NRAO Internal Report

STATUS REPORT ON 1 MILLIMETER PROJECT

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I. INTRODUCTION

The project for development of a reasonable and convenient 1 mm. observational capability for the 36-foot telescope at Kitt Peak, Arizona, has now been under way for four months. A number of interesting discoveries have been made concerning the limitations of the equipment previously developed by the Low group, certain *ad hoc* modifications have been made, and the two observational runs to date have produced some useful data. It is now possible to foresee with some clarity a specific program for development of a good system. Proposals and cost estimates for implementing this program will be outlined.

The contents of this report will continue with four parts, as follows:

II. Diagnosis of Problems With the Existing Equipment
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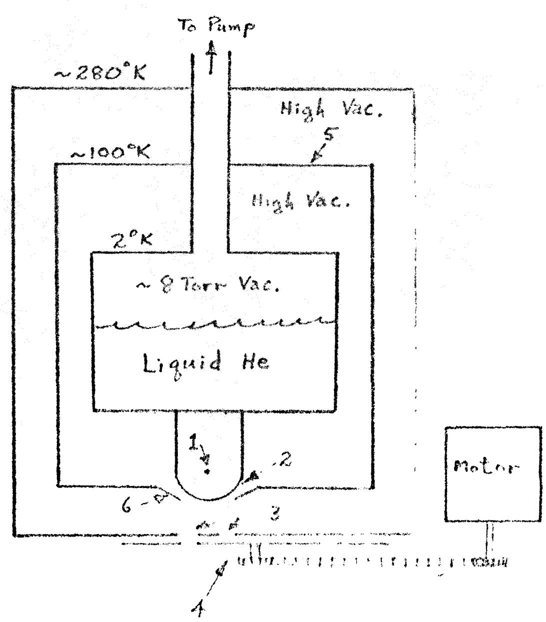
II. Diagnosis of Problems with the Existing Equipment

1. The Cryogenic Hardware.

The only experimental cryostat now in hand is approximately ten years old. It was originally fabricated by the Low group while they were at Texas Instruments, Inc. The length of the dewar chamber is significantly longer than that of contemporary versions, and its internal mechanical structure is relatively crude. Nevertheless, the basic thermal design seems to be sound since the holding time at 2°K under optimum conditions is ∿ 12 hours, which compares favorably with other dewars of similar helium capacity now available. Erratic behavior which has sometimes been experienced, such as premature loss of helium and thermal oscillations with a period of several seconds, is ascribable to damage or design errors in the bolometer head or other errors in procedural technique as will be discussed. Further problems due to slow leaks between the helium chamber and the isolating vacuum have been more vexing. In any event, the chamber bears much resemblance to an old war-horse, and it is my intention to re-design at least the outer casing and heat shield when modifications are made to permit operation of the equipment at a 45° tilt as will be described in part IV below.

The construction of the "business end" of the original apparatus elicits fewer kind remarks. The sketch below shows schematically the construction of the device as originally delivered to me:



The gold doped-germanium detector (1) is supported on an extension of the copper cold-work surface which also supports a sapphire hemispherical lens (2) placed 0.3 mm. in front of the detector. The function of the lens is to image the two fixed 2.5 mm. apertures (3) on the detector with a 3× reduction in image size, thus permitting a smaller (and theoretically quieter) detector. The apertures were permitted to view alternately the source and adjacent sky via a slotted chopper wheel driven by a no-slip belt and sprocket wheel (4). The heat shield (5) is supposed to prevent the 2°K helium chamber from seeing anything other than a reflection of itself except through the opening in the cone at (6). Pump-out of the high vacuum region inside the heat shield must occur by diffusion through small drill holes and cracks, particularly since the opening at (6) is now covered by a quartz window as will be discussed. Since the high vacuum region is normally closed off (no active pumping), any outgassing which occurs will tend to contaminate the volume between the helium chamber and the heat shield. As long as the dewar contains helium, the contaminants will tend to freeze out on the cold surface. If the system is permitted to warm up, however, and is then recooled, the contamination will add to convective heat transfer and accordingly reduce the hold-time of the dewar. It is thought that this phenomenon contributed to the troubles encountered at approximately three day intervals during the January, 1971, observing run. Considerable substantiating evidence was provided by a mass-spectrometer helium leak detector which showed substantial build up of helium gas each time the dewar underwent the unavoidable expansion and contraction associated with the refilling operation. If this problem persists replacement of the entire dewar may be necessary as it is most likely due to "burping" of helium through a submicroscopic weld pore.*

Another deficiency of the original equipment was related to the conical part of the heat shield (6). The part in question was made of .004" copper shim stock soldered to a .020" copper disk which was fastened to the heat shield with a

*Leak became catastrophic during March run. Repairs now being attempted.

circle of 12 brass bolts (one missing due to stripped thread). The bolt circle was not located on a precision circle, as can be seen in Photo #1. Apparently the centering of the cone was accomplished simply by a "guestimate" and by deforming the .004" shim stock. The resulting position was quite far off center as can be ascertained from Photo #2, which shows a redesigned cone with an adapter plate to produce a centered bolt circle. The off-center opening in previous runs may have contributed to problems of balancing the signal from the two apertures. Further experiments with a heat gun showed that differential expansion of the heat shield with respect to the lens mounting could bring the four bolt heads surrounding the lens (Photo #1) into contact with the .004" shim stock. Deep dents were observed in the shim stock at the concurrent positions. Although it is unlikely that the bolometer would have operated at all if contact had existed at 2°K, the heat loss which occurred during cool-down could have contributed to considerable helium loss, and possible transient contact could have produced some of the erratic behavior which has been sometimes observed in the past.

In view of these findings, the redesigned and properly centered cone shown in Photo #2 was installed prior to the January observing run. The further addition of a thermal short, consisting of a thin fused quartz plate across the cone opening, was not added until after the third day of the run to correct some unforseen difficulties which emerged in the form of ~ 5 second oscillations in the bolometer output and premature boil off of helium. The strip chart records show that the oscillation varied from 1 cycle per 3 seconds at 90° elevation angle to 1 cycle per 8 seconds at 30° with an approximate amplitude of 3°K. The radiation shield floats at some intermediate temperature between that of the helium container and the outer casing. Since the shield, no doubt, supports temperature gradients over its surface between its end walls, where the supports are located, and its side walls, the most likely explanation for the variable output is that a thermal relaxation oscillation was mechanically changing the bolometer's view of the outside world. The frequency dependance upon dewar tilt angle was most likely due to changing heat capacity of the copper work surface as the depth of the helium varied. It is interesting to note that at any elevation angle the amplitude of the oscillation grew much larger and the frequency lower in the half-hour just prior to exhaustion of the helium. In any event, the addition of the quartz window supposedly made the front of the heat shield more nearly isothermal and effectively eliminated the problem. I am indebted to Arnold Davidson for suggesting this solution.

2. Chopper Problems.

Previous observational attempts have been plagued with output oscillations due to defects in the chopper wheel mechanism. The difficulty was readily diagnosed by putting a strobe target on the motor shaft and comparing its motion to that of the chopper wheel by means of a General Radio Strobotac light. The chopper was wandering through angles of $\sim \pm 3°$ with a primary period proportional to the speed of the no-slip driving belt. The belt also exhibited vibrational modes. For these reasons, plus the obvious fact that different amounts of opacity in the reflective lands between the slots in the chopper wheel (due to dirt, scratches, etc.) will produce cyclical variations in the background, it was decided to dispense with the wheel altogether. A "taut-band" chopper made to my specifications by Bulova-American Time Corp. was substituted. This device is a driven, resonant pendulum which operates on the same principle as the Accutron watch made by the same company. Reference to Photo #4 will show that the chopper blade, supported by a lever, is attached to the center point of a taut band which lies approximately along the line of sight. The natural frequency of the system is determined by the moment of the chopper blade and the torsional restoring force of the taut band. Also attached at the center point is a transverse arm which can be seen to support a small permanent magnet at each end. One magnet induces a current in a proximal coil. The current is amplified by an external amplifier and positively fed back to an identical coil which serves as a driver near the other magnet. The pendulum is, thus, a very high-Q mechanical filter. The frequency and amplitude of the motion have proven to be stable to better than 0.01% at the operating frequency of 13 Hz, provided that the taut-band always remains in the elevation plane of the telescope. Slight assymmetry of the waveform results if the loading of the chopper blade is unsymmetrical with respect to gravity, but it is easily possible to avoid this problem so long as detectors are used which are not polarization-sensitive and hence do not require rotation of the Stirling mount. Finally, it is clear that the back of the chopper blade presents the same aspect to both beams during every cycle, unlike the chopper wheel.

3. Focus and Balance.

Among the most obvious difficiencies of the old system was the complete absence of any means for adjusting any of the system parameters except by "cut and try". It was never possible to balance the main and reference beams properly. There was no provision for focussing the detector lens on the apertures even though the heat gun experiments previously mentioned clearly showed large changes of internal geometry.

The focussing problem was solved pragmatically by making the entire end of the dewar movable. Photos #3 and 5 show disassembled and assembled views of the equipment. It can be seen that the end of the chamber has been made into a sliding piston with an O-ring seal. Adjustment of the knurled ring permits varying of the lens-to-outside-aperture distance over a range of 20 millimeters. During the January run the focus distance was optimized by simply adjusting for maximum bolometer signal with the equipment looking at the laboratory background. New modifications now permit remote adjustment of the focus with the equipment mounted on the telescope. The end can be moved by motor driven screws rather than by turning the knurled ring as shown in Photo #6.

The value of this capability was demonstrated during the March observational run. An enhancement of 35% in signal strength was obtained by focussing the dewar to the inmost position then available, which was 5 mm. inside of the previously fixed position. During the next run the equipment will be readjusted to permit even shorter focal distances.

The balancing problem was more difficult. The solution which has been adopted utilizes a variable iris to make the effective aperture of the reference beam opening equal to that of the main beam opening. This is accomplished by having the main beam opening located on the center axis line of the lens and the reference beam opening centered 5 mm. off axis.* The reference beam hole has a diameter of 4 mm., while the main beam opening is 3 mm. in diameter. This provides for the decrease in lens efficiency off the lens axis. The actual aperture of the reference opening can be varied by opening and closing a camera iris which is in contact with the under side of the opening. The small departure from roundness of the reference beam hole is believed to be of no significance since the two beams still traverse the same air mass within the atmosphere.

The ten-leaf steel camera iris was cannibalized from an old camera. The leaves were removed from the assembly and coated by vacuum evaporation of copper followed by gold to obtain an inert and highly reflective surface. A gear was machined to permit the iris control ring to be precisely moved by a miniature Itec pulse motor (Photo #7). Each pulse produces 15° motor shaft rotation which is reduced to 0.35° at the iris. The iris is driven from fully closed ($\sim$0.5 mm. diameter aperture) to fully open ($\sim$18 mm.) in 150 pulses. A Monsano digital counter remotely monitors the iris opening and permits it to be exactly reset to any position.

Figure 4 illustrates the operation of the iris. (a) and (b) show the chopper alternately uncovering the main and reference beams. (c) shows an Ecco-Bond "hot load" at ambient temperature swung into position over the main beam by a rotary solenoid. (d) shows how the iris can be closed

*This corresponds to a beam separation of 110" of arc. The actual separation varies slightly with the balance adjustment and is usually 107" ± 2".

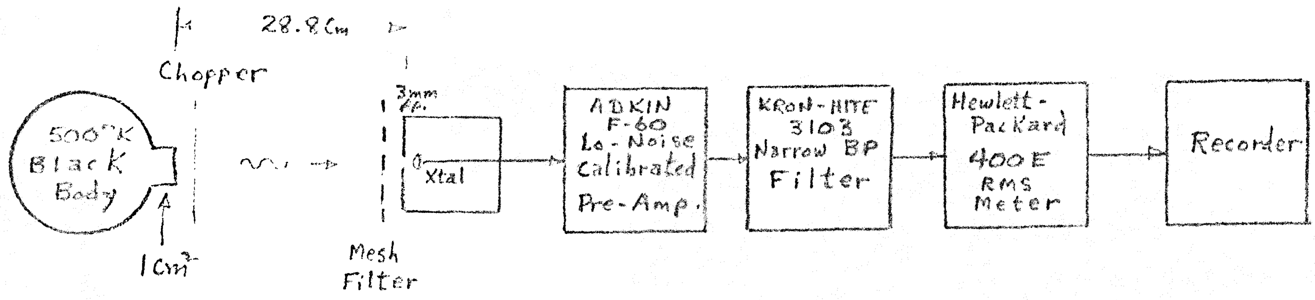
so that the main beam can be switched against the closed reference beam, which reflects the 2°K radiation back into the dewar. In the latter configuration the hot load, of course, can be inserted as shown or withdrawn to permit the main beam to see the sky.

