Dow Metal plate 0.081 in. thick in the plane of the equator of the truncated hemisphere. On this plate practically the whole recording mechanism was mounted.

The windows (A)(C) were slightly tapered glass cylinders, ground into place and fastened with red sealing wax. The lens (B) was a 7.5 mm focal length, 7 mm diameter cemented achromatic microscope objective purchased from the Wollensack Optical Company, Rochester, New York. It was crimped into the end of a hollow Dow Metal cylinder which was threaded on the outside. On the end of this cylinder was machined a flange to keep stray light from passing the lens on the outside. The lens holder was screwed and clamped into a Dow Metal arm which came down from the cover of the ionization chamber. By screwing the lens in or out the focus could be adjusted. On another branch of this same arm was clamped the quartz rod (W) which held the electroscope system which will be discussed later.

The cover to the ionization chamber was a thin steel conical shell with a copper tube soldered on at its apex. A flange around the bottom of the cone was provided with a tongue which fitted the groove of the gasket seat nicely.By means of this flange the cover could be bolted on with 18 #3 screws. Around the outside of the copper tube was wound a solenoid (N) whose magnetic field, practically unaffected by the copper, furnished the pull on the iron rod within the tube. The whole charging mechanism described above was mounted from this cover so that lens, electroscope system and charging mechanism could all be adjusted outside with assurance that they would be the same inside.

In order that the mirror (D) and slit (E) might be adjusted independently they were mounted on the ends of hollow cylinders with the slit cylinder fitting closely inside the mirror cylinder. Both were held firmly by the cylindrical clamp next to the slit. The mifror was mounted on the diagonal end of its cylinder with four screws so that by shimming one or more of the screws its angle could be changed. The mirror was a narrow strip of glass upon which had been evaporated aluminum. The slit was made by evaporating aluminum onto a narrow piece of glass upon whose surface had been stretched a 20 u quartz fiber. After removing the fiber a 20 u slit results. To protect the aluminum coat a thin piece of cover glass was fastened over it with Canada Balsam. To keep the walls of the camera black and also to cover up any holes in the aluminum coat this cover was painted with black Egyptian Lacquer almost up to the edges of the slit. All metal parts above were Dow Metal except the mirror cylinder which was brass. Of course various slots and holes had to be made in all these cylinders to let the light beam and the barometer and fiducial arms in.

The thermometer (I) was the thermal element taken from an ordinary oven thermometer. It was mounted on the side of the mirror cylinder.

With the lever arm used the sensitivity was about 7° C per millimeter on the film. The barometer was an aneroid bellows two inches in diameter and about an inch long. As mentioned above it was specially made for this laboratory by Julien P. Friez & Sons of Baltimore. The sensitivity is about 5 - 6 millimeters deflection per atmosphere. Since one could measure the shadows to about 2 u, the pressure could be read to 0.2 or 0.3 of a millimeter of Hg. However, due to other uncertainties such as variations of film development and possible slight lags in the barometer the actual pressures are certainly no better than 0.5 mm if they are that good. The temperature of course could be read more accurately than necessary.

The clock situation was investigated very thoroughly by both Dr. Neher and the author. Many clocks and watches were tested for fluctuations over periods of two to four minutes. Two other factors were that the timepiece had to run the charging switch, and also had to be regeared so that its main shaft rotated once in 3 1/2 hours instead of once an hour. Since a watch has hardly enough power to run a charging mechanism and because both this and the regearing would be very hard to install on a watch due to its smallness and compactness, a small alarm clock was finally decided upon. The clock chosen was one manufactured by the New Haven Clock Company ~~for use in small alarm clocks~~. It weighed about 60 grams. The alterations for charging switch consisted merely in putting two platinum contact posts on a gear already in the clock. The gear ratio alterations involved practically a reconstruction of the main shaft. Both of these jobs were very efficiently done by Mr. S. A. Asquith, 2514 Sunset Blvd., Hollywood. The fluctuations were not more than three per cent for two minute intervals.

The drum (J) was a 4 in. Dow Metal cylinder, hollow with the exception of a thin solid web of the metal near one end which connected it to its hub. This flat web rested upon a 1" diameter flat brass disc which was fastened on the main shaft of the clock. A projection from the drum fitted into any one of 24 holes in this disc so that the drum could be set within at least 7 1/2 degrees of any position. The drum could be removed from the shaft by merely unscrewing a nut on the main shaft above and lifting it off. The film was wrapped around the outside of this with emulsion side outward. It was held at one end by two of the very short projections of two small screws. At the other end it was held by two similar projections which were mounted on the end of a stiff piece of piano wire which was fastened near the hub. These two projections ran in narrow slots in the periphery of the drum so that variations in the length of films could be taken up. The wire into the hub of course furnished the tension by which the film was held tightly onto the drum.

The source of power (M) for the solenoid was a pair of Bond No. 101 Mono-Cell flashlight batteries connected in series. One of these batteries alone would just barely work the switch. Two in series gave a little factor of safety. An investigation of flashlight batteries of this size showed that these were the best. One of these cells would deliver 7 amperes on short circuit as against only 3 for Eveready and others. It was found that the batteries dried out and lost their current capacity in the dessicated chamber. To prevent this they were painted with beeswax. The batteries were normally merely wrapped in cotton or wool and were laid on top of the drum box.

The charging switch on the clock was merely an insulated strip of phosphor bronze plated with platinum (A) which normally rested at (B) against a spring terminal (C) of the solenoid circuit. The other terminal was the whole electroscope. Thus when the platinum post (D) on the uninsulated clock gear came around and touched this strip, the circuit was closed. (See Fig. 7) However, as the gear post pushed this strip it moved it away from its insulated terminal, thus breaking the circuit after about one or two seconds. Eventually the gear post snapped by the strip and the stage was all set for the next contact about 4 minutes later.

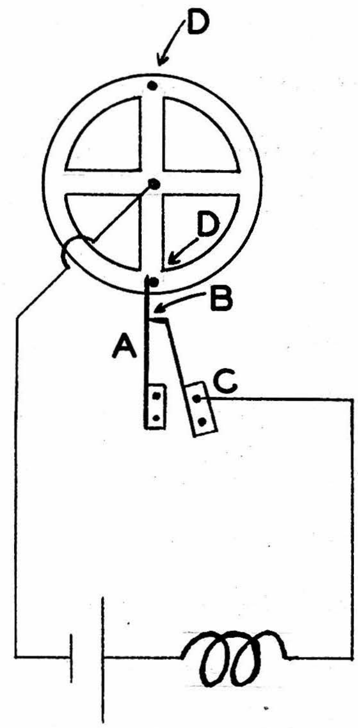

FIG. 7

The chamber (T) for the siccative had a fine nickel screen for its top and was ordinarily filled with Magnesium Perchlorate

$Mg(ClO_4)_2$. Next to P_2O_5 this is the most efficient dryer known. At 25° C it keeps the pressure of water vapor down to 6 x 10^{-4} as against 2.5 x 10^{-5} for P_2O_5. It has the advantage of having 5 to 7 times the water capacity of P_2O_5 and in addition does not make a devastating liquid like Phospheric Acid. When the condensers were behaving badly in Texas, Dr. Neher tried P_2O_5 but it proved no better.

Dow Metal was used wherever possible because of its extreme lightness. Dow Metal F was the Catalogue designation. It has specific gravity of 1.77 and is composed of 95.7% Mg, 4% Al, and 0.3% Mn. Its melting point is 1160° F and its coefficient of thermal expansion is 1.6 x 10^{-5} per degree Fahrenheit. It is manufactured by the Dow Chemical Company of Midland, Michigan. Some parts could not be made of this because it is practically impossible to solder. All possible solder joints were eliminated, but where necessary, brass was used.

IV. Calibration of the Electroscopes

The electroscope system used was the Neher all quartz torsion type. The construction of this system has been written up by Millikan and Neher[24)], and so will not be discussed further here. Its properties should, however, be remarked upon. Its working voltage range is 70 - 100% of its top voltage, the latter being anywhere from 350 - 700 volts. Over this range it has an almost linear voltage calibration. The reading for full scale deflection varies by less than 0.3% between +20°C and -20°C. It is insensitive to the direction of gravity and to almost all vibrations.

Let it be assumed that the slit of the camera is exactly perpendicular to the direction of motion of the film and that the charge sensitivity is exactly uniform over the width of the film. This is consistant with an almost linear voltage calibration since the capacity doesn't change much. If the electroscope, having been filled with argon for example, is now exposed to some constant source of radiation, the image of the electroscope fiber will move across the slit at a uniform rate. If the film also moves at a uniform rate, the result will be a sloping straight line. Since the film rate is always the same, the slope of the line will be proportional to the rate of motion of the electroscope image due to the exterior source of ionization plus the rate due to leak and radioactivity in the electroscope chamber itself. Since the rate of motion due to the exterior source is proportional to the rate of formation of ions in the chamber due to that source,

$$km = I + Z \tag{4.1}$$

where m is the slope, I the rate of ionization due to the external source, Z the equivalent rate of ionization due to the instrument itself, and k the constant of proportionality. Now k and Z are constants only of the instrument. If they are known, then for any measured slope m, I can be found.

The constant k could be determined by measurement of the capacity of the system, its voltage sensitivity, and the rate of motion of the film. Z could be determined from the asymtotic value which the slope approached as the electroscope is submerged in water, or else by the slope when the electroscope is placed in about 10 cm of lead in a deep mine or tunnel.

However, Millikan and Cameron[25)] had made very accurate capacity measurements on several of their precision electroscopes which were used for under water work. They[9)] had also by trial and error methods with the help of the Gold Tables found the asymtotic value which the under water curve was approaching as the depth increased. With these precision instruments, accurately calibrated, Millikan proceeded to detemmine with great care the numbers of ions per cm^3 per sec at 30 atmospheres pressure in one of his electroscopes when it was inside of a certain 10 cm thick spherical lead shield at a given spot in his basement.

This same procedure of calibration could have been followed with the balloon electroscopes. It is obvious, however, that this would be a very long and difficult process which should be avoided if it were possible to obtain the equivalent result in another way. Moreover, it is not certain that the two electroscopes thus independently calibrated would then agree, for instance, in the lead in Millikan's basement. In fact due to the much thinner walls of the balloon electroscopes one might find a slight difference between the two due to the Ubergangseffekte. This should certainly not be more than 1% for cosmic rays, however, between electroscope walls of 0.018

in. and 0.125 in.[27)]

Therefore in order to save time and expense and also to make the balloon results exactly comparable to those of the thicker walled electroscopes, it was decided to make the balloon electroscopes agree with Millikan's value of the ionization in the 10 cm lead shield in his basement. This value is 25.04 ions per cm^3 per sec at 30.1 atmospheres of air. To change it to one atmosphere it is necessary to divide by 13.80 which factor was determined by Millikan[28)]. If m_B is the measured slope on a balloon film taken in this lead, then

$$k\, m_B = \frac{25.04}{13.8} + Z \qquad (4.2)$$

where Z now is the zero of the instrument reduced to one atmosphere.

