Measuring the Gravitational Redshift

or "Weighing" Photons

Initially a Collaboration with

Glen A. Rebka, Jr
Graduate student 1954-60
Ph.D 1961

Followed by

Ronald W.P. Drever
Research Fellow 1960-61

and

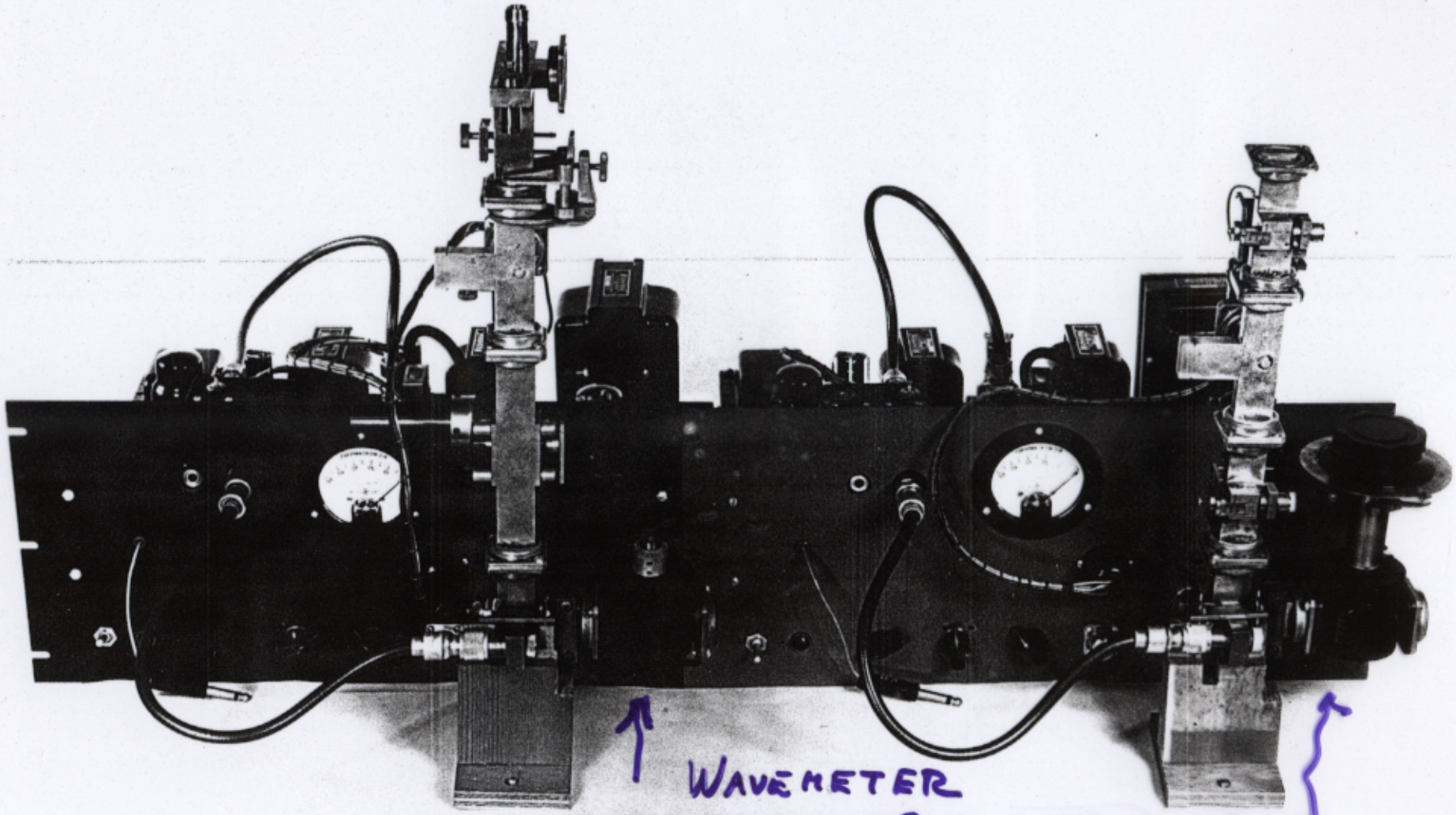
Joseph L. Snider
Instructor, Assistant Professor
1961-65.

Special thanks due to staff
of the Physics Laboratories.
especially F. B. Robie, Engineer
Wm Kennedy, Carpenter

Supported, in part, by Office of Naval Research
under the Joint Program of ONR & AEC

M.I.T MUSEUM (31)

TWO X-BAND "I-F" STABILIZERS.