Operation of the balancing scheme seems to be entirely satisfactory. The synchronous detector signal can be balanced to zero at the front end, and variable opacity fluctuations are reduced by at least 70%. Quantitative measure of the sky noise reduction will be improved after the filter modifications to be discussed below. At present the broad response of the filter apparently averages the sky fluctuations so that they are frequently smaller than the system noise even in the unbalanced mode. The effect of the balancing can be best seen when clouds are moving in front of the sun. Even though the atmosphere appears as an isothermal bath and the optical depth is greater than 1, the changes of opacity produce changes of many degrees in the load switched signal which disappear in the balanced mode.

4. Filtering and Detector Sensitivity.

The most perplexing findings relevant to the equipment inherited from the Low group concern the bandwidth and sensitivity of the detector itself. The unexpectedly high observed temperature of the atmosphere even during dry periods led to a suspicion that the bandpass includes local sources of high opacity. This motivated experiments to evaluate the spectral response and sensitivity. Detailed measurements of spectral response are possible but very expensive and time consuming due to the special equipment which is required. The usual method employs a black body source fed into an interferometer to yield a monochromatic line which is then tuned across the passband of the detector. Long integrations are necessary at each measured wavelength. A cruder but nevertheless informative technique utilizes high-pass filters to truncate a black body source. This method was used to determine the response of our system.

The experimental setup was arranged as shown :



An Advanced Kinetics Corp. Black Body (ε = 0.999) with its own chopper was employed. The bolometer iris was set for single beam operation with the full 3 mm. opening. As viewed from the 3 mm. aperture, the 1 cm. diameter black body aperture subtends an angle of:

$$\Delta\Omega = \frac{\frac{\pi}{4}}{(28.8)^2} = 0.95 \text{ millisteradians} \quad .$$

The access to the detector has an area of:

$$A = \frac{\pi \cdot (0.3)^2}{4} = 0.0707 \text{ cm}^2 \; .$$

Hence, the surface angle of the source at the detector is:

$$A\Delta\Omega = 6.68 \cdot 10^{-5} \text{ cm}^2 \text{ sterad.}$$

The proceedure is, then, as follows:

(1) Select the Spectrum Interval with standard high-pass mesh filters:

(a) Lower limit λ_0 selected to be 50 μ because transmission of sapphire $\simeq$ 0.5% maximum for $E_{\parallel}$ optic axis;

(b) Upper limit selected to correspond to a 96% reflectivity of metallic grid (λ/g = 3).

(2) Determine Power Transmitted for each spectrum interval:

(a) Obtain $W \cdot A \cdot \Delta\Omega$ for each interval;

(b) Correct for transmission losses:

i) Obscuration factor due to grid wires (labelled c_1).

$$F = 4\left(\frac{a}{g} - \left(\frac{a}{g}\right)^2\right) \qquad \begin{array}{l} 2a = \text{grid wire diameter} \\ g = \text{spacing} \end{array}$$

ii) Transmission through sapphire weighted for each interval (c_2). This is difficult to estimate with accuracy because of the substantial thickness of the element. Values have been adopted for the transmission which give the existing system the benefit of the doubt at all wavelengths.

iii) Include further factor of 2 for losses in the two quartz windows.

iv) Obtain net power at the detector.

The filtering scheme employed by Low is described as follows:*

(1) A 0.004" thick cardboard filter which "has high absorption in the 0.4 to 0.8 mm. region but virtually no loss in the 1.2 mm. window."

(2) "A 1/4 lambda coating of NaCl on the lens." (Actually appears to be KBr, approximately 1 1/2 lambda.) "This coating serves both as a (sic) anti-reflection coating at 1 mm. and a blocking filter for the 40 to 400 micron region. The sapphire also serves as a blocking filter for the 5 to 40 micron region."

(3) "Sandwiched between the sapphire lens and the sapphire spacer is an annular ring of 0.001" aluminum foil which serves both as an interior baffle and a spacer ring for a 0.001" coating of '3M Black Velvet Coating.' This coating completely blocks the visible to 5 micron region.

"In the above explanation, all filter cut-on/cut-off points are not sharp and well-defined, but overlap each other; however, the total effect is almost complete rejection of all energy short of 0.8 mm.

"Wavelengths above 1.6 mm. are not completely rejected; however, system design parameters are optimized for the 1.2 mm. window and efficiency falls off rapidly above 1.6 mm."

So far as I can determine, no actual measurements were ever made on this particular instrument to substantiate the above filter specifications. I was told that the cardboard was added as an afterthought because it "quieted down the background."

In the recent experiments three cardboard samples were first evaluated separately. The results are given in Table 1. (The mesh filters which determine the "narrow-band" transmission

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*Quotations in this section are from the Infrared Laboratories, Inc. Instruction Manual to the 1 mm. Bolometer System.

in all of the calibrations were selected solely on the basis of availability.) The overall responsivity data for the system is given in Table 2. Although it is unfortunate that another point could not be obtained in the vicinity of 800 μm, it is obvious that the response of the detector extends liberally into the opaque regions above and below the desired 1.0 to 1.6 mm. passband. Graph 1 shows the most charitable data interpretation.

The question of detector sensitivity is even more difficult and more embarassing. A direct measurement of bolometer output as a function of output network bandwidth is given in Graph 2. Here an amplifier of calibrated gain has been used to measure directly the output noise per $\sqrt{Hz}$, where the bolometer iris has been blocked off by the chopper blade taped over the hole. The usual measure of detector sensitivity is the Noise Equivalent Power (N. E. P.). In this case we obtained the N.E.P. by dividing the graph value for $\sim 2°$ and 13 Hz by the measured responsivity given in Table 2.

$$\text{N.E.P.} = \frac{\sim 1.4 \times 10^{-5}\ \text{V}/\sqrt{\text{Hz}}}{1.4 \times 10^{3}\ \text{V/watt}} = 10^{-8}\ \text{watt}/\sqrt{\text{Hz}}$$

This is five orders of magnitude worse than the N.E.P. advertized by Infrared Laboratories! --This disparity is largely responsible for the tardiness of this report, as I have tried very carefully to understand this huge difference.

The most obvious single factor to which we might appeal is that the noise power is proportional to T^4. If the effective temperature of the detector is a factor or 10 to 30 higher than the 2°K temperature of the helium, the difference is explained outright. In fact, however, a difference of only a very few degrees may be adequate to explain the discrepancy for the following reason: the responsivity used by Infrared Laboratories in specifying their sensitivity is determined from a load curve giving voltage across the bolometer as a function of current. The value thus obtained is given in the manual as 7×10^5 V/W, about 500 times the directly measured value given in Table 2. I have repeated the load measurement, and obtained substantially the same result. Responsivity determined in this way has little to do with the response to an external power input. A major problem of semiconductor detectors at 1 mm. is surface reflection. If only 10% of the incident power actually heats the crystal, then a factor of 10 in temperature makes the remaining 10^4 difference. If only 1% penetrates, a factor of 3.2 in temperature suffices.

In summary, I feel that the new data is probably in error by a factor of 2 or 3, and certainly no more than a factor of 10. If we take the worst case, the responsivity may differ from Infrared Lab's by a factor of 5000. Then a factor of only a couple of degrees in effective detector temperature will account for the remaining difference. In view of the fact that the detector is situated on a long protruberance about 5 cm. out from the cold work surface and less than 1 cm. from the heat shield, which is floating at $\sim$ 100°K, it is not surprising that a couple of degrees difference may accrue in the equilibrium situation. Much more work remains to be done to prove this, however. Hopefully it will be possible to fix the problem without resorting to a rigorous proof!

5. Helium Filling and Pumping.

During the January run I was appalled by all aspects of the helium filling proceedure. Not only was the method dangerous for both men and equipment, it required a total turn-around time of about 2 1/2 hours twice a day. Furthermore, since several failures of various sorts occurred, we spent perhaps the better part of six hours per day dangling in precarious positions some 70 feet above the floor in a cold wind. For this reason, I have devoted most of the past month to the development of a quick servicing scheme which permits use of the normal service tower and the elevator at a similar height.

The basic problem is that the dewar cannot be tipped more than $\sim$ 45° out of the vertical without loss of helium when full. The new device solves this problem by extending the dewar on gimbals so that it hangs vertical under its own weight. After filling, the telescope is simply run up to

90° elevation. An air cylinder is then actuated which pulls the dewar home to three conical locators which return it to the same position within .001" every time. If it becomes necessary to service the dewar at any time, the telescope is returned briefly to 90° elevation and the mechanism is extended. The turn-around time during the March run just completed was reduced to 1 1/2 hours, most of which was spent in pump-down after filling. Further improvement is probable with practice.

Photos #8 and #9 depict the operation of the extension device while bolted to the wooden ring of its new service cart. The photos also show how the service cart is used as a cryogenic work bench. The pumping manifold and an auxilliary vacuum pump for lab use are located under the top table. The receiver mount can be tipped about the swivels at the left end of the cart to stand it stably in the vertical position off the end of the cart. The cart will also soon be equipped with an oil diffusion pump obtained as a gift from the Kitt Peak optics shop and a high vacuum ion gauge to be purchased. Our operations will then be completely autonomous except for the continuing need for a mass spectrograph helium leak detector, which has been readily available on loan from the Kitt Peak space lab.

6. Use of TV to Observe Feed Support.

The COHU television camera which was formerly used with the 300-foot dish has been in Tucson for some time. I decided to put it to use to help evaluate the stability of the feed support. The TV also is useful for verifying correct operation of the chopper, iris and hot load calibrator and for checking for proper retraction of the dewar after servicing. (It is also reassuring to observe that the feed support is still there!)

An adjustable adapter for mounting the camera to the theodolite mount in the dish vertex was constructed with the help of Omar Bowyer in the Green Bank shop. A Questar telescope was borrowed from the Kitt Peak Solar Division and adapted to the camera. The resolution on the TV screen is so good that deflections of 0.3 mm. at the feed support can easily be observed. Even better resolution may be possible by analyzing a single horizontal scan line traversing the image of a millimeter ruler by means of a time delay oscilloscope. To date the principle observations are that a 20 mile-per-hour wind produces RMS excursions of about 3 millimeters (or $\sim$ 1 beamwidth) in the feed support and that similar excursions occur when the telescope is shifted between main and comparison beams. In the latter case the damping time is about 10 seconds. Very precise absolute deflections will be measurable when a set of short guy wires now in preparation is added to the camera support.