In order to determine both k and Z another relation is necessary. There is a tunnel in the San Gabriel Canyon of about 400 ft length running under a spur. Dr. Neher has rambled around on top of the spur with an aneroid barometer and has estimated the depth of the middle of the tunnel to be 234 ft. The density of granite, of which the San Gabriel Mountains are formed, is 2.6 gm/cm^3. Using the mass absorption law which seems to hold pretty well, gives 608 equivalent feet of water or 185 equivalent meters. However, the surface above is not flat as in the case of a body of water. This makes it somewhat easier for the rays to get in at inclining angles. If one approximates the granite over the tunnel by a sphere and takes account of the uniform 10 meter layer of the atmosphere above, one finds for the most penetrating cosmic rays ($u = 0.03$ per meter of water)[26] that the equivalent depth below the top of the atmosphere is 147 meters. This is certainly an absolute lower limit to the depth. From Regener's[8] under water curve one gets 0.15 volts/hr. for 185 + 10 meters and 0.3 volts/hr. for 150 meters below the top of the atmosphere. A compromise value of 0.2 volts/hr. was taken for the tunnel. Since Regener's volts per hour are almost exactly equal to Millikan's ions/cm^3 sec at 30 atm. the value of 0.2 ions/cm^3 sec has been used as the intensity of cosmic rays in the tunnel. The correction for local radioactivity was made in the following way. One of Neher's electroscopes was run in the tunnel, unshielded, and gave 94 ions/cm^3 sec practically all of which was due to local radiation. Previous experiments by both Millikan and Neher with both radium and thorium had indicated that only 0.6% of radioactive radiation registered by their electroscopes unshielded, was measured inside of the 10 cm lead shield. This gives a value of about 0.6 ions/cm^3 sec inside the shield due to local radiation and a total of 0.8 ions/cm^3 sec. The cosmic rays are of course unaffected by the lead after passing through so much granite. If m_T is the slope measured on a balloon film taken in the tunnel in lead, then

$$km_T = \frac{0.8}{13.8} + Z \qquad (4.3)$$

Combining (4.2) and (4.3) gives

$$k = \frac{25.04 \cancel{m_T} - 0.8 \cancel{m_B}}{13.8(m_B - m_T)} \tag{4.4}$$

$$Z = \frac{25.04\ m_T - 0.8\ m_B}{13.8(m_B - m_T)}$$

Thus one must only determine accurately m_B and m_T in order to calibrate the electroscope against those of Millikan, Cameron and Neher. This method of calibration should give k quite accurately and Z only approximately for the calculations for the ionization in lead in the tunnel may have been off by as much as 20%.

It is not usually true that the charge sensitivity is exactly uniform and the slit exactly perpendicular to the film motion. Corrections must be made for these factors. In Fig. 8 let CD be a line parallel to the motion of the film which intersects at D the linear electroscope trace AD. Let AC be perpendicular to CD at C and intersect AD at A. The measured slope of the electroscope trace will then be $m' = AC/CD$. If the slit AC' is not parallel to AC but makes an angle ϕ with it, the distance moved by the film while the electroscope was discharging from A to D is not CD but C'D. Thus the true slope is given by

$$m = \frac{(AC)}{(C'D)} = \frac{(AC)}{(CD) - (CC')} = \frac{(AC)}{(CD) - (AC)\phi} = \frac{m'}{1 - m'\phi}$$

If the slit were off the other way the negative sign would change to positive. The relation connecting the true and measured slopes when the slit is not perpendicular to the motion of the film is then

$$m = \frac{m'}{1 \pm m'\phi} \tag{4.5}$$

Since ϕ will always be small, this correction will be important only when m' is large, but may become extremely important there.

In Fig. 9 let the curve YZA be the non linear discharge curve of an electroscope exposed to a constant radiation. Let OX be the fiducial line and OY the direction of the slit which is taken here as perpendicular to OX. The electroscope then discharges in the negative y direction and the film moves in the positive x direction. If one were to measure short sections of this curve, he would get different slopes although the ionization is the same throughout. It is obvious therefore that a correction factor must be applied to make all these slopes agree. It was decided to take as the true slope of this discharge, the slope measured between Y and A, the two edges of the film. Let this be denoted by m and any other slope measured between ordinates y_2 and y_1 as m'. Let the difference in ordinates between YZA and the line YA at the abscissae x_2 and x_1 be Δ_2 and Δ_1. Then

$$m' = \frac{y_2 - y_1}{x_1 - x_2} \qquad m = \frac{(y_2 - \Delta_2) - (y_1 - \Delta_1)}{x_1 - x_2}$$

$$m = m'\left(1 - \frac{\Delta_2 - \Delta_1}{y_2 - y_1}\right) \tag{4.6}$$

It remains only to show that the ratio $\frac{\Delta_2 - \Delta_1}{y_2 - y_1}$ is independent of the value of m. Let the actual speed of the electroscope image in the y direction be v_t where t = 0 when y = Y, and let the y speed of the hypothetical image which travels the straight line YA be v. Also let Δ_t = the ordinate difference at any time t. Then

$$y_2 = Y - \int_0^{t_2} v_t \, dt \qquad \Delta_2 = vt_2 - \int_0^{t_2} v_t \, dt$$

$$y_1 = Y - \int_0^{t_1} v_t \, dt \qquad \Delta_1 = vt_1 - \int_0^{t_1} v_t \, dt$$

$$\frac{\Delta_2 - \Delta_1}{y_2 - y_1} = \frac{v(t_2 - t_1) - \int_{t_2}^{t_1} v_t \, dt}{\int_{t_2}^{t_1} v_t \, dt} = \frac{v}{(v_{av})_{t_1 t_2}} - 1 \qquad (4.7)$$

These relations are true because all points having an abscissa x_t are exposed at the same time t. Because v depends only on the intensity of ionization and $(v_{av})_{t_1 t_2}$ only on this and on the ordinates y_1 and y_2, there is nothing in (4.7) which depends on the speed of the film. Since by varying this speed we can give m any desired value, it follows that $\frac{\Delta_2 - \Delta_1}{y_2 - y_1}$ is independent of the value of m and only dependent on the ratio m/m'. Thus it is only necessary to determine and plot the Δ's as a function of y for one value of m. This was customarily done for m = 1. Fig. 10 shows a typical curve.

One other correction was necessary. The intensity of cosmic rays varies with the thickness of the atmospheric blanket. From Millikan and Cameron's[9] under water curve one finds that this correction amounts to -0.45% per 1/10 in. of Hg for 10 cm lead shield at approximately sea level. Millikan's value of 25.04 ions was determined for a barometric pressure of 29.26 in. Hg. All calibration slopes were corrected to conform to this pressure.

Whenever the electroscopes were being run in a place where the slope would be less than 0.2, the charging switch was disconnected and the electroscope allowed to discharge completely across the film. Otherwise the condensers were allowed to function in the usual fashion.

There were five instruments in all. In order to attempt to detect possible heavy particles in the stratosphere two were filled (Nos. 1 and 2) with air and three (Nos. 0, 3, 4) with argon. It was thought that there might be more recombination in the air electroscopes due to high density of ionization if heavy particles were present. The equivalence of air and argon for cosmic rays had previously been tested by Millikan and Neher up to 22,000 ft. This incidentally was one of the very strong arguments for the active agent of ionization up to this altitude being electrons.

In considering the following discussion of the actual calibration, one should bear in mind that it was done in a very limited time, for the approximate date of the flights had to be set considerably in advance. Several films in Millikan's basement were taken for each instrument. One film for each was taken in the tunnel. After a run in the Sophomore Laboratory with thorium it became obvious that #4 leaked. After it had been repaired there was no time to do the tunnel run over again so #4 was run in symmetry with all the others in two places on the campus and also in Millikan's basement. Actually, as will be shown later, No. 4 was only run against No. 0 and No. 3 for when subject to local radiation the air and argon electroscopes did not agree. On these last runs it was discovered that the focus of No. 3 was not satisfactory and so it was dismantled. When reassembled it was run against No. 0 and No. 4 both on the campus here and at Fort Sam Houston, Texas. After its first flight No. 4 had to be taken apart and repaired. It was recalibrated by one film at Fort Sam Houston and one in Millikan's basement after its return. The two air electroscopes were the ones whose condensers went bad before their flights. Therefore each of these was reduced to one atmosphere pressure of argon and used without condensers in the former manner. The only calibration run possible with these was one at the standard spot at Fort Sam Houston because all the other instruments had been sent off. As a result it was necessary to make No. 1 agree with the others at some point on the flights. The reason why more of the instruments could not be recalibrated after coming back was that No. 4 was the only one which was working upon its return. All of the results of these flights therefore depend upon the original calibrations of No. 0 and No. 3. These represent about 25 hours run in Millikan's basement and about 6 hours run in the tunnel. The complete summary of the calibrations is given in Table IV. The following is the legend used.

Table IV

	0	3A	3B	4A	4B	1A	1B	2A
R. A. M. Basement in 10 cm Pb	1.813	1.813	1.813	1.813	1.813	1.813		1.813
San Gabriel Tunnel in 10 cm Pb	0.058	0.058				0.058		0.058
Electroscope Constant	58.3	53.0	42.6	46.0	62.4	79.8	536	74.4
Electroscope Zero	0.89	0.43	0.23	0.26	0.91	0.08	3.79	0.10
Sophomore Lab. with Thorium	125.7	126.6				108		107.4
On Campus near Astrophysics	9.84	9.71		9.59		7.84		7.80
Sophomore Laboratory	11.14	10.97		11.18		9.16		8.89
Sophomore Laboratory	11.14		11.21	11.24				
Fort Sam Houston 8/3/36 Press = 29.15 in Hg	6.00		6.23	6.57		5.18		4.98
Fort Sam Houston later with pressure corrections					6.29		6.29	
Above Fort Sam Houston at 4.29 meters of Water							45.0	
R. A. M. Basement in lead with Thorium	17.89							17.00
Carl Anderson's Water Tank with Thorium	383							348

4A	No. 4 after leak was fixed	120 lbs argon
4B	No. 4 after its first flight	120 lbs argon
3A	No. 3 before being refocused	75 lbs argon
3B	No. 3 after being refocused	85 lbs argon
0	No. 0	115 lbs air
1A	No. 1 filled with	225 lbs air
2A	No. 2 filled with	260 lbs air
1B	No. 1 filled with	14.7 lbs argon

Underlining denotes runs used for determining constants. All values are in ions per cm^3 per sec per atm.