WAVEMETER

RESONANT CAVITIES

~ 9200 MHz

Q ~ 10,000

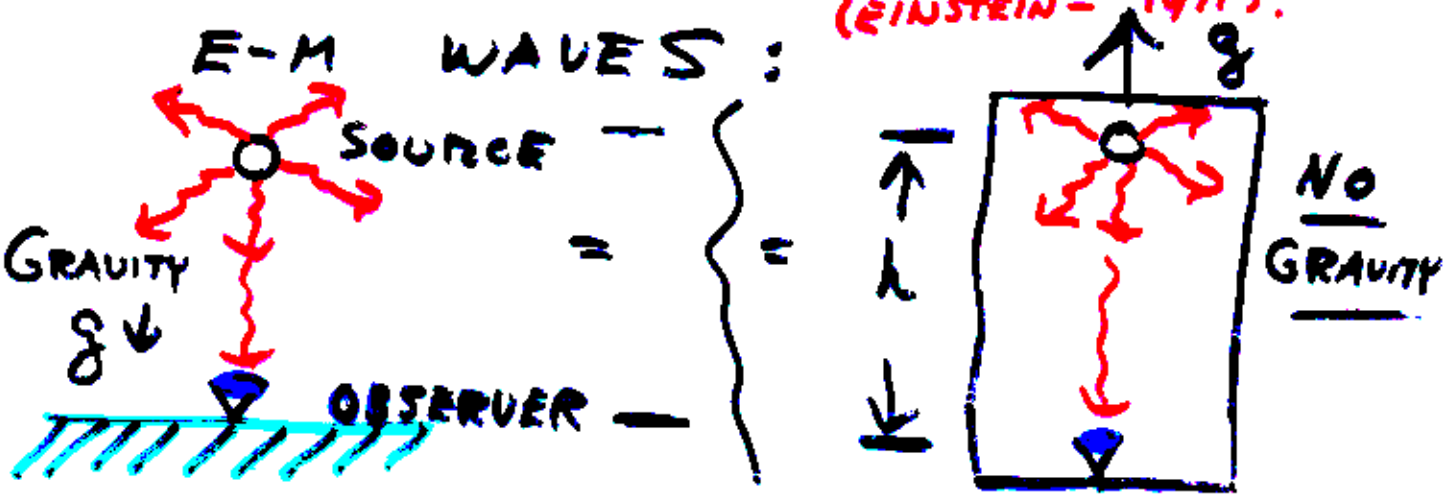
Some Worthy Applications of Precise Time-Frequency Sources to Physics

Relative constancy of Physical "constants" (h, G, E-M, nuclear force) (Dirac, Gamow).

Einstein's "Gravitational Red-Shift" (other 2 of his 3 "crucial tests", i.e. deflection of light by Sun, & perhelion precession of planets not specific challenges to time-freq).

Tests of Special Relativity;
Transverse Doppler effect,
Isotropy of velocity of light
(Mach's Principal, Lorentz Invariance)

G.R. - Principle of Equivalence for E-M Waves: (Einstein - 1911).



Time of flight (on right) $\tau = h/c$

Change of speed during flight $\Delta V = g\tau = g\frac{h}{c}$

Fractional Doppler (1st order) $\frac{\Delta\nu}{\nu} = \frac{\Delta V}{c} = \frac{gh}{c^2}$

More general $gh = \Delta\Phi$; $\therefore \Delta\nu/\nu = \Delta\Phi/c^2$

Values:- at Earth's surface

1.09×10^{-16} / meter.

Earth to remote orbit

7×10^{-10}

Sun to Earth 2.12×10^{-6} $\left(\frac{M}{R}\right)$

White dwarf (Sirius B, 40 Eridani B)

2×10^{-4}; 1×10^{-4} ? $\frac{M}{R} \sim 10^{-4}$

Einstein's conclusion that is that the lines of the solar spectrum, compared with those of a terrestrial one, should be *shifted towards the red*, the proportionate increment of wave-length being

$$\frac{\delta\lambda}{\lambda} = \frac{f}{R} = 2{\cdot}109.10^{-6},$$

or equivalent to a Doppler effect due to a (receding) source velocity of 0·633 kilometers per second. This amounts, for violet light, to about 0·008 Å. Now, although with the modern means one-thousandth of an Å or even less can be well detected in comparing spectra, Dr. St. John of the Mount Wilson Observatory, who observed 43 lines of nitrogen (cyanogen) at the sun's centre, and 35 at the limb, was unable to detect any trace of the predicted effect. His observations were made and discussed in 1917, and his final conclusion then was that "there is no evidence of a displacement, either at the centre or at the limb of the sun, of the order 0·008 Å". Since that time, however, in view of the entanglement of the Einstein effect with shifts of a different origin, and seeing that the results of other astrophysicists were not quite so definite, Dr. St. John suspended his final judgment and is now taking up a thorough discussion of the whole material of solar spectrum shifts from E. L. Jewell's first observations, made about 1890, up to the present. The natural impression now is that it would be premature to either assert or deny the existence of the gravitational spectrum shift.

Einstein himself has, on more than one occasion, expressed the very radical opinion that, should the shift be absent, the whole theory should be abandoned. Yet, in view of the hypothetical nature of *the sameness* of atoms in the explained sense of the word, such an attitude, though personally intelligible, is by no means necessary. It is true that the invariability of an atomic *s*-period of vibration in a gravitational field can, with the aid of the equivalence hypothesis, be reduced to its invariability while the atom is being moved about, —a property of atoms as 'natural clocks' already utilised in special relativity.* Yet we do not know whether the atoms actually possess even the latter property. Thus Einstein's intransigent attitude proves only the strength of his belief that the atoms are or will turn out to be such natural, ideal clocks. But, after all, this is only a guess A very reasonable one to be sure; for if not among the atoms then there is indeed but little hope to find such clocks among other 'mechanisms', natural or artificial.

At any rate, a final astrophysical verification of Einstein's spectrum-shift formula, supported perhaps by repeated experiments on canal rays, would be an achievement of fundamental importance. Until then 'the natural clock' will remain a purely abstract concept.

*It is this theoretical attribute of atoms which has led to the conclusion that moving hydrogen atoms (canal rays) will emit, in transversal directions, waves $(1-v^2/c^2)^{-1/2}$ times longer than atoms at rest. But even this shift effect, though tried experimentally, does not seem to have ever been detected.

GENERAL RELATIVITY & GRAVITATION.

L. SILBERSTEIN. 1922

VAN NOSTRAND, & U. OF

Science in Context 6, 1 (1993), pp. 137-186

to Prof Pound
with kind regards
and many thanks

KLAUS HENTSCHEL

The Conversion of St. John: A Case Study on the Interplay of Theory and Experiment

The Argument

Gravitational redshift of spectral lines as one of the three early-known experimental implications of Einstein's general theory of relativity and gravitation was intensively searched for by researchers all over the world, but around 1920 most of the contemporary evidence in the sun's Fraunhofer-spectrum conflicted with the predictions of relativity theory.