Table 1

RESULTS ON CARDBOARD

Broadband Transmission of Cardboard Discs

(Looking at 500°K Blackbody, into Bolometer Passband)

Cardboard #1	0.006 inch thick, grey	0.55 ± 0.05
Cardboard #2	0.009 inch thick, grey	0.52 ± 0.05
Cardboard #3	0.008 inch thick, yellow	0.45 ± 0.05

Narrowband transmission of Cardboard

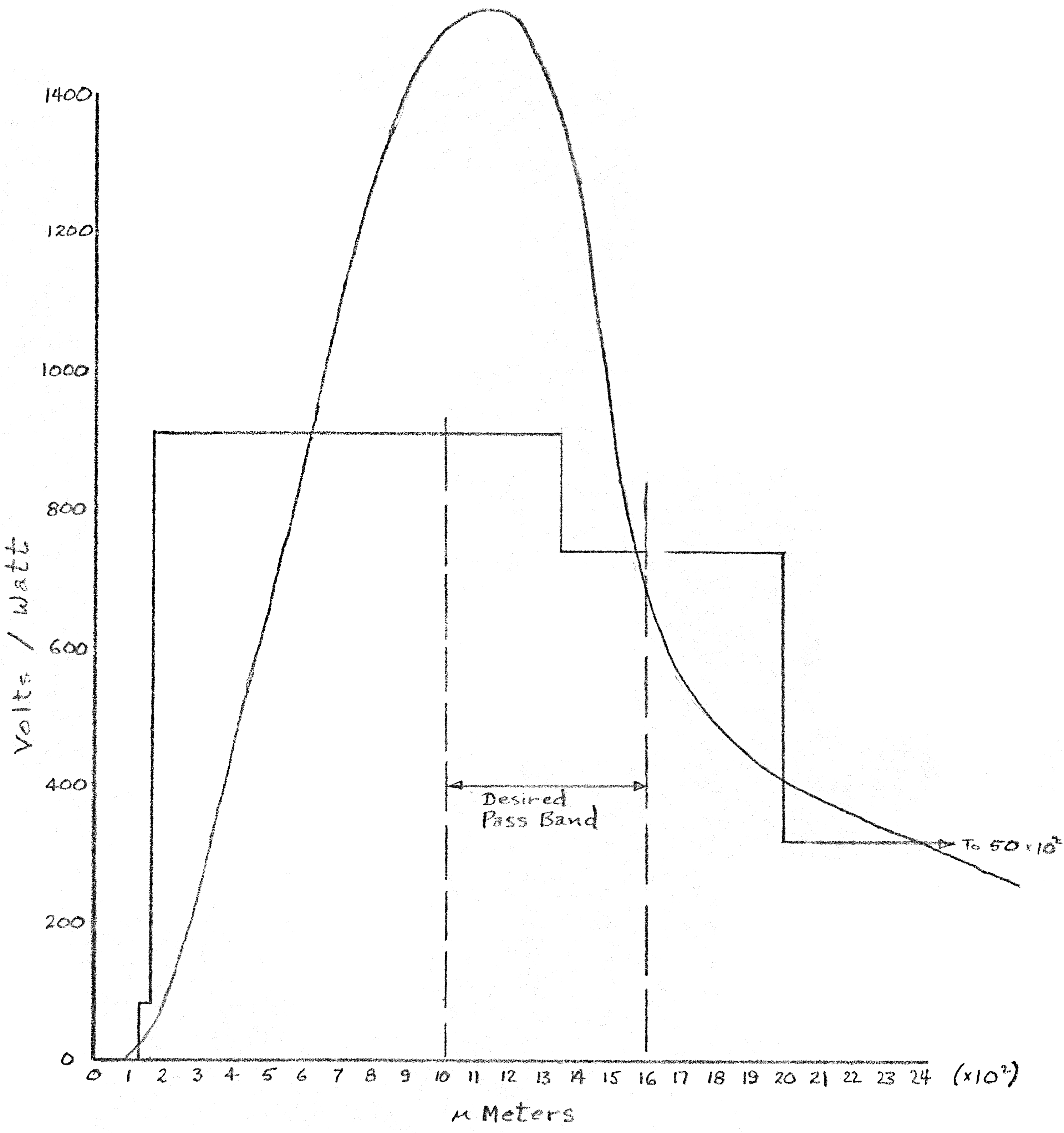
50 - 130 μm	0.28 ± 0.06
50 - 160 μm	0.29 ± 0.06
50 - 1350 μm	0.49 ± 0.05
50 - 2000 μm	0.52 ± 0.05
50 - 5000 μm	0.46 ± 0.05

Table 2

RESPONSIVITY DATA

	λ_0 (μm)	λ (μm)	AΩW (nw)	C_1	C_2	C_3	Power at Detector (nw)	Detected Signal (nv)	Responsivity (v/w)	Detected Signal w/cardb. (nv)	$\frac{nv}{\mu m}$
	50	130	22	3.1	40	2	0.09	870	9666	246	3.07
	50	160	37	2.3	30	2	0.27	900	3332	261	2.37
	50	1350	60	3.1	8	2	1.2	2250	1874	1110	0.85
	50	2000	62	2.5	6	2	2.05	3360	1638	1740	0.89
	50	5000	63	1.95	6	2	2.7	4290	1600	1950	0.39
Differential	130	160					0.18	30	170 (85)*	15	0.5
	160	1350					0.93	1350	1450 (910)	849	0.713
	1350	2000					0.85	1110	1300 (740)	630	0.969
	2000	5000					0.65	930	1430 (320)	210	0.07

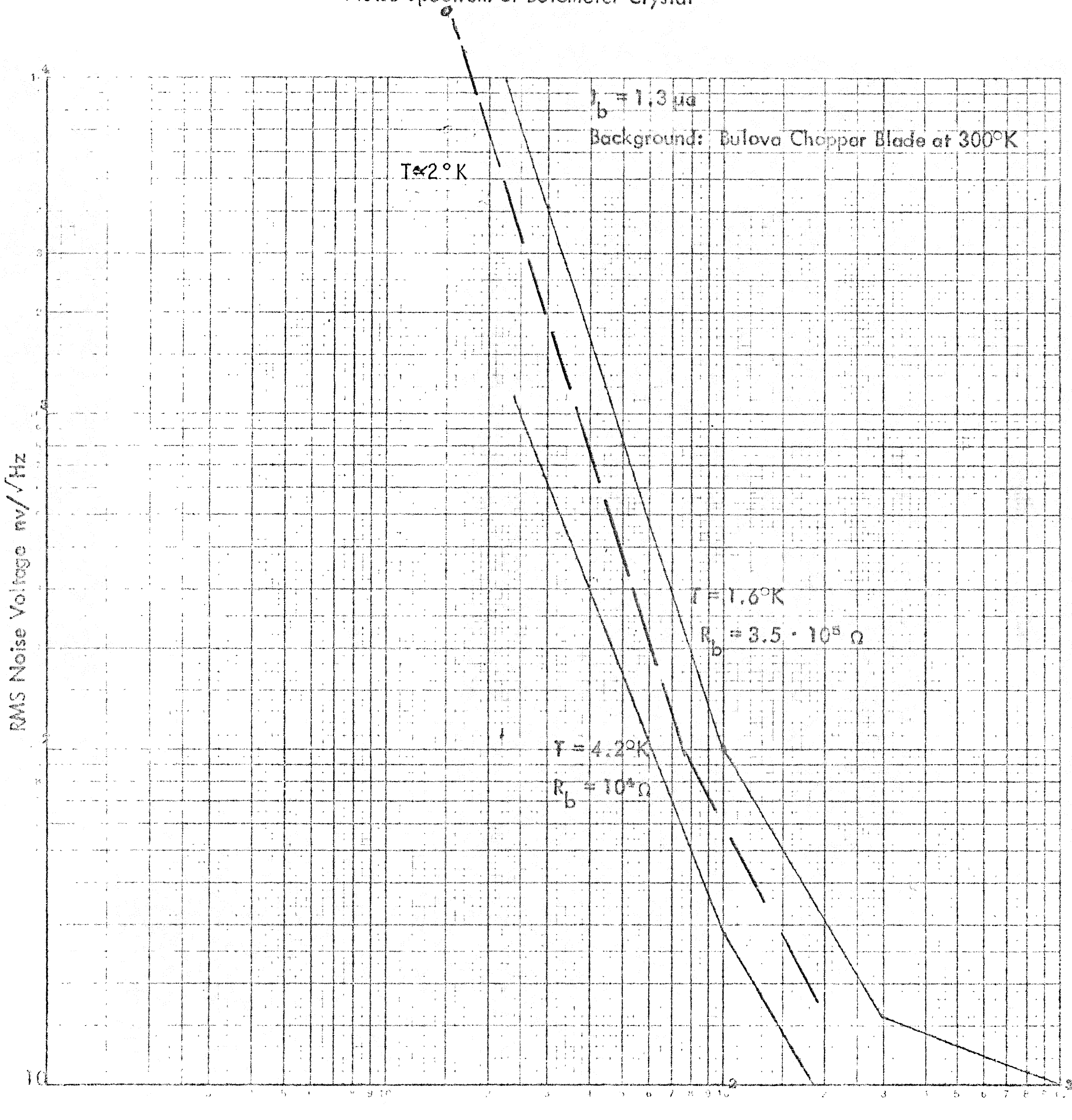
*Derived differential responsivity with cardboard indicated in parentheses.



Graph 1. Differential responsivity with cardboard filters included. Blocks indicate measured values. Curve indicates a hypothetical most charitable interpretation of the data.

Graph 2.

Noise Spectrum of Bolometer Crystal



III. Results of Observations

In addition to providing observational data and evidence for evaluating the 36-foot telescope behavior at 1 mm., the time spent on the telescope has been particularly valuable in teaching me the necessities regarding the maintenance of the liquid helium and other problems of changing and servicing the receiver in an operational situation. The two runs have showed that the behavior of the dewar differs markedly from its bench performance when it is kept cold for days at a time and spends most of its time tipped considerably from the vertical. Telescope time was also essential in understanding the deficiencies of the filtering system because the atmospheric tipping experiments which revealed the problem require reasonably good angular resolution. Likewise, the balancing system depends for its evaluation upon a narrow atmospheric path as defined by the telescope beam.

The January run is summarized in considerable detail on the following page. It can be seen that the conspiracy between the weather, equipment problems and my own ignorance was about 50% effective! Nevertheless, quite a bit of useful information was obtained.

The four-day March run last week was also truncated to two days by equipment failure: the very small helium leak which has previously poisoned the dewar vacuum on a three or four day time scale (see p. 3) became suddenly worse during the warm-up at the end of the fourth helium depletion. At room temperature the leak is so small that I have located it today by using the mass-spectrometer detector only with the greatest difficulty. At 2°K, however, the superfluid helium flowed through on a wholesale basis, making it impossible to fill the dewar. I hope that the worst gremlin that has plagued the old cryostat has now been found out. The positive results of the March run are not to be ignored: detailed observations of Jupiter show that we still have a 1 millimeter telescope _post accidentis_. Dr. G. Feix obtained a good map

RATHER

BALANCE VS. ELEVATION ANGLE
ATTEMPTED OBS. 3C273
OBSERVED MOON
POINTING (MOON)

SEARCHING FOR SATURN

CALIBRATION STUDIES
HEATING OF ECCO-BOND
BALANCE VS IRIS OPENING
SKY TEMP. VS ZENITH ANGLE

SEARCH FOR SATURN THROUGH CLOUDS
MAPPED ORION B REGION
SEARCH FOR JUPITER AND VENUS THROUGH CLOUDS.
NO SUCCESS.

SEARCH FOR SATURN

NO OBSERVATIONS
DEWAR IN TUCSON

SATURN FINALLY VISIBLE!
POINTING ON SATURN:
MAIN BEAM +40", S15"
REF. BEAM -1'20", S15"
BEAM SEP. 2'00"
SCANS 44-50: 0J287
51-60: 3C273
60-65: URANUS
POINTING ON JUPITER:
MAIN +35" S18"
REF. -1'12" S09"
SEP. 1'47"

CALIBRATION RUN ON SATURN.
SCANS 75-84.

NO OBSERVATIONS. NIGHT TRADED WITH CONKLIN FOR OPTICAL POINTING.

BEST OBSERVATIONS.
SCANS 85-99: 0J287
SCANS 100-109: 3C273
(DETECTED WITH S/N = 2/1)
SCANS 110-115: JUPITER
(S/N = 38/1)
GOOD CALIBRATION DATA.

SIMON

OPTIMUM FOCUS FROM SUN
SCANS = 620.
POINTING CORRECTIONS:
AZ = +25", EL = S 12"
SUN/SKY = ~ 1.7

POINTING: AZ 40", EL S 17"
SKY TEMP. VS. EL . ODDLY,
MIN. AT EL = 60°
SUN/SKY ~ 1.5
SUN & SKY OSCILLATION RUNS

SIMON AND RATHER

SUN OBSERVATIONS THRU CLOUDS
DETERMINED ATT'N THRU DOME:
SUN(OPEN)/SUN(CLOSED) = 1.8
OPTICAL TELESCOPE OFFSET FROM 1 MM. BEAM

NO OBSERVATIONS.
DEWAR IN TUCSON.

OBTAINED 1 MM. SUN MAP

NO OBSERVATIONS.
DAY WAS USED FOR MAINTENANCE.