Several factors concerning this table should be discussed. First, 0 and 3A agree reasonably well on all their runs at both high and low ionization. On the runs in Pasadena by which 4A was fitted to 0 and 3A, the consistency was quite good. In view of these agreements which are all within 2% it was surprising to find the 10% disagreement between 0 and 4A at Fort Sam Houston. On this run the electroscopes were set 50 ft north of the weather station on the ground. The author feels that the radioactivity of the ground may not have been uniform and that 4A was more exposed than 0. Whatever the explanation, this disagreement weakens the accuracy of the results somewhat because the calibrations of 4B and 1B depend upon the ionization at this place. Second, 1A and 2A, the two air electroscopes, agreed with each other quite well on all four of their simultaneous runs but showed about 20% less ionization than the argon filled instruments. This was surprising in view of the fact that Millikan and Neher had found that argon and air electroscopes which had been calibrated against each other at least partially in lead, agreed very well not only for cosmic radiation but for radioactive radiations. This discrepancy is apparently due to difference in wall thickness. The 1/8 in. walls of the large electroscopes will certainly stop all β particles with energies less than 2 million electron volts while it should take only approximately half a million to get through 0.018 in. steel walls. Since there are almost no natural radioactive β particles either primary or secondary of more than 2 million volts while there are a large number between 0.5 million and 2 million, there will be many β particles which will get into the balloon electroscopes which could not enter those with thicker walls.

This led Millikan to suggest that the difference is due to the difference in recombination in air and argon. It is well known that β particles ionize very heavily near the end of their ranges. Many of those getting through the walls will have lost most of their energy and will thus be ionizing very heavily. Inasmuch as the argon and air were at 8 and 18 atmospheres respectively, these particles should not go far before being completely stopped. Thus most of this heavy ionization should take place near the walls where the field is weakest. Since the initial recombination in nitrogen and oxygen is much worse than in argon[29], it is not surprising that the air electroscopes failed to collect as many of these ions as the argon instruments. This effect is also accentuated by the fact that the air pressure was double that of the argon. While this seems to be a very reasonable explanation, it should not be taken as fact until a special investigation has been made.

The reason for comparing 0 and 2A subjected to thorium in Millikan's basement in lead and in Carl Anderson's water tank was to see if eliminating these β particles would remove the discrepancy. The results show that it is reduced but not eliminated. Secondary particles from air and lead probably account for the remaining difference but again further investigation is necessary.

The barometers were calibrated by enclosing the instruments in an air tight container and pumping down in stages of about 5 cm of Hg to about 0.3 cm. The pressures were measured by a closed tube manometer on the closed side of which a pressure of $< 10^{-1}$ mm was maintained by a separate pump. Meniscus and temperature corrections were made so that the resultant pressure values were good to at least 0.3 mm. These calibrations were made at 23° C and at 0° C, the latter measurements being made in the Cold Room of the Biology Department. In most cases this made a difference in the total expansion of the bellows which corresponded to about 6 mm of Hg. No runs were made at higher temperatures because it was expected that the temperatures would be low in spite of attempted compensations. A brief discussion of the actual temperatures encountered and their possible effect on the pressure readings will be given below.

The thermometers were calibrated by enclosing the electroscope in a Balsa-wood box and cooling with solid CO_2. The temperature was lowered to between -10° and -20° Centigrade in about 3 hours. A thermocouple with one end connected to the inside of the electroscope next to the thermometer element, and the other to a potentiometer gave the true temperature. The light was turned off at each reading in order to mark the film.

V. Flight Technique

An electroscope which was to be sent off was prepared for its flight in the following way. Fresh siccative was placed in the drying chamber and fresh dry batteries connected to the solenoid. The clock was suitably wound, the film loaded in the usual manner, and the hemispherical cover fastened on with screws. In most cases in order to keep the inside dry, this joint as well as all screws which connected with the recording chamber were painted with beeswax. After the condenser had been charged and checked through the eyepiece opening, the cap (U) was screwed on loosely in order to provide air passage for the barometer. A small reflector was fastened to the window (A) at such an angle that when the ion chamber was above the recorder, the light which exposed the film came from slightly below the horizontal. This insured light from the earth and sky near the horizon. This light should be quite constant both in intensity and in spectral distribution, whereas skylight from overhead gets less and less strong as the top of the atmosphere is approached.

Part of the heat insulation was next provided by wrapping the whole instrument with about 1 in. thick white wool. In order to protect the instrument in case of a hard landing, it was supported in a basket made of 1/16" steel wire, thoroughly braced with finer wire. The electroscope was supported by six steel wires of the same kind. These were fastened by screws to the junction of the two electroscope spheres and also hooked on to the basket, where they were wired in place. To provide for the remainder of the heat insulation the cage was completely covered with cellophane. Thus besides the wool, a layer of fairly still air surrounded the instrument. One quarter to one third of the cellophane was painted black so that it would absorb some of the sun's radiation and supply a little extra heat. This was merely an estimate since the exact calculation of the problem of temperature control is virtually impossible. The extent of success obtained will be discussed later. The electroscope was then ready for its flight and was kept upside down until the send-off in order to keep the clock stopped.

The balloons were 2 and 2.5 meters in diameter and were composed of sectors of very thin rubber cemented together. They were provided with a thick rubber pad 1 foot in diameter surrounding the appendix. There were in addition five lugs on the balloons, one on top and four evenly spaced around the equator. It was natural to suppose that the load was to be attached to the four equatorial lugs and that the top lug was merely for support while inflating. This was the method used in all the 1932 flights by both this laboratory and by Regener. When this time the balloons did not seem to give proper altitudes, the load was suspended in one case from the appendix and in another from the top lug. Later it was found that the appendix actually is the proper point of suspension. In two cases two 2 meter balloons were used with the loads suspended from the side lugs. The balloons were made by the Continental Caoutchouc Company of Hanover, Germany. The weight of the 2.5 meter balloons was about 10 lbs and the 2 meter somewhat less.

A six foot square, bright red, China silk parachute was provided to bring the instruments down when and if the balloon burst. The red color was chosen to attract attention and so to make the recovery quicker and surer. The top of this parachute was provided with a release to clear it from the remnants of the balloon. Four strings extended from the corners of the parachute to a 1 ft wicker ring which acted as a spreader. Below this was usually hung the electroscope, although a vane for turning the instrument was inserted for one flight but later found unnecessary.

As soon as the morning fog cleared the inflation was begun. The balloon was filled with hydrogen until it would lift weight equivalent to the actual load plus 1200 gms free lift, if it was a 2.5 meter balloon. In the case of the 2 meter balloons 800 grams free lift was provided. The resultant rate of rise was 600-700 ft per minute. When the inflation had been completed, the parachute and instruments were fastened below and the balloon released. The man holding the instruments had to keep nearly under the balloon until it was high enough to swing the instruments clear of the ground. In these flights very little wind was encountered. Almost the last act of the instrument carrier was of course to give the electroscope a sharp twist to start the clock. Three theodolites were used to follow the balloon and measure altitudes. With suitable Army publicity throughout the countryside, it was hoped that recovery would be speedy. Fig. 11 shows the inflation of a 2 meter balloon. Fig. 12 shows the electroscope and parachute ready for the ascent. Fig. 13 shows two balloons with parachute and instruments shortly after the send off.

VI. Results of the Flights

Six flights were made in all. Electroscope No. 4 was found on the same day it was sent out and therefore was recalibrated and sent out again. Table V gives a summary of the results of the flights except the actual ionization measurements. Of the three balloons which did not come to rest with the instruments, two were found. Since all six instruments were returned eight out of nine possible recoveries were made or 88 per cent. One outstanding fact revealed here is the very poor altitude reached in all the flights except the one on August 7, 1935. It is believed that the rubber deteriorated in the long trip from Germany and in the heat encountered in Texas.

Table V

	Date Sent	Date Recovered	Maximum Pressure Altitude in Feet	Maximum Theodolite Altitude in Feet	Distance in Miles	Azimuth Clockwise from North	Interval in feet givi Electroscope Record	Cause of Failure	Ground Temperature	Maximum Temperature	Minimum Temperature
Electroscope #0 2 meter Balloon	8-4	10-18 8-4	43,000	-	47 102	338 346	0-39,000	Condenser	36°C	37°C	15°C
Electroscope #3 2 1/2 meter Balloon	8-6	11-17 11-17	-	36,000	23 23	310 310	None	Not exposed	-	-	-
Electroscope #4 2 1/2 meter Balloon	8-7	8-7 8-7	-	63,000	83 83	302 302	19,000- 29,000	Drum caught	29°C	32°C	-
Electroscope #4 2 1/2 meter Balloon	8-12	(9-1)	36,200	48,000?	25	8	0-29,000	Condenser	35°C	39°C	20°C
Electroscope #1 2-2 meter Balloons	8-20	(10-10) (10-10)	39.000	-	43 43	301 301	0-38,000	Electroscope all discharged	33°C	36°C	18°C
Electroscope #2 2-2 meter Balloons	8-22	8-26 9-4	41.200	42,000	50 112	323 333	None	Accidental Discharge	34°C	37°C	13°C
Radio Transmitter		(9-8)			50	323					

It is significant that only two out of eight balloons burst and even those at the unconventional altitudes of 36,000 ft and 21,000 ft. In all other cases the balloons merely sprang leaks and descended slowly. It is interesting to note that in three cases the balloons and instruments were found together, while in three more they were not. On the flight of August 7 and August 20 the balloons brought the instruments down and collapsed with them. On the flight of August 6 the balloon burst at 36,000 ft and being tied too close to the electroscope fell on it before the release could work. In the crash the electroscope system was broken but the clock continued to run and the film was undamaged although the bottom of the electroscope was badly smashed in. This speaks extremely well for the protection given the film by electroscopes of this design. In the flight of August 12 the parachute release apparently let go at about 10,000 ft on the descent. This electroscope came back in perfect condition with hardly a hole in the cellophane, thus showing the size of parachute to be sufficient. The balloon has not yet been found. In the two other cases where the balloon came to rest far from the instruments, it is quite apparent that the instruments in striking the ground before the balloon, were disconnected when the parachute release worked. The balloon thus relieved of its load rose rapidly and traveled 50 - 70 miles further.