In 1923 the American astrophysicist Charles Edward St. John announced that his own solar spectroscopic data would force him to retreat from his former skepticism concerning the existence of gravitational redshift. This statement was at the time widely interpreted by scientists and journalists alike as the open confession of a rapid conversion of one of the few remaining serious scientific opponents of Einstein's theory.

This paper demonstrates that this illusion of a sudden "Gestalt switch" in St. John's evaluation of data can be dissolved by a careful step-by-step account of St. John's research practice between 1917 and 1923. After a fine-grained diachronic report of the development of St. John's interpretation of his and others' data, the second part of the paper consists in a systematic analysis of the heuristics and arguments used by St. John pro and contra gravitational redshift.

Introduction

This paper is a case study of an episode in the history of experiments on the redshift of spectral lines and the evolution of their theoretical interpretation. My plan is to present a diachronically organized analysis of publications and as yet unpublished letters by and concerning Charles Edward St. John, an important American astrophysicist, who was a major influence in the change of opinion as to whether the gravitational redshift predicted by Einstein's general theory of relativity (in what follows often abbreviated GRS and GTR, respectively) can be detected in spectroscopic data of the sun's Fraunhofer spectrum.[1] During the 1920s, more and

[1] For reviews on this issue, see in particular Earman and Glymour 1980 (theory) and Forbes 1961 (experiment); see also Hentschel 1990a–b, 1992, 1993

Offprint from "Archive for History of Exact Sciences";
Volume 47, Number 2, 1994, pp. 143–201

Printed in Germany

Erwin Finlay Freundlich and Testing Einstein's Theory of Relativity

KLAUS HENTSCHEL

Communicated by J. D. NORTH

Table of Contents

1. Introduction

This article covers aspects of the life and career of ERWIN FINLAY FREUNDLICH, the first German astronomer to consider seriously EINSTEIN's theory of relativity and gravitation. In doing so, he made himself so unpopular among his colleagues that nothing less than EINSTEIN's powerful intercession was necessary for him to be able to continue his research on the experimental verification of this theory. From 1917, FREUNDLICH was the first full-time employee of the *Kaiser-Wilhelm-Institut für Physik* which had been established for EINSTEIN in that same year. In 1920, FREUNDLICH became director of an institute that had been furnished especially for him, informally called the Einstein Tower. Here FREUNDLICH planned to investigate in particular the gravitational redshift in the solar spectrum predicted by EINSTEIN. The background on the funding and the early history of this research station for solar physics, along with its related architectural and sociological aspects, is presented more comprehensively elsewhere;[1] therefore, following a brief biographical summary of the astronomer

[1] See HENTSCHEL [1992]b.

FINLAY-FREUNDLICH — 1957

WIDTHS/λ

$5\text{-}300 \times 10^{-6}$

∴ Sλ/w

$10^{-1} - 10^{-2}$

BUT ASYM

WORK T

PROBLEM

SOLAR STUDIES

TABLEAU I

Déplacements moyens observés pour chaque raie, unité 10^{-3} Å

Position du disque			0	1	2	3	4	5	6
Sin θ			0,000	0,377	0,769	0,884	0,918	0,959	0,984
Longueur d'onde	Id.	Int.							
6 013,5	Mn	6	+ 2,8	3,8	3,5	4,2	4,3	5,0	11,2
6 016,6	Mn	6	5,1	5,6	5,8	5,0	6,3	9,8	12,3
6 021,8	Mn	6	6,5	7,0	6,0	7,5	7,0	10,0	14,2
6 024,1	Fe	7	2,5	1,5	4,0	2,8	4,5	7,7	9,0
6 027,1	Fe	4	9,8	8,7	8,8	7,0	11,8	14,7	16,8
6 042,1	Fe	3	7,6	9,3	9,5	11,3	11,0	13,2	18,5
6 056,0	Fe	5	2,5	3,5	3,0	3,5	8,0	7,3	11,5
6 065,5	Fe	7	7,1	7,8	7,8	9,7	10,2	12,0	14,5
6 108,1	Ni	6	1,3	— 0,3	— 0,7	1,3	3,7	3,0	8,3
6 122,2	Ca	10	6,3	7,5	7,3	9,5	10,7	11,2	14,0
6 200,3	Fe	6	5,5	5,7	7,3	7,2	6,8	10,5	13,8
6 219,3	Fe	6	3,2	3,8	4,7	2,8	7,0	7,8	10,8
6 265,1	Fe	5	9,8	10,3	9,0	10,0	12,2	12,2	19,2
6 270,2	Fe	3	0,4	0,3	— 0,7	0,5	0,3	3,2	7,0