NO OBSERVATIONS
DEWAR IN TUCSON

POINTING: AZ +1', EL S 15"
ATTEMPTED MOON/SUN RATIOS

EQUIPMENT NOTES

INSTALLED 1 MM. EQUIPMENT

DEWAR FILL

DEWAR FILL

COMPUTER FAILURE
COULD NOT TRACK

DEWAR FILL

DEWAR FILL

DEWAR FILL

DEWAR FILL

DEWAR PREMATURELY EMPTY (REFILLED)

DEWAR EMPTY AGAIN

TOOK DEWAR TO TUCSON FOR EXPLORATORY SURGERY. INSERTED QUARTZ ISOLATOR AT HEAT SHIELD OPENING. THIS CURED THERMAL OSCILLATION.

RE-INSTALLED EQUIPMENT.

DEWAR EMPTY AGAIN.

BALKY COMPUTER FOR MAP PROGRAM.

DEWAR FILL

DEWAR EMPTY AGAIN AFTER 4 HOURS. RETURNED TO TUCSON FOR VACUUM WORK. DISCOVERED NEW QUARTZ ISOLATOR HAD COME LOOSE AND PRODUCED THERMAL SHORT FROM HEAT SHIELD TO OUTSIDE. MISTAKE BY A.D. IN PUMP-DOWN DAMAGED DEWER SO BADLY THAT REPAIRS COULD NOT BE EFFECTED UNTIL LATE IN THE EVENING OF JAN 26.

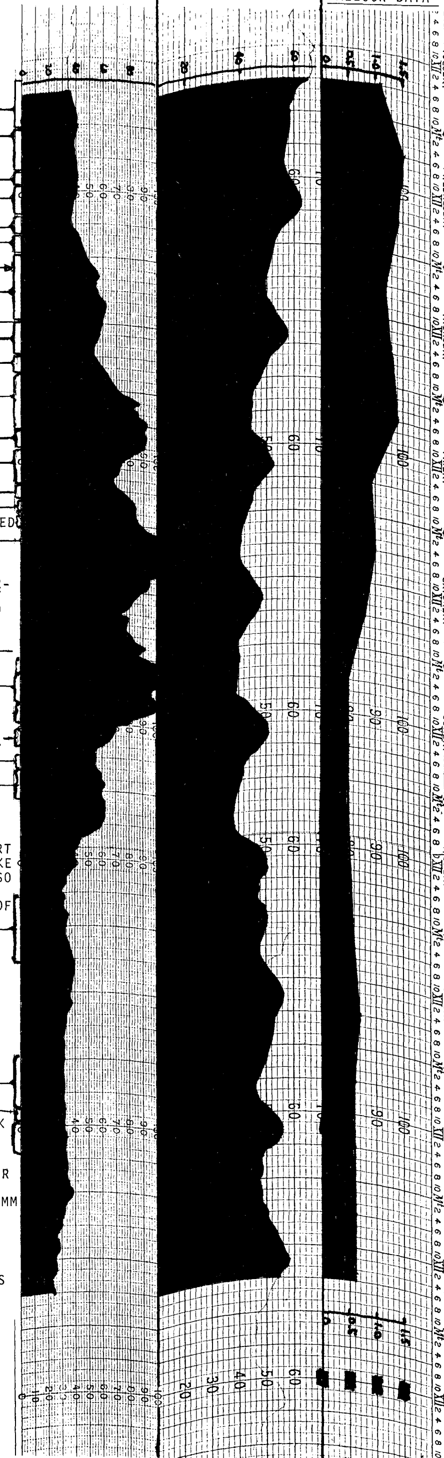
DEWAR FILL

BOLOMETER OUTPUT BECAME WEAK AND NOISY.

AT FIRST INTERNAL TROUBLE IN BOLOMETER WAS AGAIN SUSPECTED. RETURNED AGAIN TO TUCSON SINCE BASE PRESSURE WAS 12 MM INSTEAD OF 8 MM AS IN ALL PREVIOUS RUNS. SUBSEQUENTLY DISCOVERED THAT PREAMPLIFIER POWER SUPPLY VOLTAGE WAS LOW.

SHUT DOWN FOR CHANGING GUY WIRES

BLACK FRIDAY.



WEATHER NOTES

CLEAR BUT MUCH PRECIPITABLE H_2O

LOAD SWITCHED SCANS THRU SUN NOT REPRODUCABLE AT THE 2% OF SUN SIGNAL LEVEL. POSS. DUE TO LARGE ATMOSPHERIC ATTENUATION.

CLEAR

SCATTERED HIGH CLOUDS

SUN OBSERVATIONS THROUGH CLOUDS.

ENTIRE SKY COVERED WITH HIGH, THICK STRATOFORM CLOUDS.

NO BRIGHT SOURCES
WEATHER HOPELESS

OPACITY TOO BAD FOR POINTING ON SUN

SKY FINALLY CLEARING

CLEAR

CLEAR

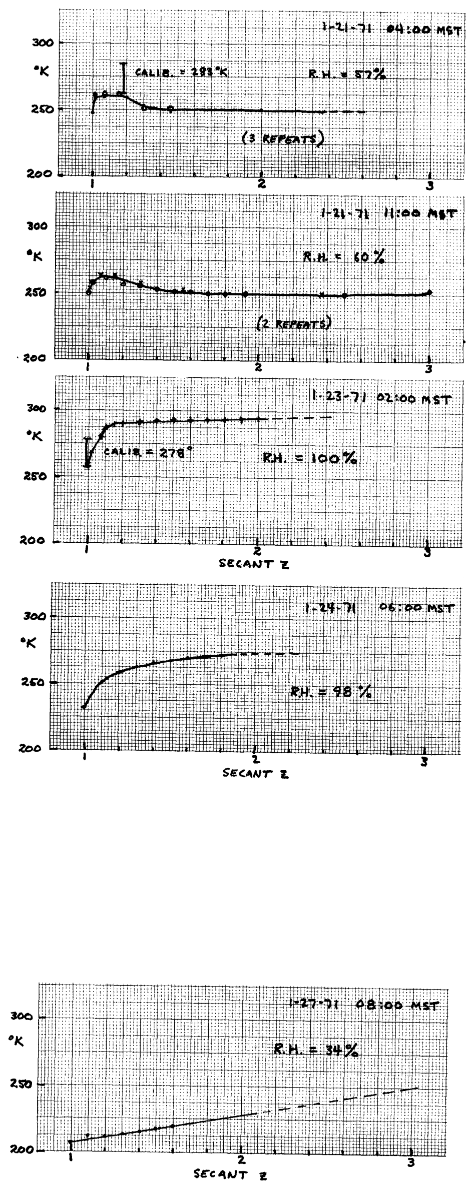
CLEAR

CLEAR

CLEAR

CLEAR

SKY TEMPERATURE VS SECANT Z SCANS



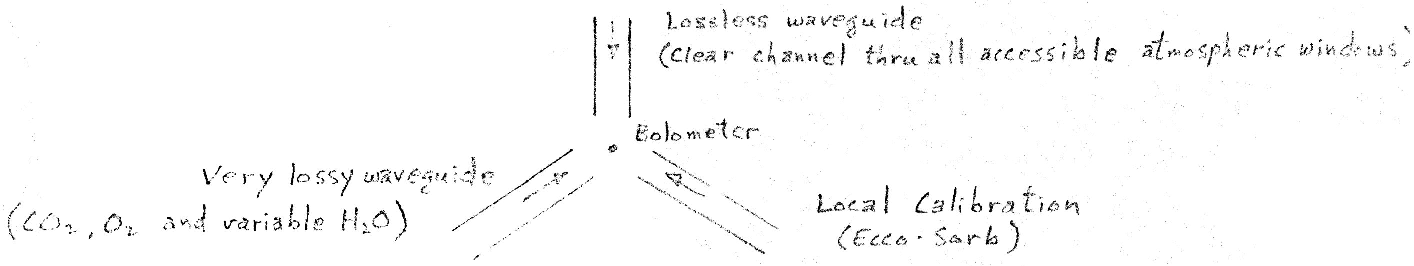
of the sun and some data on the brightness of active regions. I was able to optimize the new servicing proceedures using the extendable dewar mount. I also attempted observations of three more extragalactic sources. The most significant results of the two observational runs are summarized here.

1. Sky Brightness.

Reference to the graphs shown in the schematic chart of the January run demonstrates the very strong dependance of atmospheric tipping results on the total precipitable water in the atmosphere. The balloon data, which I obtained from the National Weather Records Center in Asheville, N. C., was essential to an understanding of the tipping scans. The reason for this can be seen from the fact that the relative humidity at ground level in the dome gave a poorer representation of the total water than the cloud cover. The balloon flights were launched from Tucson airport and drifted generally in the direction of Kitt Peak. Water measurements normally extend to c. 25,000 ft.

At first I was fairly bemused by the wide variation in the T_{sky} vs. Secant Z plots. It became clear, however, that on days when the atmosphere contains more than about 1 cm. of precipitable water the telescope seems to be surrounded by an isothermal sphere of high opacity (see two top graphs). The 10° brightening at high elevations was most likely due to laminar wind flow carrying moist air from the valley over the mountain. On cloudy days in the winter, clouds frequently cling to the peak. As the high altitude moisture decreased, the curves more nearly match a plane-parallel atmospheric model of high opacity (3rd and 4th graphs) and eventually produce a very satisfactory linear profile (5th graph). The only disconcerting matter is that the Secant Z = 0 intercept occurs at 220°K instead at at $\sim$ 3°K as it should. Moreover, a sky temperature of < 200°K has never yet been observed by me. Indeed, on very dry days a temperature of $\sim$ 220°K is observed at all elevations, suggesting strongly that we are still surrounded by an isothermal bath of high opacity. This datum is what led to the realization that the bandpass of the filtering in the detector is very poor. The laboratury results discussed in Chapter II (4) now make it clear that we are seeing CO_2, O_2 and many other sources of constant opacity in the infrared.

An equivalent circuit for the prevailing situation is illustrated by the three "waveguides" shown below:



Clearly, the only reason that I have obtained the limited results now in hand is due to the fact that an approximately loss-less channel <u>does</u> exist. This provides considerable encouragement that a good system <u>can</u> be worked out when the filtering problem is solved.

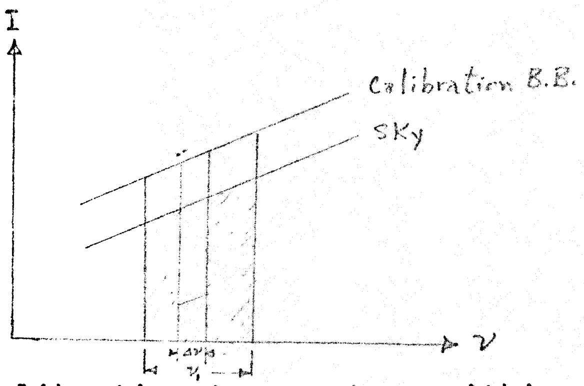
It is perhaps instructive to try to estimate the attenuation which we can expect when we have a filter that confines detector response to the 1.0 → 1.6 mm. passband. The dry weather tipping plot (5th graph) shows a slope of ∿ 20°K per Secant. If we use the small opacity approximation, ignoring for the moment the constant 220°K background, we have:

$$T_{\substack{atm.\\H_2O}} = \frac{\tau(z)}{\text{Sec } Z} = \frac{20°K}{\text{Sec}} \quad .$$

Since most of the atmospheric water vapor is limited by its condensation properties to the first 2 Km. above the ground, where 6°/Km. is a typical lapse rate, a good approximation to $T_{atm.}$ is obtained by subtracting 10°K from the ambient temperature at ground level. Thus

$$\tau = \frac{20°}{270°} = 0.074 \quad .$$

This, however, is not the correct value for the following reason (elucidated by E. K. Conklin): Referring to the idealized graph on the following page, let us consider a bandpass interval ν_1 defined by the existing filters. Now consider within the bandpass a sub-interval $\Delta\upsilon$ which is less opaque.



The effective calibration temperature within $\Delta\upsilon$ is greater than it is for the remainder of υ_1. The temperature scale for the tipping plot must be adjusted accordingly. To a first approximation the correction factor κ can be taken as the ratio of the total atmospheric temperature ($\sim$ 270°K) to the temperature excess above the measured opaque background ($\sim$ 270° - 220° = 50°K). This should approximate the reciprocal fraction of the total bandpass which is transparent. Hence $\kappa \simeq 5.4$. Then:

$$\tau' = \kappa\tau = 0.4 \quad .$$

2. The Sun.

The January run chart indicates the disappointingly poor data obtained by Dr. M. Simon. Unfortunately, most of the problems which developed occurred first during the day hours. Simon was very helpful in obtaining approximate pointing data when conditions were too bad for other observations. His drift scans also give measurements of beamwidth and shape which are fairly consistant with the Jupiter data reported below. He was able to look for predicted flux oscillations on the first two days of the run, but was frustrated by the previously mentioned thermal oscillations in the dewar and by atmospheric problems. He obtained one good map of the sun after considerable trouble with the computer.