That all the flights except one landed in the northwest quadrant was surprising at the time. The meteorologists, however, say that such a phenomenon is to be expected in the summer in Texas. In no flight was the ionization measurement successful to the maximum altitude reached. The condensers caused most of the difficulty. However, on the one really high flight (August 7), the condensers were working well but the drum caught, stopping the clock at 30,000 ft. The satisfactory performance of the condensers on this flight was probably due to the temperature being the lowest of any flight. This in turn was due to the fact that the send off was 9:25 A.M. instead of the usual 11 A.M. to 1 P.M. In general the instrument temperature rose a little immediately after release, in every case reaching a maximum 1° to 4° C above the starting temperature after about 20 minutes of flight. The temperature then fell steadily to a minimum when the electroscope was again nearing the ground. This rate of decrease was about 20° C per hour at high levels. For a higher flight this might take the temperature too low. Therefore the cellophane will be painted half or more black on the next trial.

The curve obtained in these flights of ionization against depth below the top of the atmosphere is given in Fig. 14. Each of the points plotted represents a 4 minute reading except some of two minutes or less near the top. All the corrections mentioned above have been made. There remain, however, several sources of error. Their magnitude and the means taken to correct for them should be mentioned.

Systematic Errors.

1. There is some possibility of a mechanical lag in the aneroid bellows. That this is small has been shown in calibrations

both by Julien P. Friez and by Dr. Neher. The latter finds when one of these aneroids has been taken down to one third of an atmosphere and then returned to atmospheric pressure, that a half hour after the return the barometer registers within 1 millimeter of its reading before the pressure reduction. By decreasing the pressure during calibration at approximately the same rate as it decreased during the actual flights, this error was further eliminated. It is probably negligible compared to other errors and fluctuations.

2. There may have been a slight barometer lag due to the failure of the pressure fall inside the recording chamber to keep pace with the fall outside. If this difficulty existed it was because the recording chamber was sealed too tightly in order to keep its humidity down. It is impossible to estimate this error for the theodolite heights were not dependable enough. With the humidity problem solved, it will be easy to eliminate this uncertainty in future flights.

3. A temperature correction should be applied to the barometer. It was not possible to calculate this because no calibration runs were made at high temperatures. Judging from the temperature correction between 22° C and 0° C this correction should certainly be less than 0.04 meters of water in every case and probably negligible in the two flights of No. 4. An idea of the magnitude of this correction will be gained upon recalibration.

4. In spite of the carefully planned method of calibration, there is still a chance of error due to wall effect. Although these

thin walled instruments were made to agree with the thick walled ones for the hard cosmic rays inside of 10 cm of Pb, they might not neccessarily agree for the soft cosmic rays aloft. Since these instruments are for use with these softer rays it might be advisable to calibrate with softer rays. Both methods will be tried before the next flights to see if they agree. In this respect these thin walled chambers also differ from those of the Dallas flights of Bowen and Millikan[18)] which were calibrated with γ rays from radioactive substances.

5. A systematic barometric error may have been caused by a difference in the shrinkage of the calibration films and the flight films caused by the difference in their histories before and during development. Correction has not been made for this. Its tendency would be to make the pressure readings too high. In the future an attempt will be made to take account of this as well as the differences in temperature and wetness of the film when it is measured.

6. No temperature correction for the recombination in argon has been applied because it has been found that for the pressures used here and the small temperature variations it is negligibly small.

Random Errors

1. The lack of sufficient calibration in Pasadena and the unexpected disagreements in the value of our standard ionization at Fort Sam Houston allow an uncertainty of several per cent in the constants of all the instruments except No. 0. This may have caused a

random error of a few per cent.

2. Clock fluctuations probably account for a large part of the scatter of the points of individual instruments. Some of the clocks were tested and found to have a mean error of about 1% for a two minute period and a maximum of 3 - 4%. For a four minute run these would be somewhat less.

3. Convection currents cause the electroscope fibre to weave, thus making wiggly lines and adding considerable uncertainty to the slope. Fortunately, the method of heat insulation used eliminated convection almost completely except at the very beginning of the flight.

In view of these various sources of error, the values of the ionization represented by the curve may be in error by as much as 10 per cent. The shape of the curve is probably somewhat better (5%) though the fact that some of the electroscopes only contribute points over short portions of the curve may tend to distort it a little.

In view of the contention made above, that single discharge electroscopes are not as accurate as those using multiple discharges, it is fitting to discuss briefly the spread of the readings of electroscope No. 1 without condensers as compared to the spread of the other instruments. At pressures greater than 4.5 meters there is no question but that the internal consistency of No. 1 is much worse than that of the others. At lower pressures it is as good or slightly better. This surprising result is due to three different causes. First the film from No. 1 had by far the most contrast of any film, thus making its measurement easier and more accurate. Second, the top few values on No. 0 and No. 4, second flight, were for lengths of time much less than 4 minutes because of the low voltage of the rapidly discharging condensers. Considerably better consistency would have been gained with longer discharge because the clock fluctuations for four minutes are much less than those for 2 minutes. Finally, and most important, No. 1 went off scale at but 40,000 ft after only 1 hour and 10 minutes of flight. Since it is hoped with better balloons to reach an altitude of 80-90,000 ft in a 3 hour flight, the sensitivity of No. 1 would be far too high. It should be decreased by a factor of 4 or 5 at least. With the sensitivity thus decreased, the uncertainties which occurred at pressures higher than 4.5 meters on this flight would run all the way to 2 meters. Thus these flights bear out to some extent the need for a higher sensitivity than single charges will allow.

In Fig. 15 are plotted besides the Fort Sam Houston results (FSH), other very high altitude results obtained previously by Bowen, Millikan, and Neher of this laboratory. The curve MF up to the cross represents the result of aeroplane flights from March Field (41N). The curve D represents sounding balloon flights from Dallas (42N) and the curve (SF) is that of the Settle Fordney balloon flight (52N). At the time the Dallas results were published, very small accuracy was claimed for them. The FSH curve indicates that up to 2 meters these results were essentially correct. However, the FSH curve at 39° N should fall slightly below both the 41N and 42N curves. Instead it is

3 to 4% above. The discrepancy with the Dallas curve is not of great importance since it is for less than the combined experimental errors. Neither should the discrepancy with the aeroplane curve be too disquieting in view of the possible sources of error mentioned above.

These electroscope flights did not produce any startling results as far as the study of cosmic rays is concerned. They agree well enough with the previous results[11] that the Gold analyses applied there need not be done over. Since the previous flights a method of analysis has been developed by Gross[32] and its experimental verification is claimed by Regener and Pfotzer[33]. This analysis has been used by Gross[32], Regener and Pfotzer[33], and Compton[16][34], to bring out humps in some of the ionization altitude curves. These humps are attributed to magnetic range minima of electrons and α particles entering the earth's atmosphere from without. Whether this analysis is valid or not, the author does not believe, in view of the errors mentioned above, that the shape of the curve obtained by applying such an analysis to the Fort Sam Houston data can have any significance. The results of these flights therefore say very little about the validity of these humps. Within about 5%, however, there do not seem to be any irregularities in the curve.

In conclusion, the author wishes to thank the members of the department and shop and the graduate students, all of whom have been most helpful in many ways. In particular, the sound advice of Dr. Bowen, the able direction of Dr. Millikan, the excellent instrument design and construction of Mr. Pearson are deeply appreciated. The author was extremely fortunate in and is most thankful for being able to work with and immediately under the direction of Dr. Neher, without whose generous help very little progress would have been made. All of this work was made possible through a grant from the Carnegie Corporation of New York administered by the Carnegie Institution of Washington.

VII. Summary

1. For use as a source of electroscope potential, condensers having leakages of less than 1% an hour at room temperature were developed. Later because of the failure of these at high temperatures a new type of condenser was developed, having a leakage less than 1% per hour for all temperatures from 2° C to 47° C.

2. The coefficient of thermal expansion of ceresin was roughly measured between 0° C and 20° C and found to be $2.3 \times 10^{-4}\ {}^{\circ}C^{-1}$. A large discrepancy between the tabular values for the resistivity of mica and the values obtained from the leakage of condensers was found. This invites further investigation.

3. The design of the Neher automatically recording electroscope has been modified to make possible its use for sounding balloon flights.

4. Five of these instruments were constructed, calibrated, and flown from Fort Sam Houston, Texas, Magnetic Latitude 39° N, using the earlier type of condenser.

5. Because of the failure of condensers and balloons, ionization records were obtained only to 40,000 ft.

6. These results check those of Millikan and Bowen from Dallas in 1932 with regard to the shape of the ionization depth curve. A discrepancy of about 5% in absolute values both with the Dallas results and the aeroplane values from March Field indicates that method of calibration should be further investigated for effects of wall thickness.

7. Within a limit of about 5% which is set by the scatter of the points, there are no irregularities in the curve between 7 meters of water and 2 meters of water. At depths greater than 7 meters of water the data are very inaccurate.

8. The advantage of repeated charging has been demonstrated.

9. Possible systematic and random errors in the ionization and pressure values are discussed with suggestions for future flights.

10. The following features were found to be somewhat unsatisfactory and will be or have been altered.

a. The balloons did not rise high enough because of leaks.

b. As mentioned above, the condensers failed.

c. The super-sensitive panchromatic film fogged too much in the heat. Film which is not red sensitive will be used hereafter.

d. The temperature compensation was not quite sufficient. More of the cellophane will henceforth be painted black.