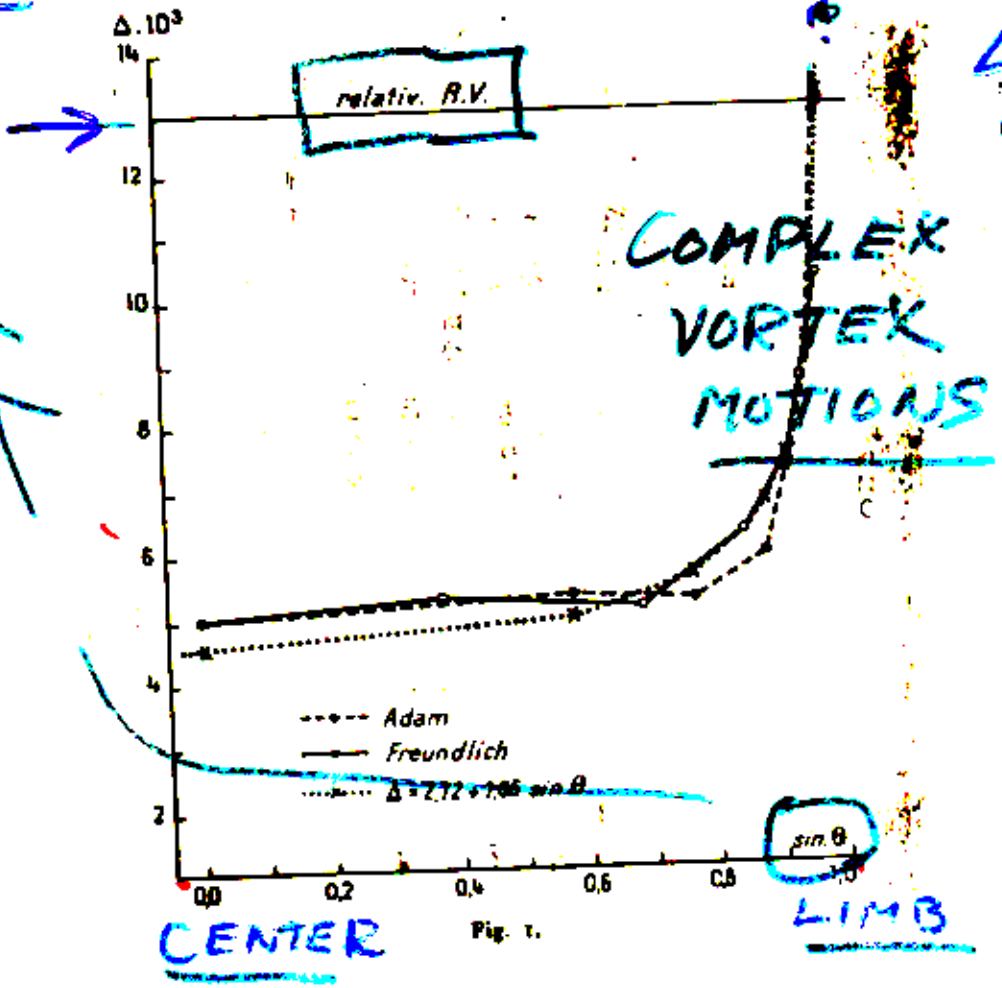


Fig. 1.

$\frac{\Delta\lambda}{\lambda} = 2.12 \times 10^{-6}$

COMPLEX VORTEX MOTIONS

C

Excerpt from the *Space Primer*

Composed by E. M. Purcell, E. H. Land, and Francis Bello

A Report to Eisenhower's PSAC, 1958

(ESTABLISHED AFTER SHOCK OF SPUTNIK)

"Physicists are anxious to run one crucial and fairly simple gravity experiment as soon as possible. This experiment will test an important prediction made by Einstein's General Theory of Relativity, namely that a clock will run faster as the gravitational field around it is reduced. If one of the fantastically accurate clocks, using atomic frequencies, were placed in a satellite and should run faster than its counterpart on earth, another of Einstein's great and daring predictions would be confirmed. (This is not the same as the prediction that any moving clock will appear to a stationary observer to lose time – a prediction that physicist already regard as well confirmed.)"

2101 CONSTITUTION AVENUE
WASHINGTON 25, D. C.

CABLE ADDRESS: NARECO
WASHINGTON, D. C.

NATIONAL ACADEMY OF SCIENCES
NATIONAL RESEARCH COUNCIL
OF THE UNITED STATES OF AMERICA

SPACE SCIENCE BOARD

September 23, 1958

Dr. Robert V. Pound
Harvard University
Cambridge 38, Massachusetts

Dear Dr. Pound:

With further reference to your recent telephone conversation with Dr. Bruno Rossi, this will confirm the organization by the Space Science Board of an Ad Hoc Committee which will take up the special project of considering relativistic clock experiments. Dr. Thomas Gold of Harvard University has agreed to serve as chairman of this committee, and we are pleased to hear that you will give your support to the committee's work. Other members will include: Professor J. A. Pierce, Harvard University; Dr. Philip Morrison, Cornell University; Dr. John Robinson Pierce, Bell Telephone Laboratories.

Chairman Gold plans to call a meeting of the Ad Hoc Committee for the Consideration of the Relativistic Clock some time during the week of October 5. Mr. M. J. Stoller of the National Advisory Committee for Aeronautics will audit the meeting. In a few days you will be advised as to the exact time and place.

We are enclosing copies of the proposals and/or letters pertaining to experiments which the committee will consider. These are as follows:

1. A satellite gravitational "red shift" experiment -- J. M. Richardson, Microwave Physics Section, Radio Standards Laboratory, Boulder

2. An accurate measurement of the gravitational frequency shift predicted by the general theory of relativity -- National Bureau of Standards, Washington, D. C. -- Letter dated 7-14-58 from L. S. Taylor, Chief, Atomic and Radiation, Physics Division

3. Letter from Professor Charles H. Townes, Physics Dept., Columbia University

4. A relativistic clock satellite experiment proposed by Hughes Research Laboratories, Hughes Aircraft (accompanied by four supporting documents and addendum dated August 22, 1958, extending the experiment to include precision measurements of the velocity of light and the figure of the earth.)

5. An experiment to observe the rate of an atomic clock in a satellite -- J. R. Zacharias, Department of Physics, Massachusetts Institute of Technology

Within a short time, we hope to obtain still another proposal from Varian Associates and copies will be distributed as soon as they are received.

It is hoped that your deliberations will form the basis for some specific recommendations in the form of a report to the Space Science Board prior to the Board's next meeting on October 24-25, 1958.