Dr. G. Feix had somewhat better luck during the two useful days of the March run. He made numerous maps, identified active regions and observed them for variations. He reports that he was able to resolve features of about 1' diameter.

Since I have not been assigned any sun time and do not have any of the other observers' data in hand, I shall not discuss the sun further except as it relates to calibration studies.

3. Planets.

The configuration of the planets has been very inauspicious for 1 millimeter work both in January and in March! Saturn was available in the evening hours in January and was used to good advantage on my three good observing days. In March it was usually so far to the West by the time the equipment was cooled down for the evening run that it was only briefly available (recall that the dewar presently cannot be tipped more than $\sim 45°$ from the vertical when full). Uranus was in a favorable position but too weak for focussing and beam size studies. Venus, Mars and Jupiter were all so near the sun in January that they could only be observed on loaned time from M. Simon when the dewar was more than half empty. In spite of this quite a bit of useful pointing and calibration data has been obtained.

The best signal to noise obtained for Jupiter was 128:1 in a set of 5 point offsets done on January 27. From a gaussian fit to this data beamwidths of $\theta_{AZ} = 71.8''$ and $\theta_{EL} = 60.6''$ have been determined. This agrees closely with $\theta_{AZ} = 66.7$ and $\theta_{EL} = 60.5$ determined from drift scans done on January 24. The separation of main and reference beams is $\sim 147''$.

Results of selected planetary observations are given in Table 3. It can be seen that there is a monotonic decrease in calculated aperture efficiency with elevation angle. The significance of this is not understood, but it is not viewed with any great alarm at this point because of the very large uncertainties due to the broad bandwidth of the filters. The same calibration factor discussed under sky brightness must also be applied to the observed sources. If the effective clear bandwidth becomes narrower at lower elevation angles, the correction factor should be proportionately larger, whereas

Table 3

Planet	Date	Ambient Temp(°F)	Focus	Elevation (Deg.)	τ Sec Z	Measured T_A(°K)	Corrected* T_A(°K)	Calc.† T_{bb}(°K)	Assumed T_{bb}(°K)	Calc.‡ η_A
Saturn	1/24/71	35	470	45	0.70	0.9 ±0.2	12.4	160° ±36	120°	33.2%
Uranus	1/24/71	35	445	53	0.46	0.24±0.1	2.0	83 ±36	100	51.0
Jupiter	1/27/71	45	392	32	0.76	2.5 ±0.2	28.9	94 ±12	130	18.1
Jupiter	3/19/71	45	505	33	0.75?	2.65±0.1	29.4	64 ± 7	130	12.6

*Corrected for τ Sec Z opacity in passband and factor of 5.4 in calibration due to opaque atmosphere included in bandpass.

†T_{bb} calculated assuming η_A = 25% in all cases.

‡η_A calculated assuming T_{bb} listed in adjacent column

we have always used the constant $\kappa = 5.4$. Obviously this is a matter that requires considerably more study. I feel that it is best deferred until good filters are in hand, however.

The one calculation of η_A since the feed support accident is also given in Table 3 for 3/19/71. The measurement was also done on 3/18/71 with the same result. It is 30% below the η_A obtained for Jupiter under similar conditions in January, but, again, there are too many uncertainties to comment on this result with any conviction at this time.

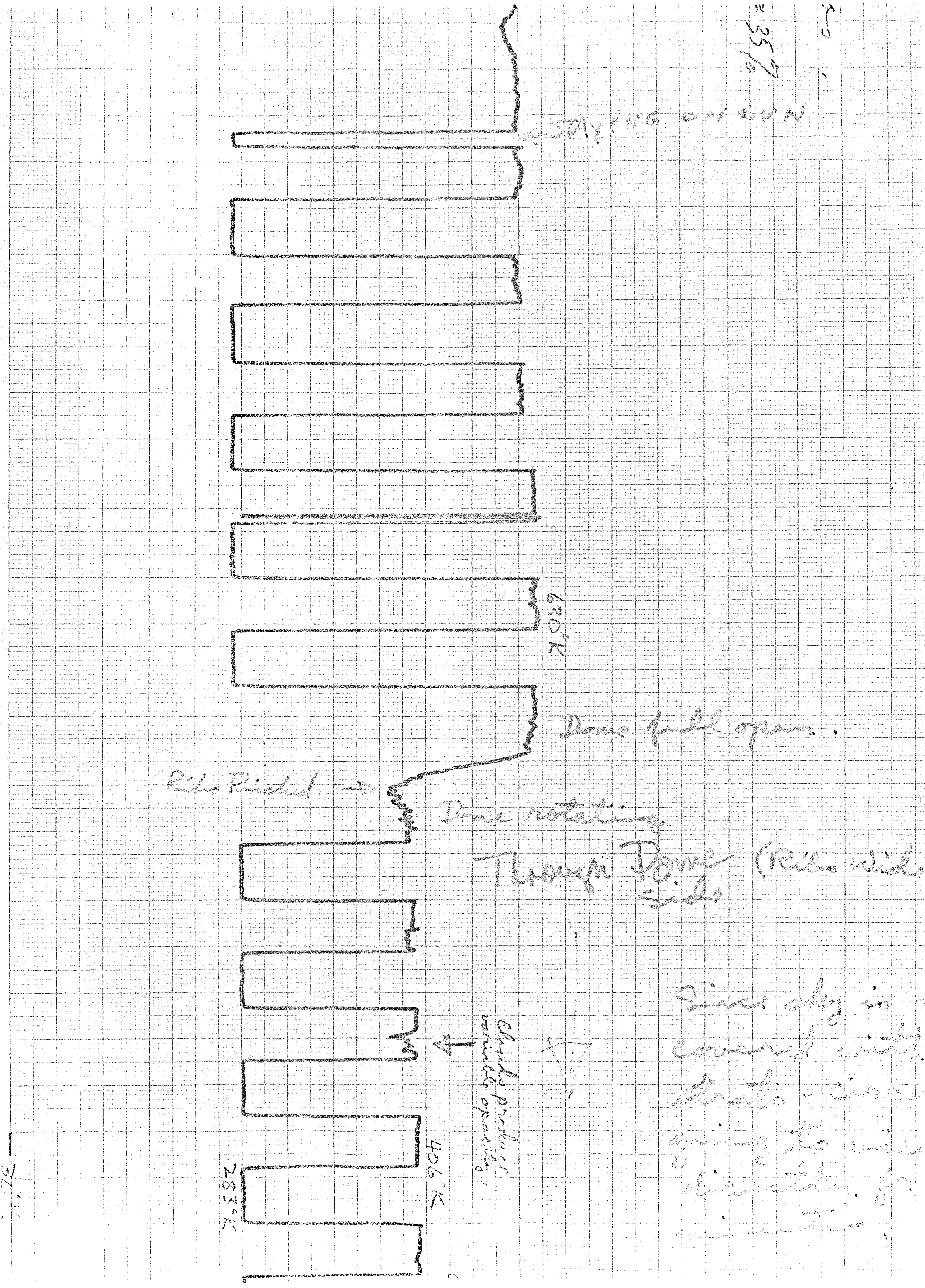
Details of the calculational proceedures are outlined in Section 6 below.

4. Extragalactic Sources.

Clearly, practical observations of extragalactic sources cannot be contemplated until the present RMS fluctuation level of $\sim$ 25 flux units in 10 minutes can be improved. Nevertheless I have attempted to observe several of the brighter sources during the hours when planets were not available. Multiple five point patterns with ± 30" offsets have been run four times on 3C273, twice on OJ287 and once on 3C345. I have obtained one probable detection of 3C273 with S/N = 2.1. The total integration time was 2 hours and the flux obtained was 55 flux units. The lengthy spate of bad weather in January permitted weak source attempts on only three nights. The dewar failure in March provided only one good night as the first night was spent in equipment tests before Jupiter rose. Hence, only four nights of extragalactic work have been attempted. No one is more anxious to further progress in this area than I am.

5. Dome Attenuation.

An interesting experiment was performed on 1/22/71 when the sky was completely covered with clouds. The Xerox copy of the chart data on the next page shows the result of rotating the dome so that instead of looking out through the fabric side we looked directly out of the open slit. This cannot be done when the day is clear because the large



HEWLETT-PACKARD 9270-1037

thermal gradients imposed on the telescope will make it useless for hours afterward). It can be seen that the signal increased from 406°K to 630°K. Hence the dome reduces the signal by 36.6% or 1.9 db.

It is also interesting to note in passing that if we assume a beam efficiency of ∿ 30%, the temperature seen by a perfect antenna would have been 2100°K. Then, assuming T ≈ 6000°K, the transmissivity of the atmosphere was 35%. Since it is likely that the clear channels discussed in the section on sky brightness were still approximately clear on this cloudy day, the transmissivity of ∿ 18.5% inferred from the sky brightness data alone seems reasonably consistant.

6. Beamwidth and Aperture Efficiency.

The beamwidth and aperture efficiency results were given in Section 3 above. The techniques used were similar to those described by Mezger *et al*. in the NRAO internal report, "Radio Tests of the NRAO 140-Foot Telescope...", August, 1966. The observed T_A's were corrected for atmospheric opacity using the value $\tau = 0.4$ obtained in the January 27 sky brightness studies. This value is consistent with published opacities at 1.3 mm. and is probably fairly consistent on typical dry, cloudless days at Kitt Peak. The calibration correction for bandwidth, $\kappa = 5.4$, was then applied. The resulting T_A was then substituted into

$$T_A = T_{bb} \Omega_s' \frac{A}{\lambda^2}$$

where T_{bb} is the assumed brightness temperature of the source, A is the effective area of the antenna and Ω' is the correction factor for resolution of the planetary disk. The latter is given by:

$$\Omega'_s = R_p R_e \left[1.0 + 1.39 \left(\frac{R_p R_e}{\theta_{AZ}\theta_{EL}}\right)\right]$$

where R_p and R_e are the apparent polar and equatorial radii of the planet, respectively, and θ_{AZ} and θ_{EL} are the measured half power beamwidths.

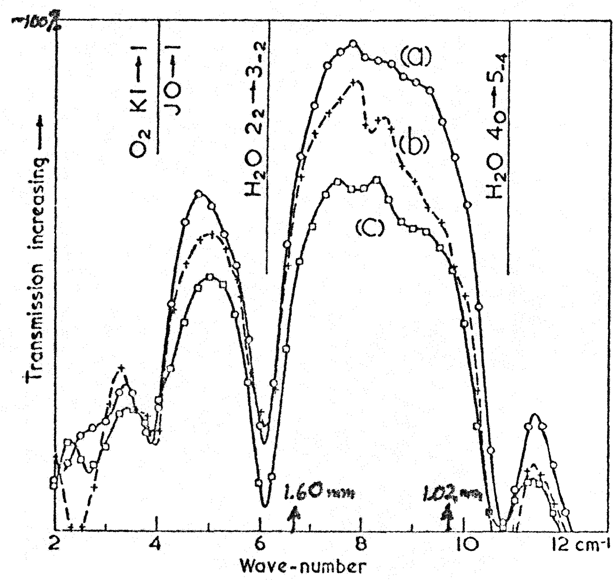
The values assumed for T_{bb} were derived by extrapolation of the 3.5 and 9.5 mm. results of Pauliny-Toth and Kellermann (Ap. Letters, 6, 185, 1970) and pragmatically subtracting 10°K to compensate for the expected lapse rate.

Limb scans of the sun have not been fully exploited to date. Fragmentary data indicates some assymetry in the main beam in addition to its ellipticity.

IV. Proposals

1. Mesh Filters.

It is clear that the present filtering scheme is unacceptable and must be replaced. Some new data on the spectrum of the water lines has recently become available which helps to define the desired bandpass. It can be seen from the figure below that abrupt cutoff is desirable at 1.6 mm. and 1.02 mm. with maximum transmission in the intervening band.