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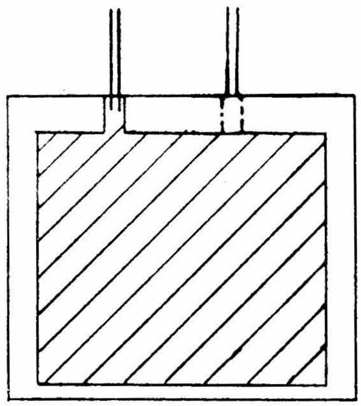
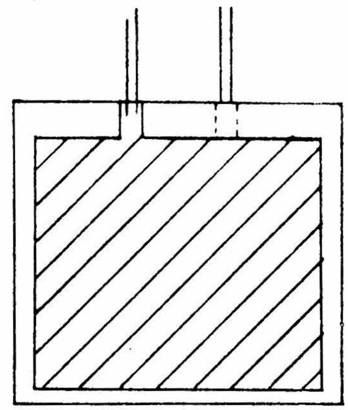
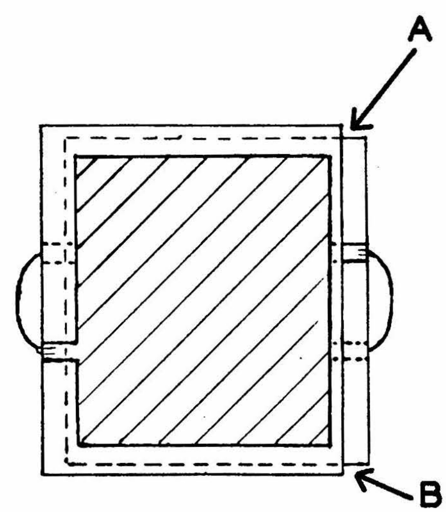


a. WIDE PLATE

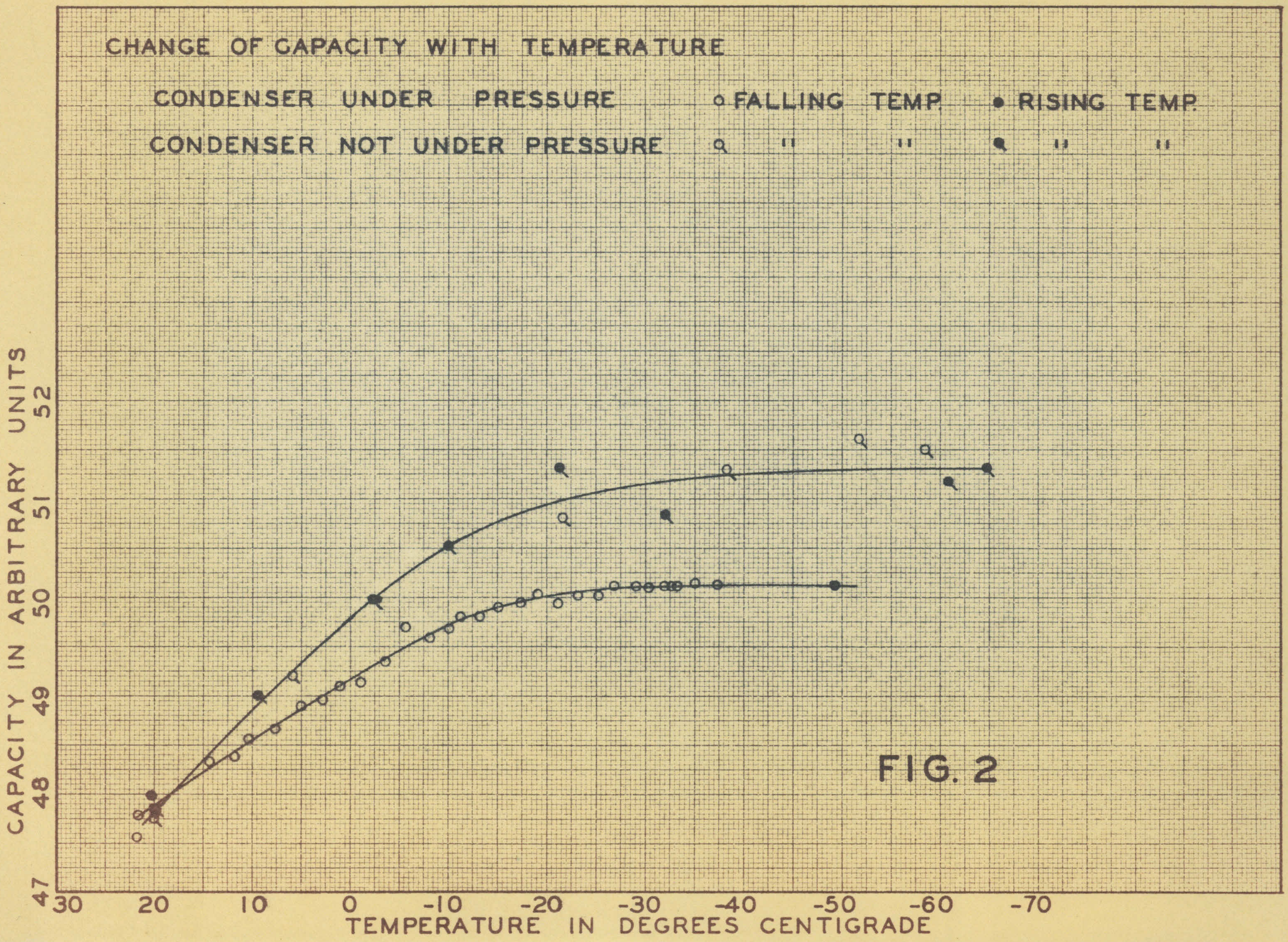


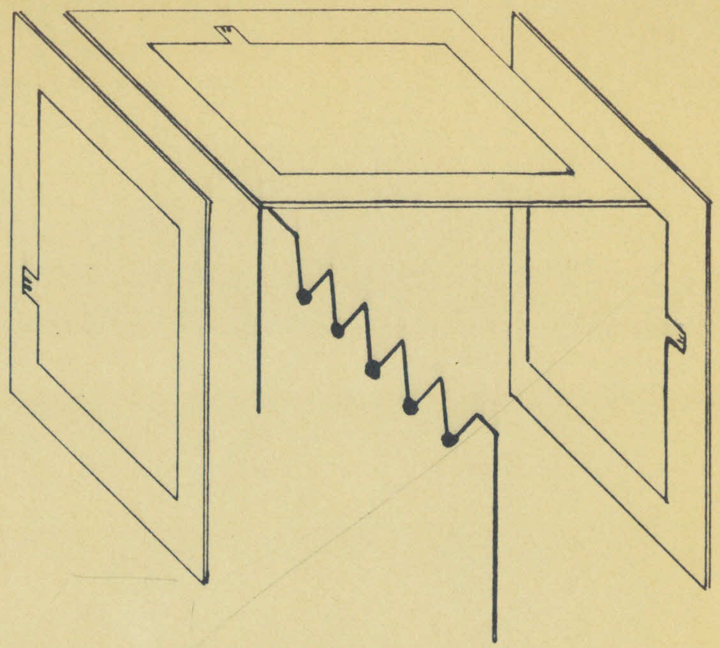
b. NARROW PLATE



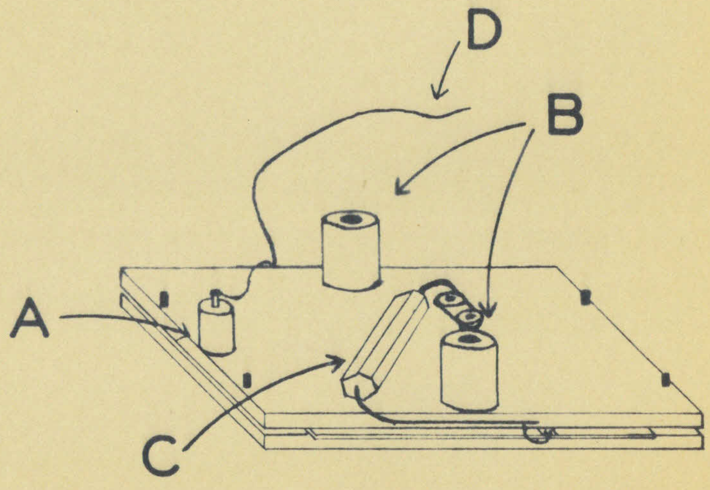
c. FINISHED CONDENSER

FIG. 1


CHANGE OF CAPACITY WITH TEMPERATURE
CONDENSER UNDER PRESSURE ○ FALLING TEMP. ● RISING TEMP.
CONDENSER NOT UNDER PRESSURE " " " "
CAPACITY IN ARBITRARY UNITS
47
48
49
50
51
52
30
20
10
0
-10
-20
-30
-40
-50
-60
-70
TEMPERATURE IN DEGREES CENTIGRADE
FIG. 2