Sincerely yours,

R. C. Peavey

R. C. Peavey, Secretary
Space Science Board

Enclosures

CONCEPT PUBLISHED PHYS. REV. 104, 11 (1956) S.F. SINGER

IN THE 20's GRS CONSIDERED CONFIRMED BY OBS. FROM SIRIUS B - BUT ~ 50% CORRECTION INVOKED FOR ESTIMATE OF LIGHT (H_p, H_y) FROM SIRIUS A.

HOW CAN THIS BE A TEST WHEN SURFACE EFFECTS, AS FOR SUN, UNRESOLVED?

M/R ? (PERIOD, PARALLAX, SEPARATION, SPECTRAL TEMP, LUMINOSITY, BRIGHTNESS)

40 ERIDANI B. POPPER, 1954.

LESS OVERLAP SPECTRA; M/R LESS CLEAR. REASONABLE RESULT - 17 ± 4 vs. 21 ± 4 KM./SEC. NOT A CONCLUSIVE TEST OF A THEORY - A CONSISTENCY CHECK.

FROM L. LEE, L. MEYER-SCHUTZMEISTER, J.P. SCHIFFER, AND D. VINCENT. PHYS. REV. LETTERS 3 223 (SEPT '59)

ONE OF TWO LETTERS IN SEPT 1 PHYS REV LETTERS REPEATING + EXTENDING WORK OF R. MÖSSBAUER PREVIOUSLY PUBLISHED IN GERMANY.

DOPPLER SHIFT SPECTRUM

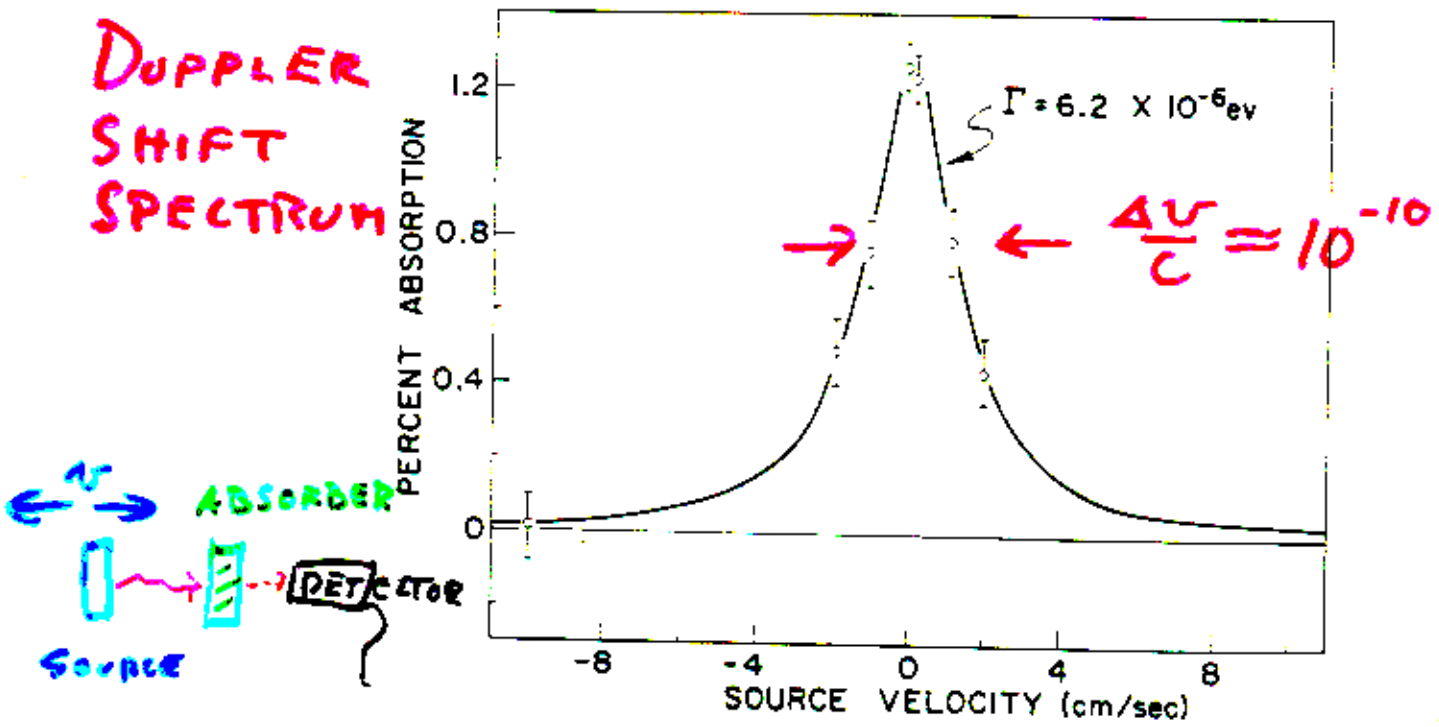


FIG. 2. Percentage of absorption plotted as a function of source velocity for the 129-kev Ir gamma ray following the decay of Os^{191}. The absorber was 0.88 g/cm^2 of iridium metal and both source and absorber were at the temperature of liquid nitrogen. The curve was calculated assuming Breit-Wigner shapes for both the emission and absorption with the width given in the figure. The errors shown for the points are those due to counting statistics alone.

MÖSSBAUER'S DEMONSTRATION OF EFFECT OF LATTICE BINDING FOR LOW ENERGY Γ AT LOW TEMP.