Atmospheric water vapor spectrum derived from solar observation at sea level. Elevation angles (a) 37°, (b) 34.4°, (c) 31°. Resolution 0.5 cm^{-1}.

Ref. J. E. Harries and W. J. Burroughs, Infrared Physics, 10, 167 (1970).

A superior filtering technique has been developed by Reinhard Ulrich of Bell Labs. The method makes use of a combination of two types of metal meshes. The first type is similar to familiar window screen, while the second type has the complementary pattern of filled squares with vacant cross-hatching supported by a mylar substrate. The former has inductive properties, while the latter is capacitive. Hence a combination of the two types can produce a resonant device with reasonably high Q. As the number of elements is increased, the cutoff becomes sharper, but the attenuation also increases in the passband. An optimum filter would probably consist of three or four elements with ∿ 75% transmissivity* in the passband and a reduction rate sufficient to reduce the transmission to 5% at the water line wavelengths. Performance of

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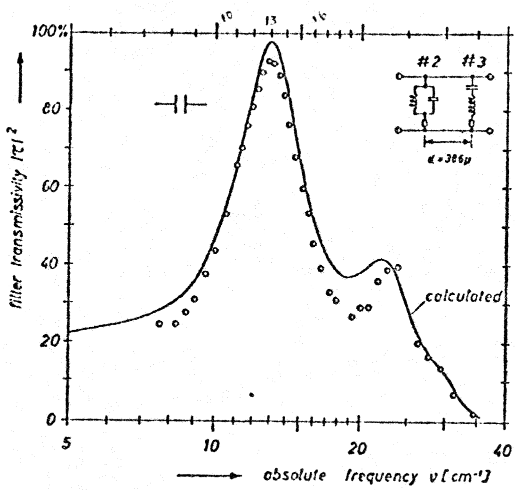
*Radiation not transmitted is reflected, not absorbed.

a typical filter constructed by Ulrich is illustrated below.

Transmissivity of an interference filter combined from one inductive and one capacitive grid. The higher order interference maxima have low peak transmission because of the assymetry of the filter.

Ref. R. Ulrich, Infrared Physics, 7, 53 (1967).



I regard deletion of water lines and other opaque lines from the passband as the number one priority item for further system development. I believe that mesh filters provide the most promising solution to the problem, and I intend to explore the possibilities to the fullest extent.

2. Calibration.

The present calibration is both inaccurate and undependable. At first a small piece of Ecco-Bond material* provided with the original equipment was used (c.f. Photo #4). Apparently the lateral dimension was too small, such that the background radiation leaked around the edges. Also the thermal conductivity of the material was sufficient that the heat due to the operation of the rotary solenoid warmed it appreciably in a few tens of seconds. The present system employs a 1-inch diameter piece of Ecco-Sorb which is nearly in contact with the chopper. A thermistor is being installed to measure its temperature. This is much better, but still leaves uncertainties.

A major need is for a second thermal calibrator to permit a differential calibration. I am considering either a small "furnace" containing another piece of Ecco-Sorb at about 325°K or perhaps a small liquid nitrogen dewar containing Ecco-Sorb. The former is by far the simplest proceedure, but the latter would permit "bracketing" the sky background. The

*Advertized as "black" at 1 mm.

nitrogen dewar could be hung from small gimbals on the lower feed support leg so that if the bolometer is released (as for servicing) it could tip downward and view the interior of the nitrogen dewar when the telescope reaches some convenient elevation angle.

The present system of switching off the chopper to obtain a zero point is a good idea, but the zero point cannot be established with confidence without two other points to give the temperature scale. Either of the systems outlined above would accomplish this.

3. New Optics and Detectors.

I plan to devote most of the summer to theoretical and laboratory evaluations of various alternative detection systems. There are at least two other detectors commercially available which appear to offer N.E.P.'s far better than that measured for the present system.

The Advanced Kinetics indium antimonide detector is directly immersed in the liquid helium bath and receives radiation through a tapered light pipe. The operational 1 mm. N.E.P. is $\sim 5 \times 10^{-13}$ W/$\sqrt{\text{Hz}}$. At 300μ an N.E.P. of 10^{-15} has supposedly been achieved at 1.2°K. Dr. Ralph Waniek, the developer of this detector, has agreed to attempt a demonstration of its capabilities on our own telescope in May. This project is now being implemented.

A new silicon bolometer is offered by Molectron Corp. On the basis of its heat capacity it should have an N.E.P. of $\sim 10^{-15}$, or 5× better than Low advertizes. The device was developed by Dr. Bruce McCaul. He says that it is available with a 377 Ω coating which greatly improves its efficiency. (As we have indicated in Section II, this is thought to be the area of greatest difficulty with Low's bolometers at 1 mm.) Molectron also offers a 1°K experimental cryostat with 3 day holding time which can be tipped to any angle. The 1°K Helium is contained in a very small closed chamber surrounded by a 4°K Helium heat shield. The possibilities of this unit are clearly of much interest. Shivanandan and Westphal have reportedly verified its superiority in the near IR.

Another project will involve remounting the present detector in a newly constructed cryostat using only the inner helium container, the top and the adjustable head from the current system. The new device will contain only reflective optics and mesh filters cooled to 2°K. It will be designed in such a way that the helium container and fill tube are positioned at a 45° angle when the telescope is pointing at the zenith and come to vertical as the telescope reaches 45° elevation. It will then be possible to reach the entire sky without the spillage problem. The air operated extension mounting will still be used for filling access. Hopefully the detector and filter mountings can be made readily changable so that alternates, such as the Molectron silicon bolometer can be substituted with ease. Finally, efforts will be made to keep the heat shield from its present close proximity to the cold work.

The question of Cassegrain optics is very much in mind. Any new devices which we build or buy will be designed in such a way that they can be used either at the prime or cassegrain focus. One problem which may be encountered is that the large cassegrain image size precludes direct projection of the focal spot onto the detector. The opening in the heat shield would have to be an inch in diameter. A tapered light pipe might provide a possible solution to this problem. It is fairly certain that a cassegrain system would require the rocking subreflector for beam switching.

4. Test Platform on Roof.

There are certain experiments which can be done without an antenna. I have operated the equipment on the roof of the Kitt Peak building which houses our lab to experiment with focussing, balance and calibration as well as wide angle sky brightness. The basement lab is now so crowded that it is difficult to do good work, particularly for any length of time. Also the other workers object to the noise of my pump and the helium leak detector. If possible I would like to explore the possibility of putting a test platform and weather-proof equipment shed on the roof as a more or less permanent location for cold tests. Kitt Peak now has two experiments on the roof and an umbrella type camping tent for equipment, so I suppose that an adèquate precedent has been set. A crane already exists for lifting heavy equipment. No particular problem was encountered in the two days that we spent on the roof in January.

5. Another Dish?

A possible extension of the above idea would include a small dish and mounting. Three dishes obtained by Low remain in Green Bank. They have apertures of 3, 5 and 10 feet, and f/D respectively of .2, .4 and .8. If a dish were mounted on a portable mount and hand guided with a bore-sighted optical telescope it might be very useful for equipment development and calibration purposes. Atmospheric studies could be made and the sun and brightest planets could be easily observed.

V. Discussion of Ways and Means

The equipment obtained from Low was purchased c. 1968 for about $7000. To date in my efforts to improve it I have spent money as follows (figures thought to be accurate to 10%; some shop charges not yet in):

Parts and Materials	$1000	
Machine Shop Time (190 man hours)	1500	(?)
Optical Shop Time (37 man hours)	150	(?)
Carpentry (20 man hours)	50	
Calibration Work	200	
Total (approximately)	$2900	

Additionally I have travelled once to Los Angeles to arrange for the bandpass calibration equipment and to educate myself in infrared technology. I have also incurred the expense of 75 liters of liquid helium at $300 per 25 liters.

Looking ahead, I foresee the following particular expenses within the next few months (figures approximate):

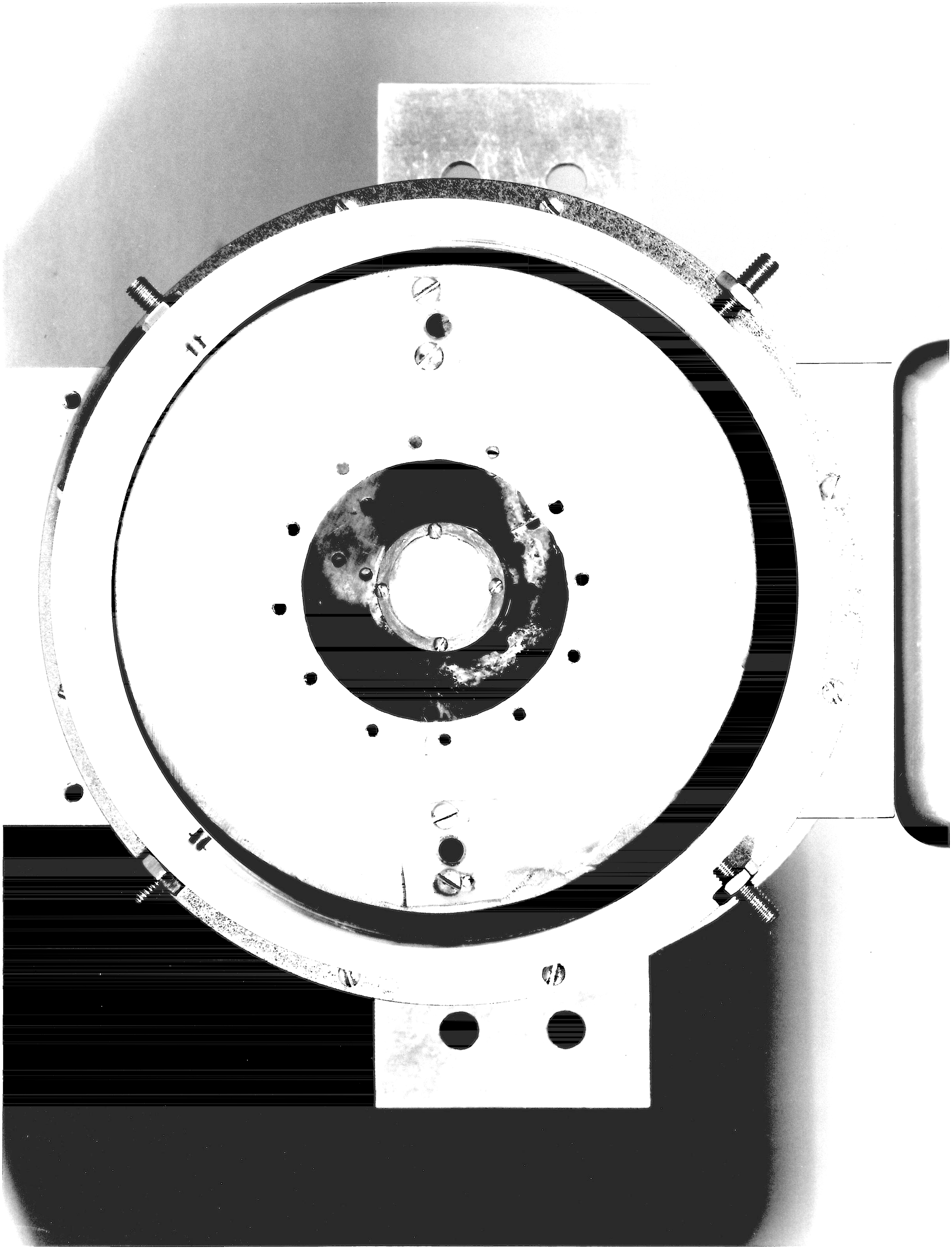
Mesh Filters	$1000	
High Vacuum Gauge & Valves	500	
Molectron Silicon Bolometer	850	
Equipment Rental and Consulting fee to Advanced Kinetics for InSb bolometer tests.	500	(?)
New Helium Transfer Equipment	150	
Total (approximately)	$3000	