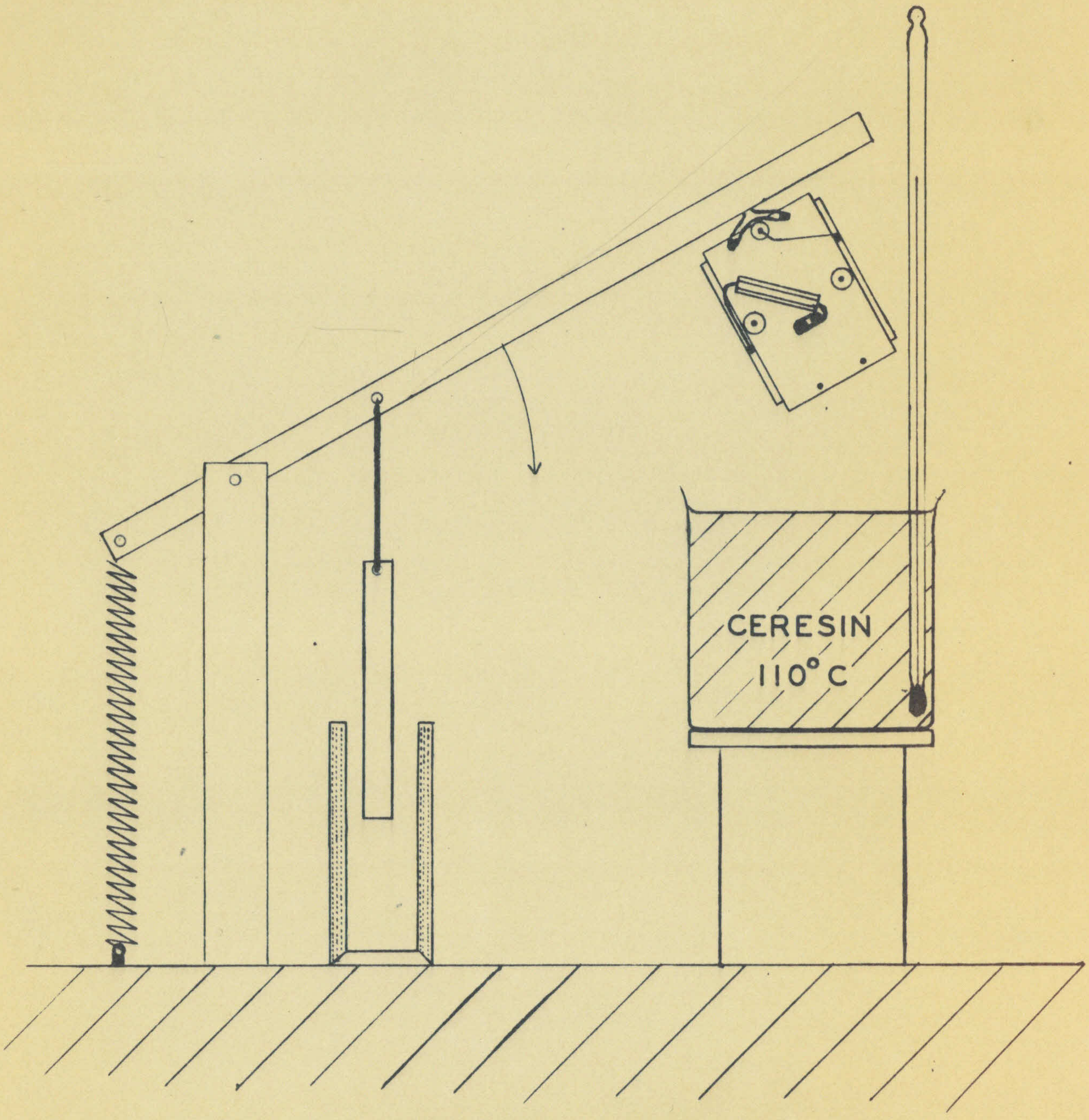


a. EVAPORATION



b. CONDENSER READY TO BE DIPPED

FIG. 3



DIPPING APPARATUS

FIG. 4

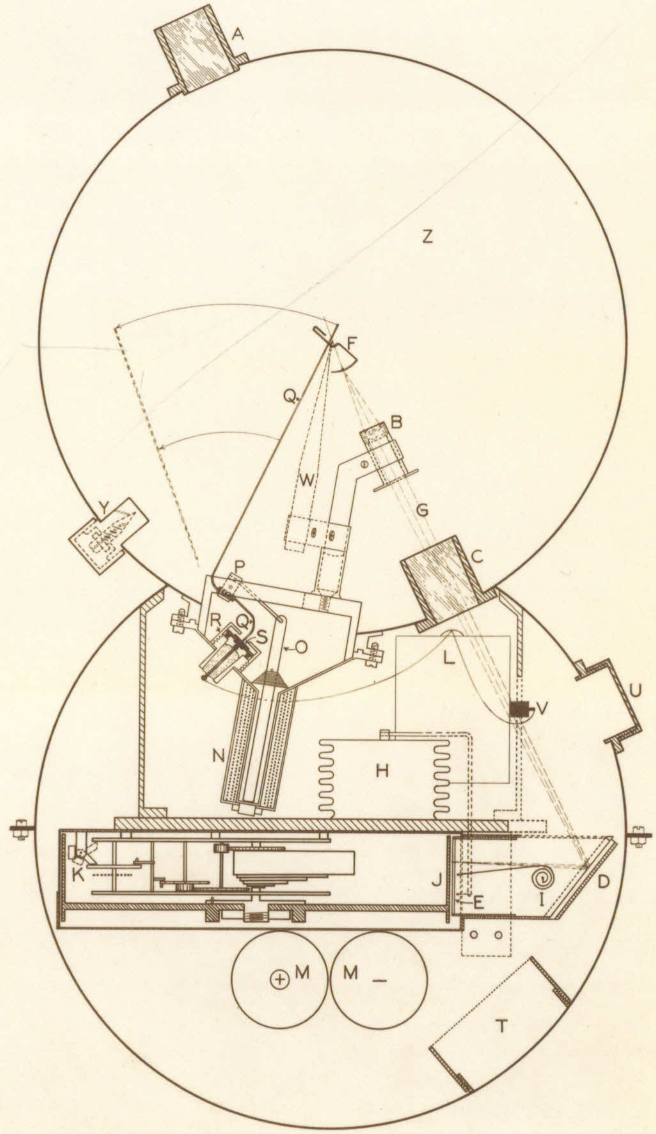
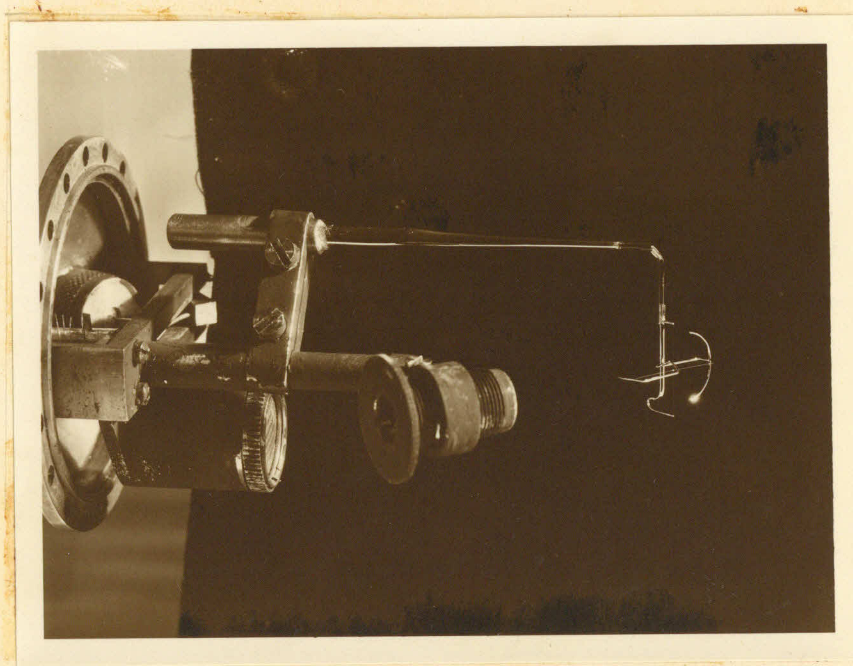


FIG. 5

FIG. 6



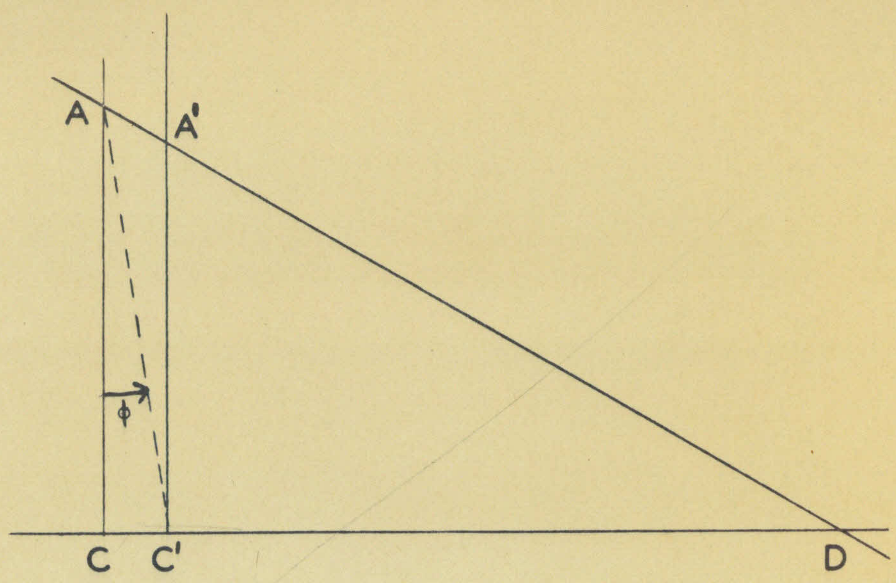


FIG. 8

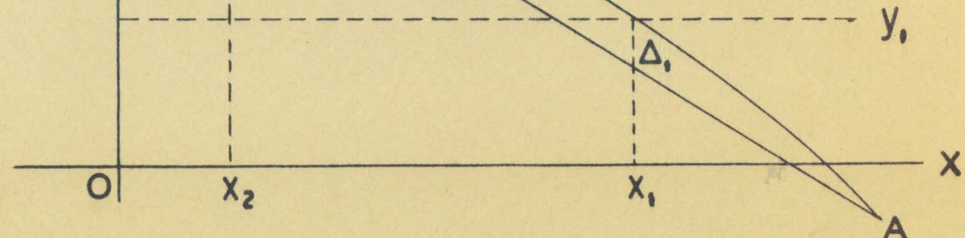


FIG. 9

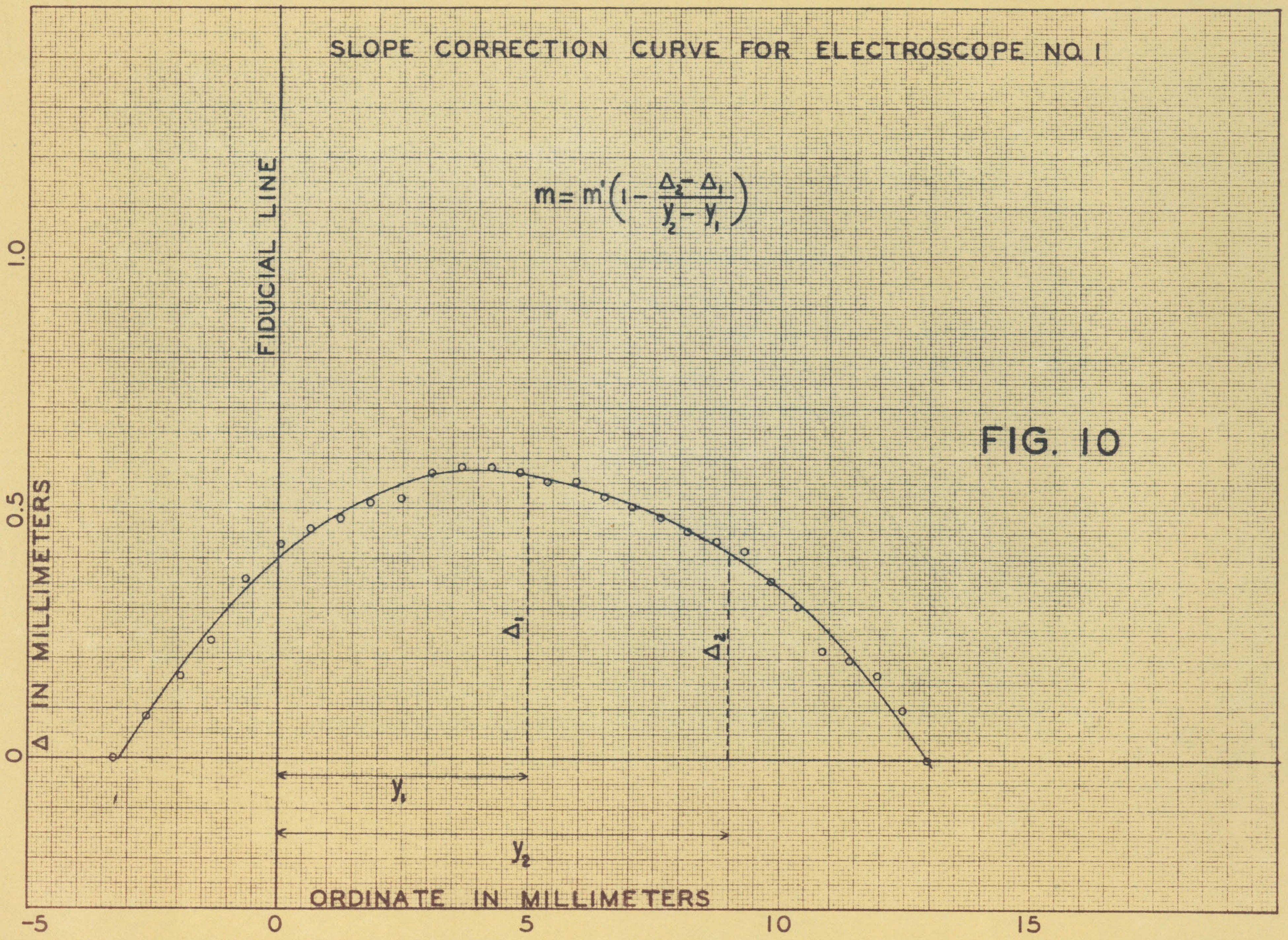

SLOPE CORRECTION CURVE FOR ELECTROSCOPE NO. 1
FIDUCIAL LINE
$m = m'\left(1 - \frac{\Delta_2 - \Delta_1}{y_2 - y_1}\right)$
Δ_1
Δ_2
y_1
y_2
Δ IN MILLIMETERS
ORDINATE IN MILLIMETERS
1.0
0.5
0
−5
0
5
10
15