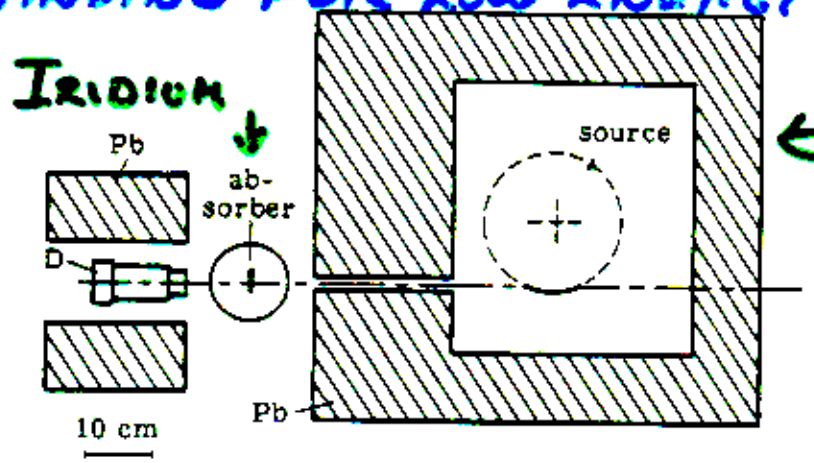
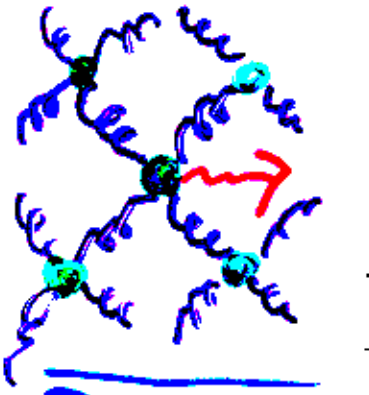
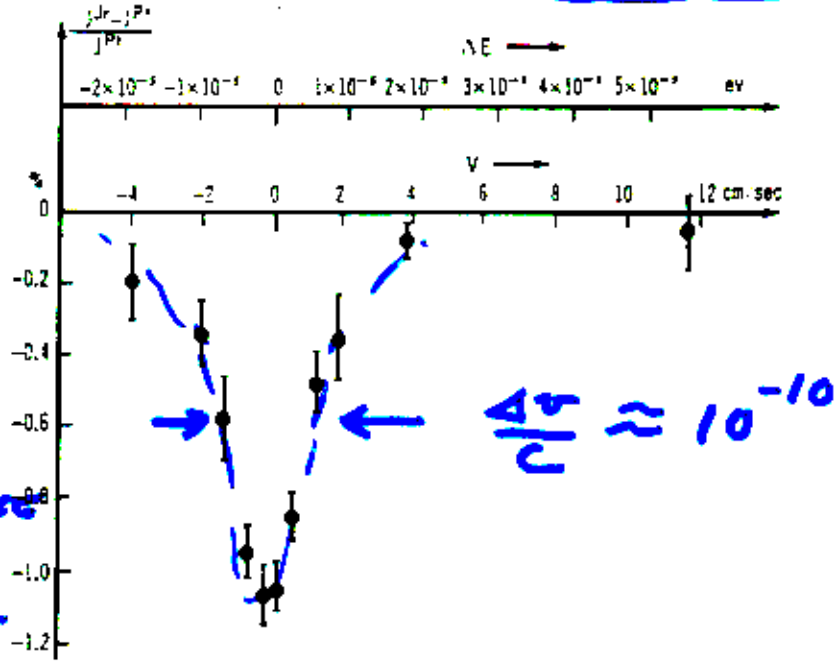
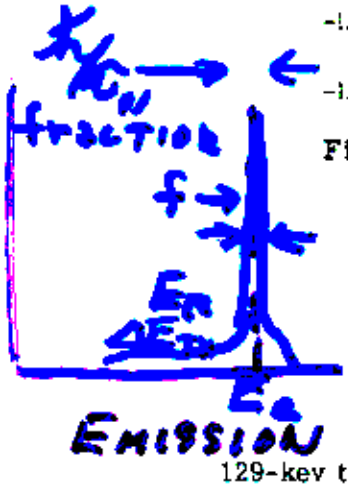


Fig. 1. Experimental arrangement: The detector D accepts only photons emitted by the source while moving on the solid portion of the path shown.

† Translation of article in Naturwissenschaften, **45**, 538 (1958).