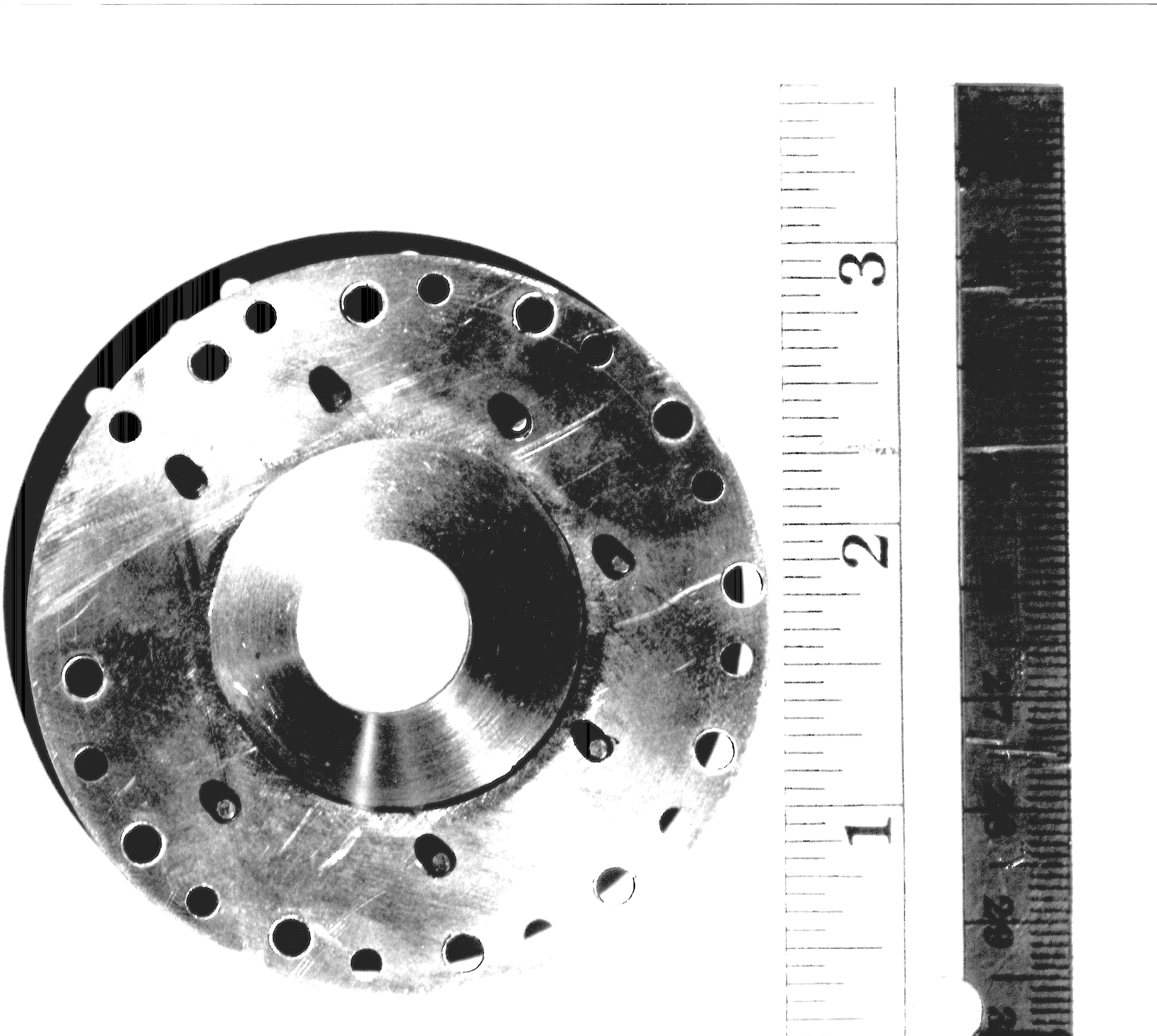
Shop work is expected to continue at the rate of about 100 to 150 man hours/month. I regard $1000 to $1300 per month as a reasonably accurate estimate of my expenses through the summer. If the decision is reached that a new detector system is needed, such as the two discussed in Chap. IV (3), the expected cost is $5000 to $8000. This would be justified on the basis of superior performance, better reliability and simpler operation. There is also some logic to the proposition of having two detector systems so that one can stand ready for use if the other fails or is being modified.

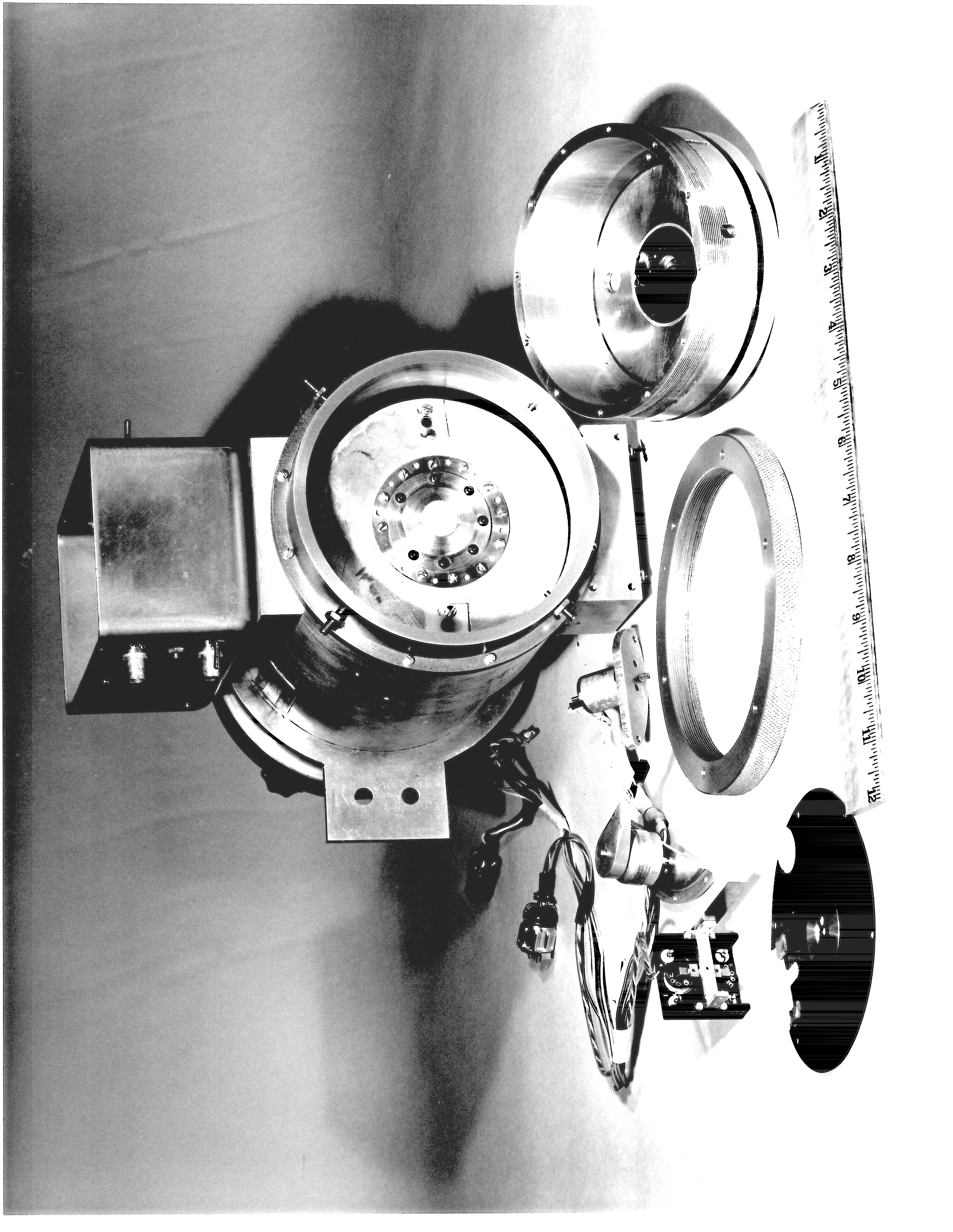
I have learned quite a bit in the nearly five months that I have been involved in this project. I have enjoyed it and look forward to continuing improvement of the equipment in the coming months.

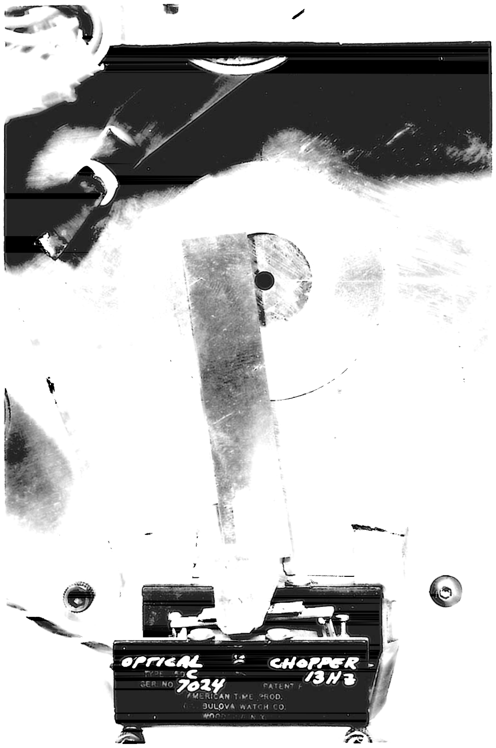
I would like to acknowledge numerous helpful discussions with Ned Conklin. I have already thanked my wife suitably for typing this paper.