FIG. 10

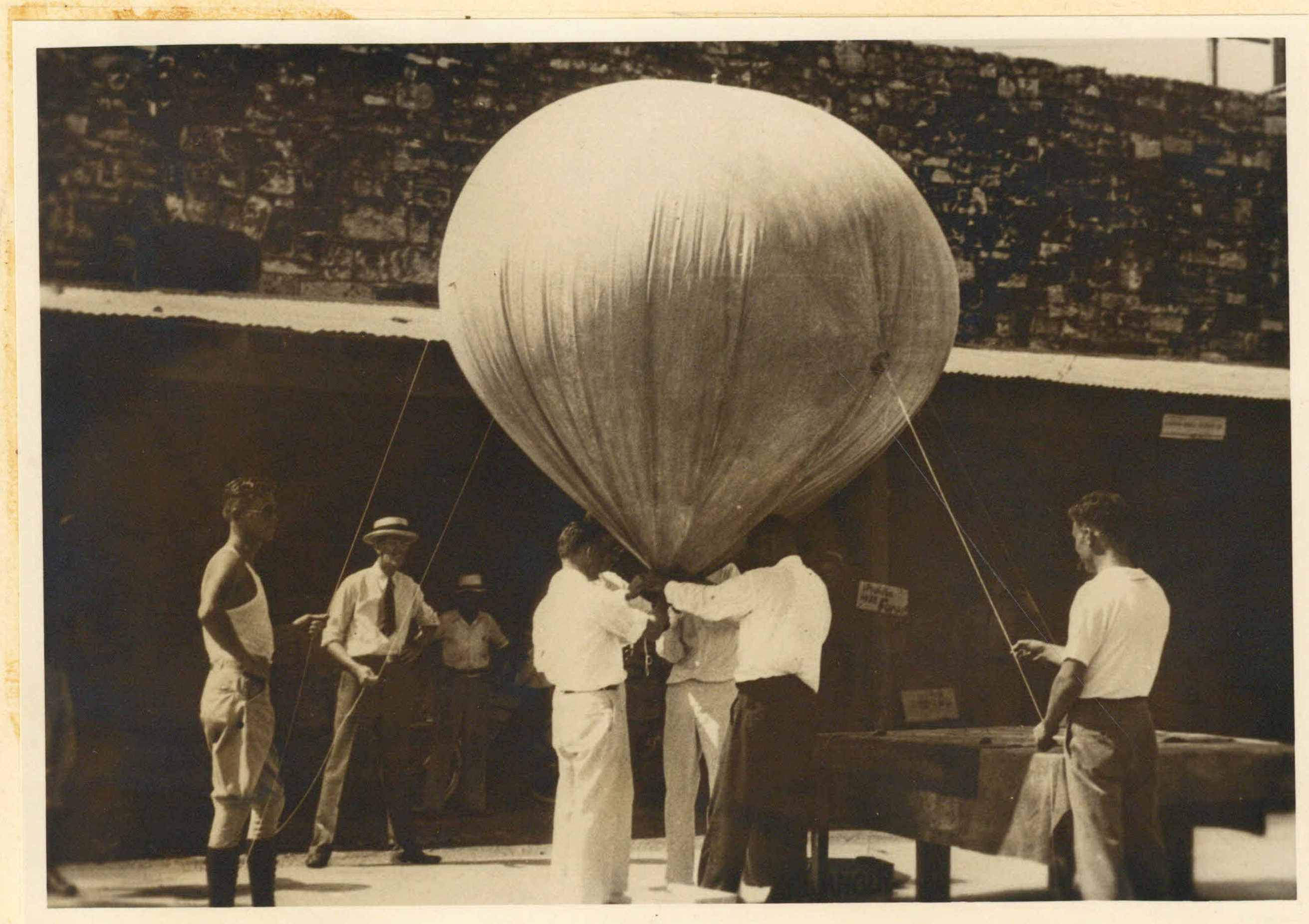
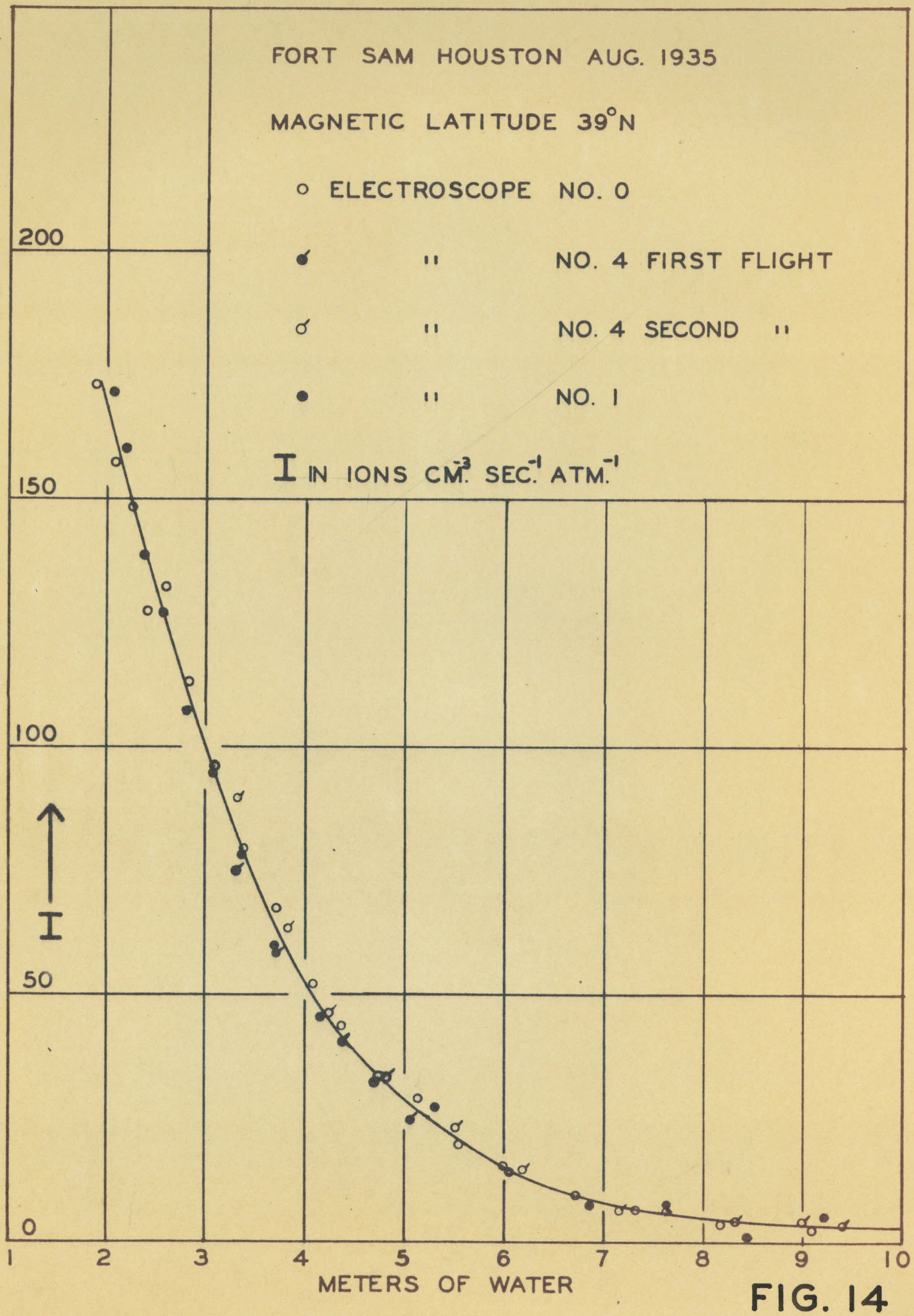


FIG. 11

FIG. 12

FIG. 13



FORT SAM HOUSTON AUG. 1935
MAGNETIC LATITUDE 39°N
o ELECTROSCOPE NO. 0
" NO. 4 FIRST FLIGHT
" NO. 4 SECOND "
" NO. 1
I IN IONS CM.⁻³ SEC.⁻¹ ATM.⁻¹
200
150
100
50
0
I
1
2
3
4
5
6
7
8
9
10
METERS OF WATER