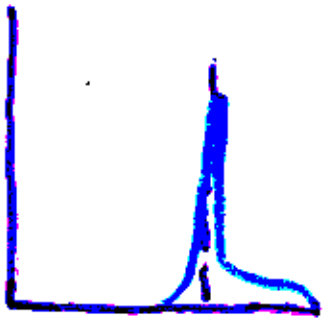
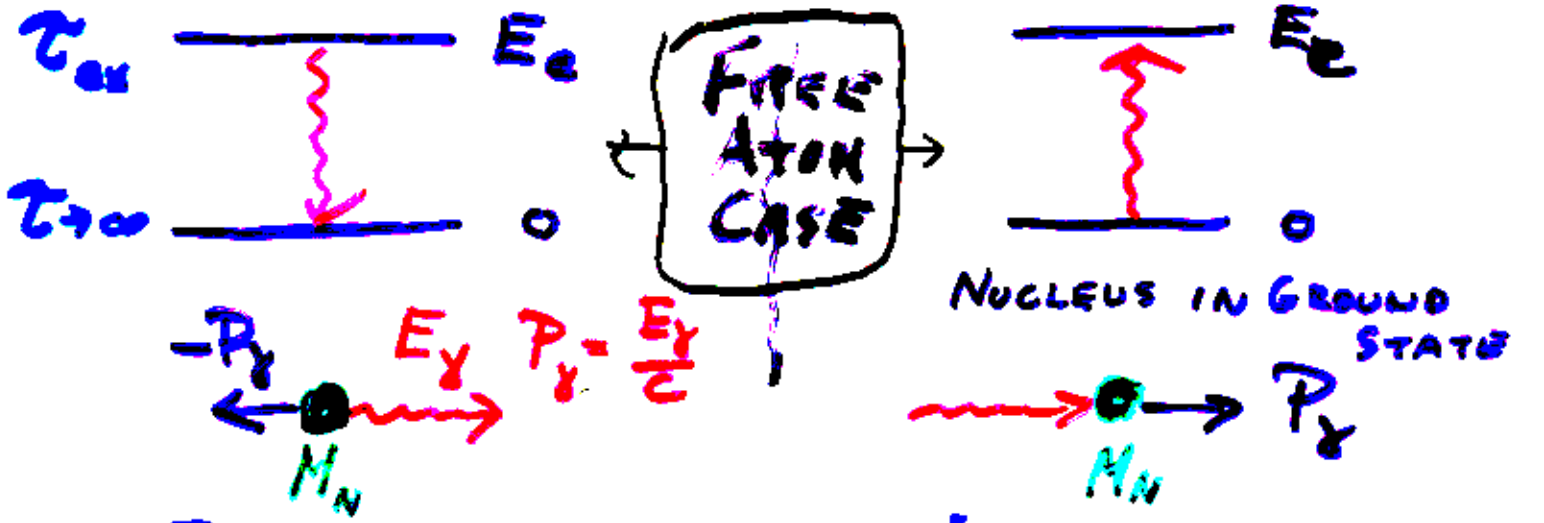
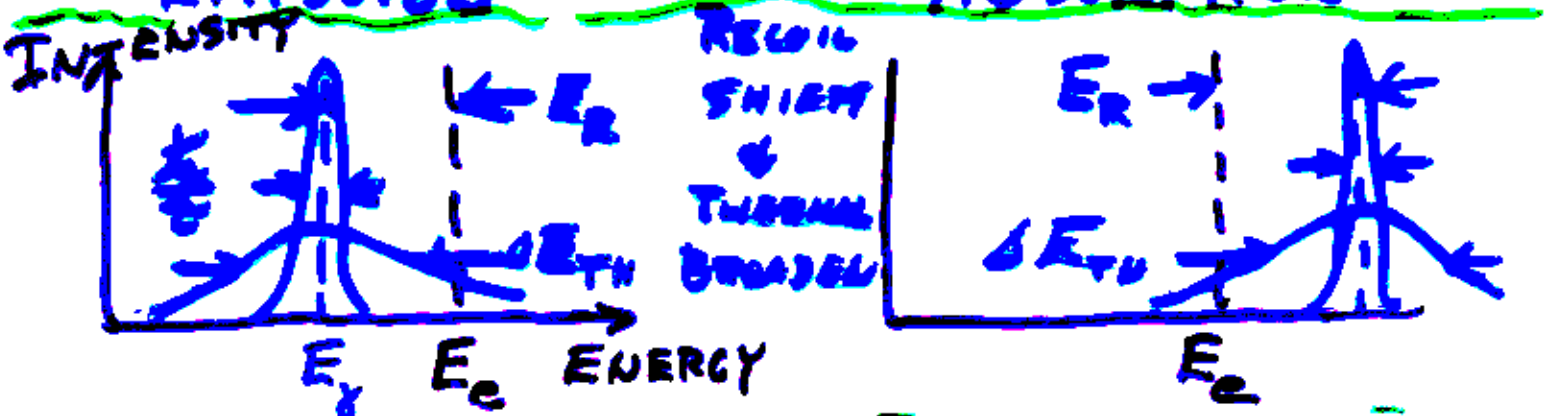


Fig. 2. Difference in intensity of the 129-kev gamma transition in Ir^{191} measured behind a resonance absorber (iridium) and a comparison absorber (platinum). This intensity difference is plotted as a function of v, the relative velocity of the source with respect to the absorber. Both source and absorber are at a temperature of 88°K. $\Delta E = (v/c)E_0$ denotes the energy shift of the 129-kev gamma rays.

129-kev transition in Ir^{191}. Thus, a new method for the direct determination of the level widths of low-lying excited nuclear states has been found. In our case, the line width of the 129-kev level in Ir^{191} agrees, within the limits of error, with the value 6.5×10^{-8} ev deter-

GAMMA RAY RESONANT ABSORPTION





$$E_R = \frac{P_\gamma^2}{2M} = \frac{E_\gamma^2}{2Mc^2} \qquad \frac{E_R}{E_\gamma} = \frac{E_\gamma}{2Mc^2} \approx \frac{10^5}{10^{11}} = 10^{-6}$$

$$\frac{\overline{\Delta E_{TH}^2}}{E_\gamma^2} \approx \frac{\overline{v_x^2}}{c^2} = \frac{kT}{Mc^2} \approx \frac{10^{-2}}{10^{11}} \;;\; \frac{\Delta E_{TH}}{E_\gamma} \approx 10^{-4}$$

IN '50s RESONANCE ABSORPTION WAS STUDIED BY COMPENSATING RECOIL BY HIGH TEMPERATURE (MALMFORS, METZGER) AND BY DOPPLER SHIFT BY A CENTRIFUGE (P. MOON, BIRMINGHAM)