John D. G. Rather

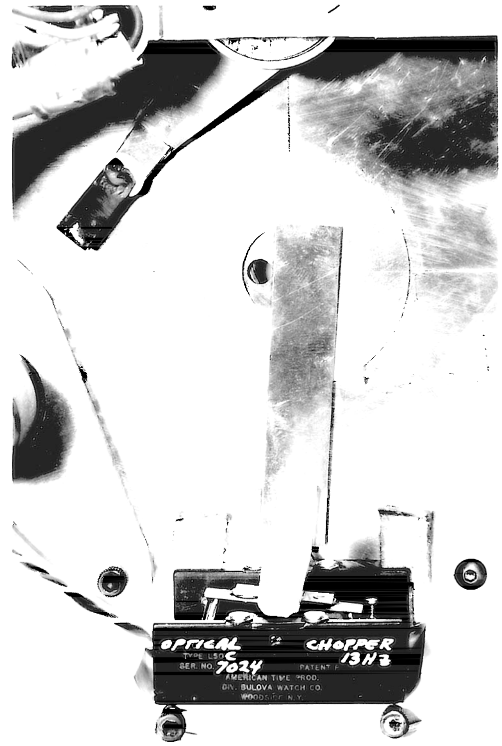




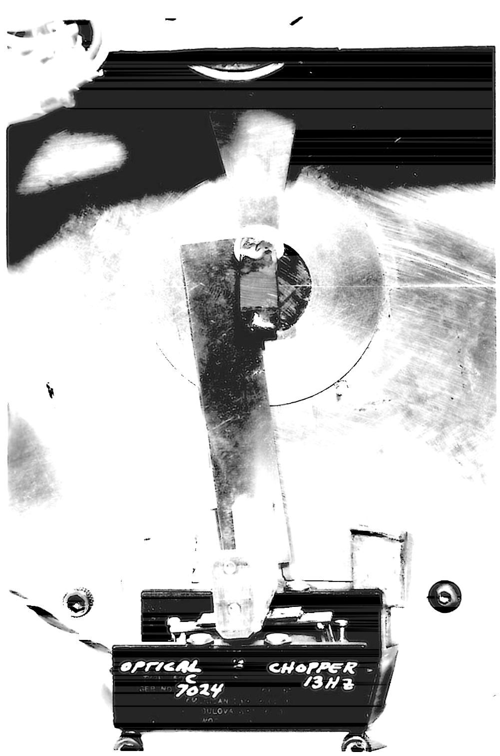




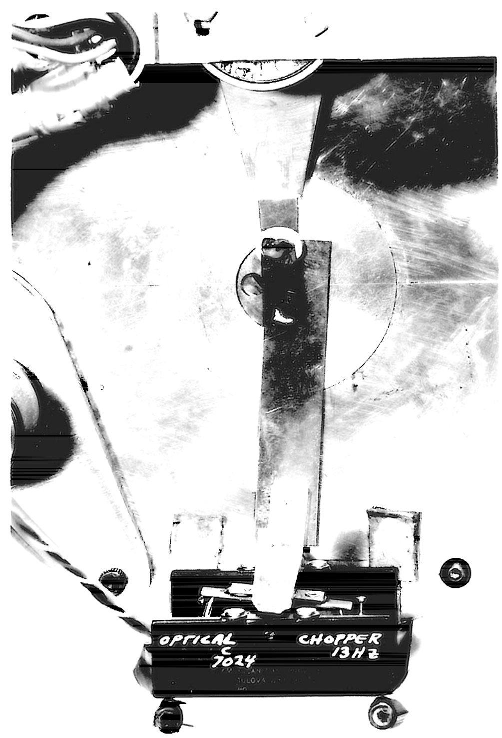
a) MAIN BEAM



b) BALANCED REFERENCE BEAM

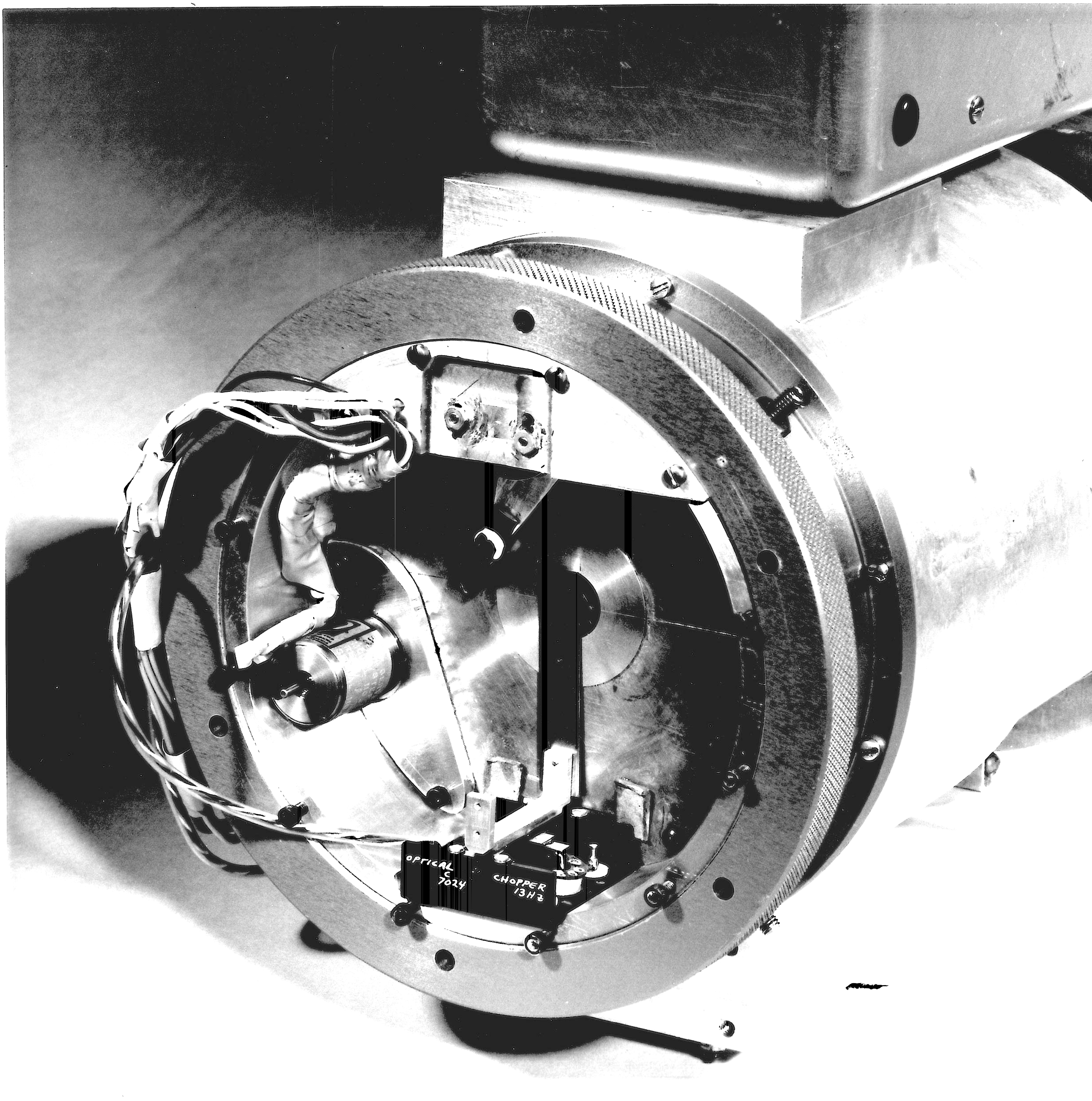


c) HOT LOAD IN MAIN BEAM



d) REFERENCE BEAM CLOSED (~2°K)

Fig. 4 CHOPPER AND IRIS OPERATION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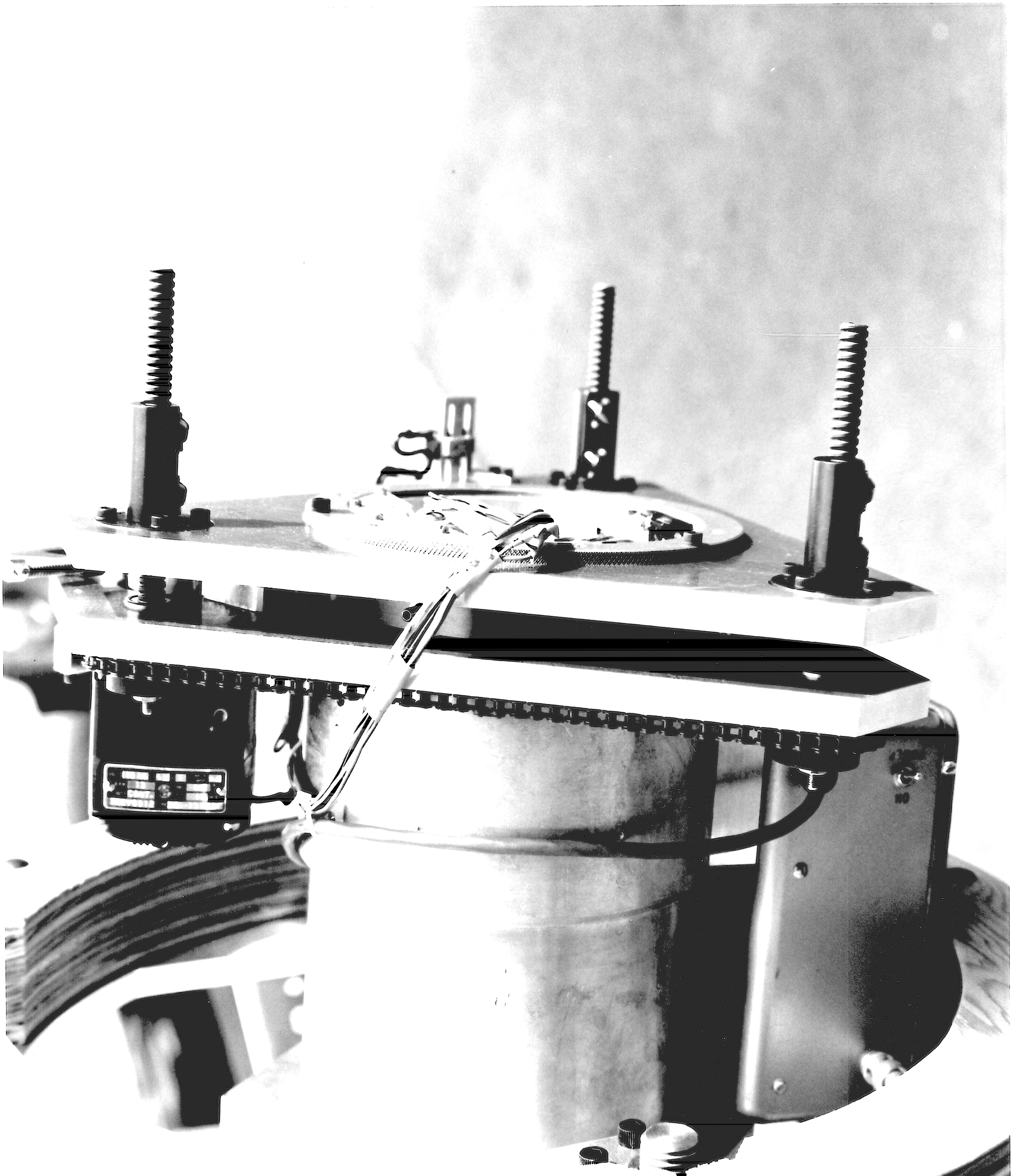

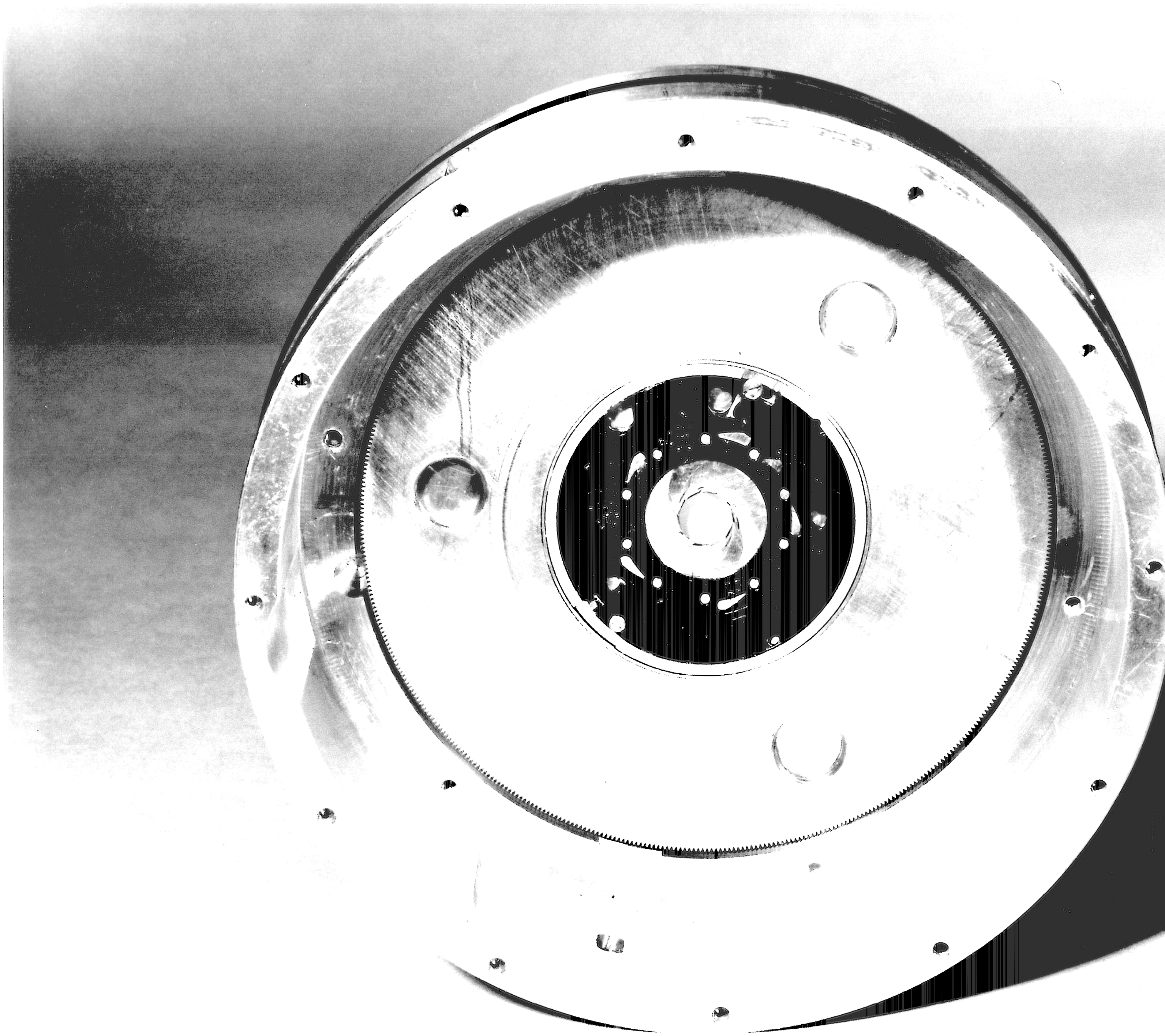


EXHAUST
VENT
PUMPING