FIG. 14

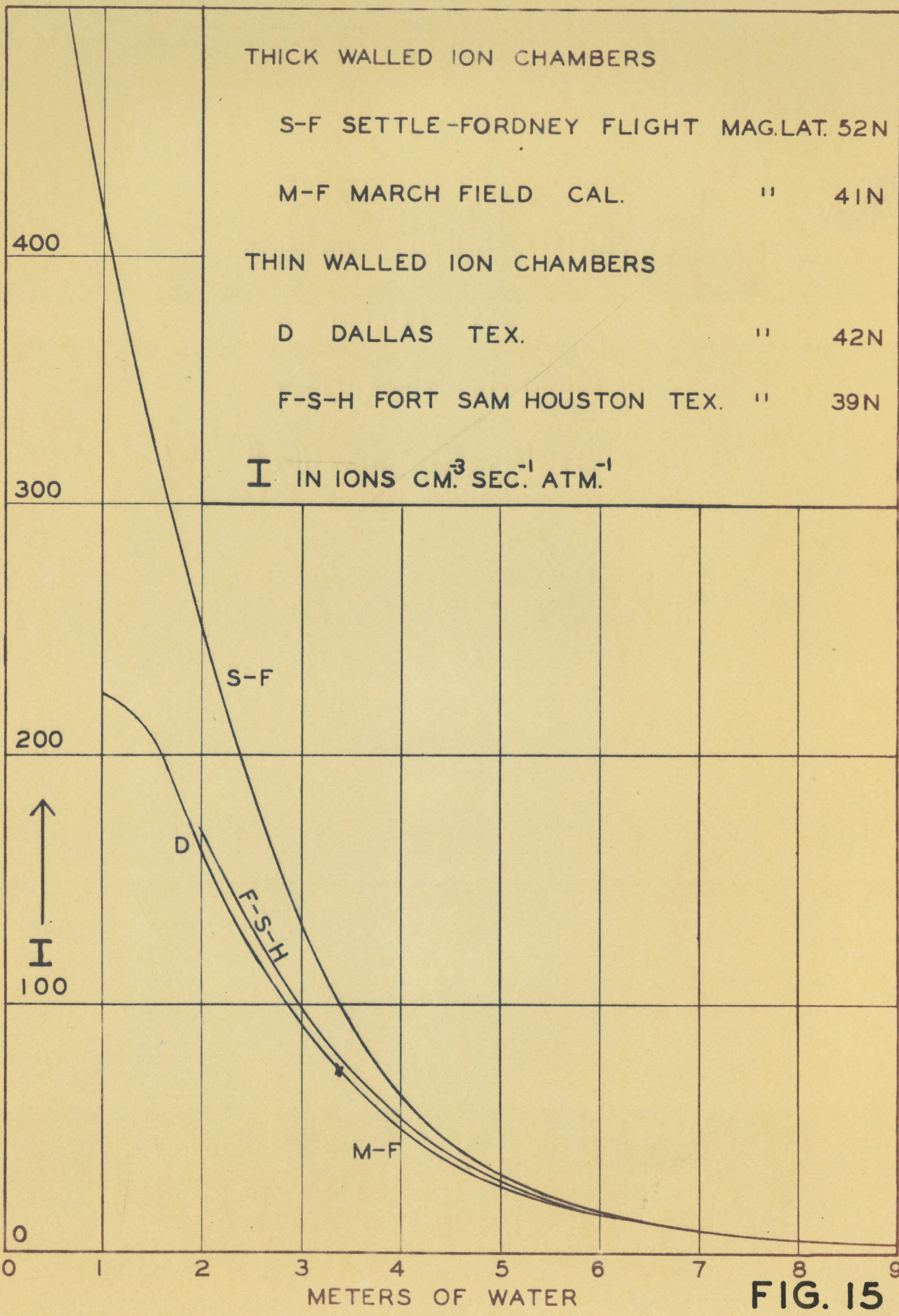

THICK WALLED ION CHAMBERS
S-F SETTLE-FORDNEY FLIGHT MAG.LAT. 52N
M-F MARCH FIELD CAL. " 41N
THIN WALLED ION CHAMBERS
D DALLAS TEX. " 42N
F-S-H FORT SAM HOUSTON TEX. " 39N
I IN IONS CM.$^{-3}$ SEC.$^{-1}$ ATM.$^{-1}$
400
300
200
100
0
I
S-F
D
F-S-H
M-F
0
1
2
3
4
5
6
7
8
9
METERS OF WATER

FIG. 15

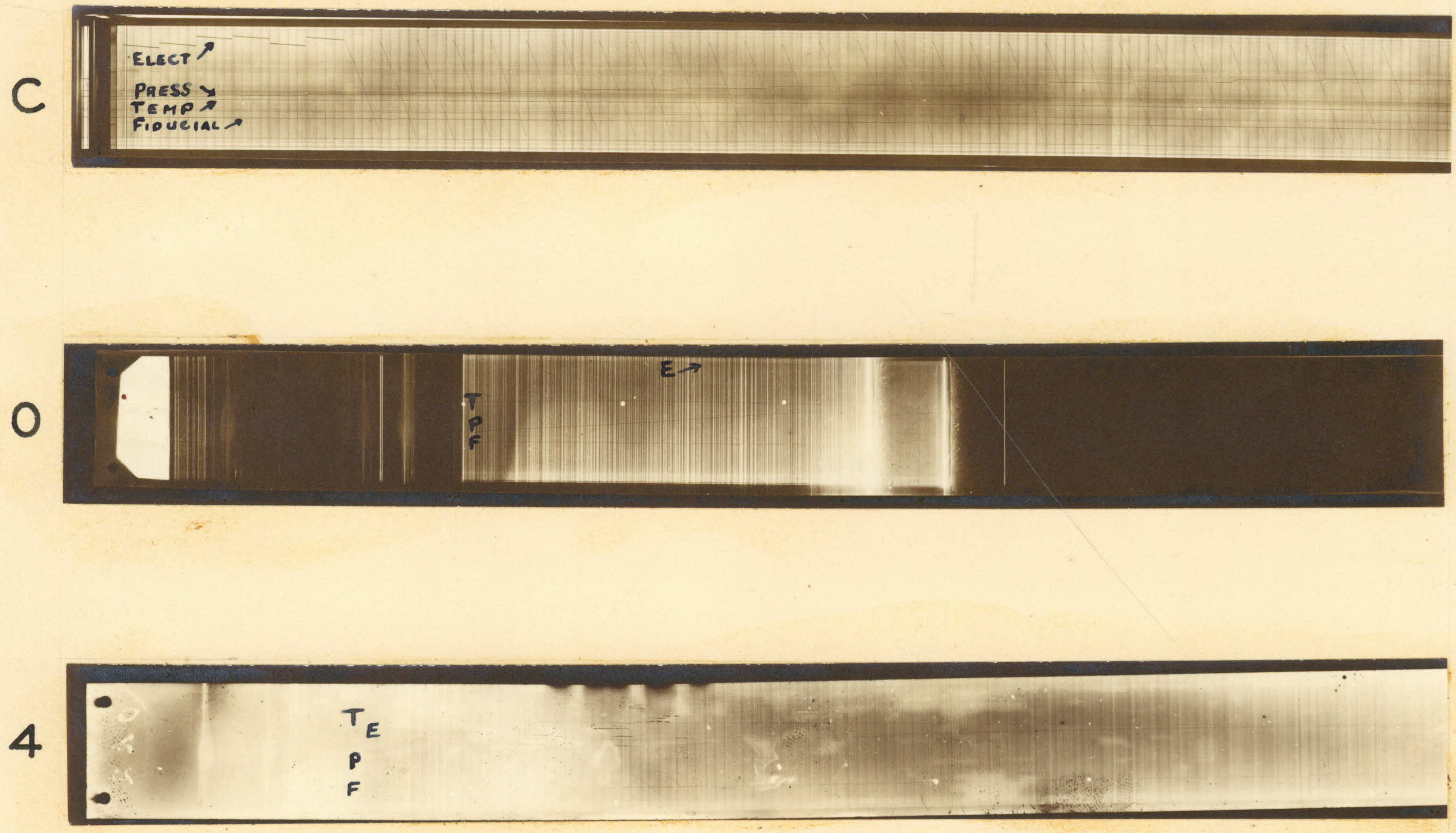


FIG. 16

4

1

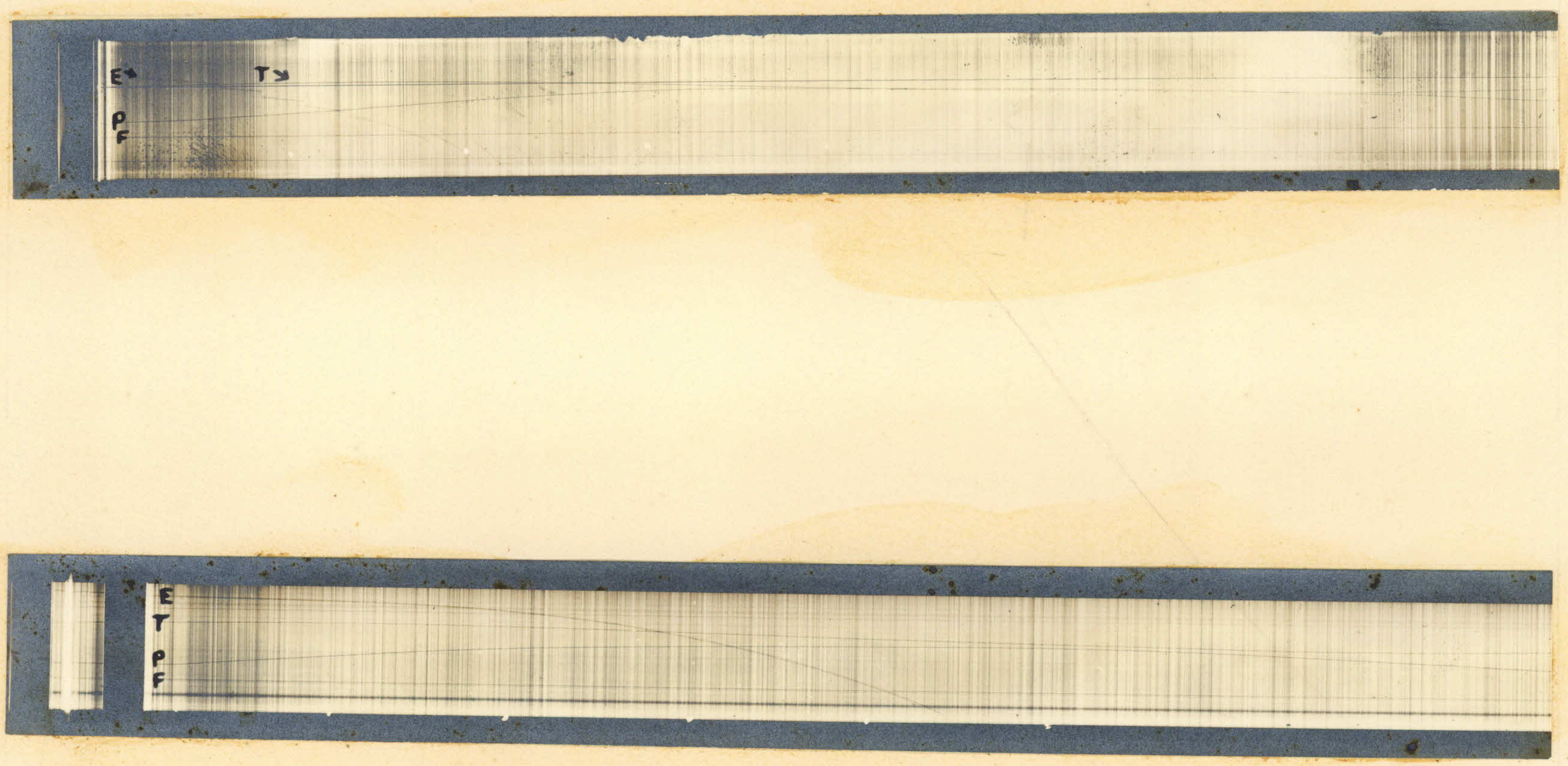


FIG. 17