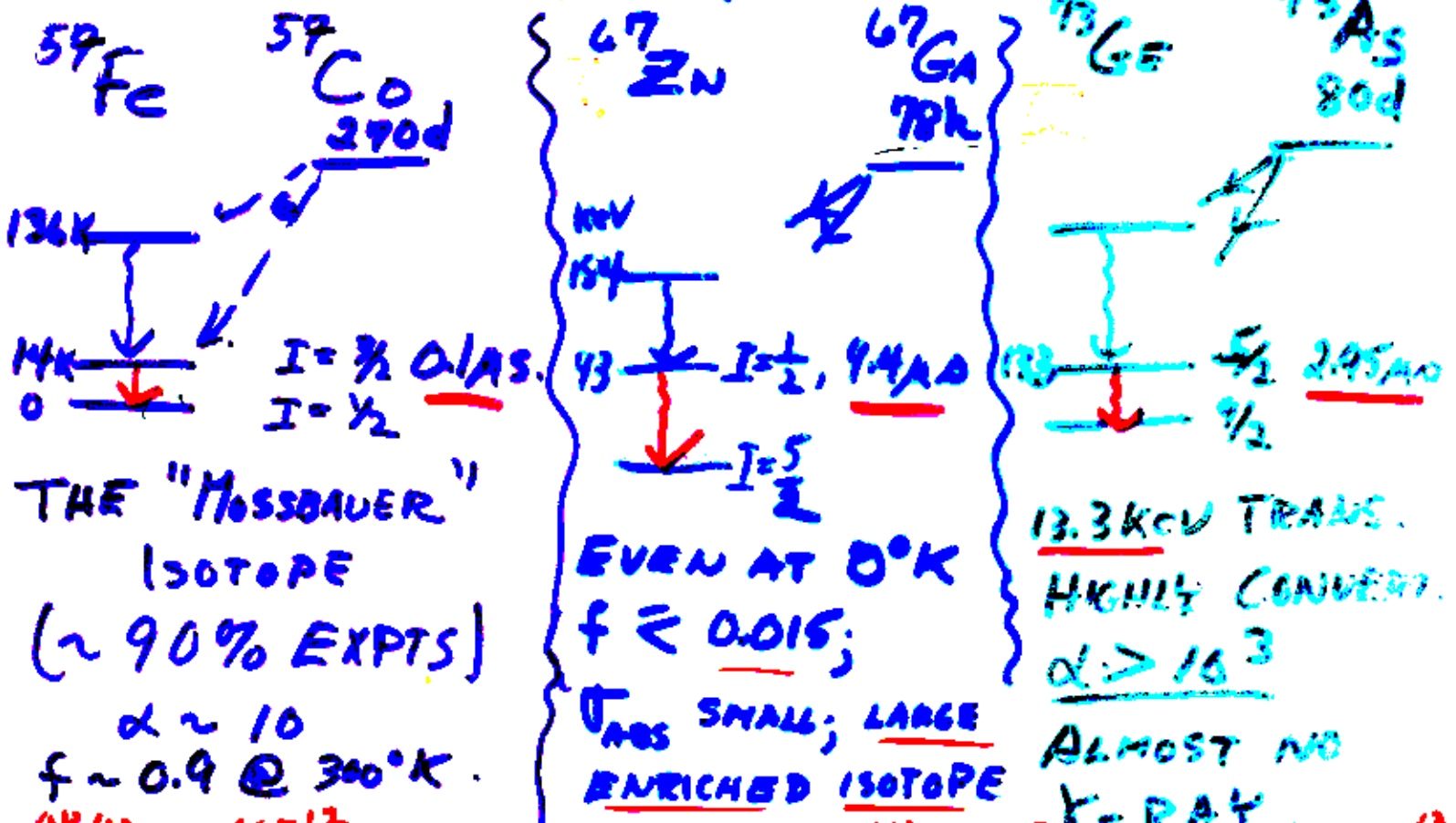
PARAMETERS NEEDED FOR HIGH RES. γ-RAY RESONANCE.

I. LOW ENERGY (< 100 KeV) LONG LIVED (T_N > 100 ns) LEVEL. (NECESSARY FOR SIZEABLE & FRACTION

II γ-RAY TO GROUND STATE OF ABUNDANT STABLE ISOTOPE - FOR ABSORB

III LEVEL FED FROM LONG LIVED (DAYS – YEARS) PARENT. ISOMER PREFERRED (BUT NOT ACHIEVED). (^{119}Sn)

1959 TABLES — REV. MOD. PHYS. - SEABORG.



THE "MOSSBAUER" ISOTOPE (~ 90% EXPTS)

$\alpha \sim 10$

$f \sim 0.9$ @ 300°K.

$\Delta\nu/\nu \sim 10^{-12}$

EVEN AT 0°K $f \leq 0.015$;

T_{ABS} SMALL; LARGE ENRICHED ISOTOPE NEEDED. $\frac{\Delta\nu}{\nu} \sim 10^{-13}$

13.3 KeV TRANS. HIGHLY CONVERTED. $\alpha > 10^3$

ALMOST NO γ-RAY.

BEGINNING MID OCT, '59 - TRY

RESONANT EXAMPLES ^{57}Fe

& ^{67}Zn. G.A. REBKA SWITCHED OVER.

FIRST EXPTS - ^{57}Co MADE

BY ^{56}Fe (d,n) $\rightarrow$ ^{57}Co

BUT N.G. - 76 DAY ^{56}Co FAR

MORE ACTIVE. (MIT CYCLOTRON)

AFTER ~ 2 WEEKS - LEARNED

(L. GRODZINS)

PURE ^{57}Co AVAILABLE FROM

NEW ENG. NUCLEAR - BOSTON!

^{58}Ni (p, 2n) $\rightarrow$ ^{57}Co.

ABSORBERS - THIN PURE IRON

G.A. REBKA > PENT NIGHTS ROLLING

& ANNEALING & ROLLING - TO 0.001"

Moving Coil Magnetic Transducer with ^{57}Fe Source & Armco Iron Foil Absorber From W.W. II PM Altimeter

FIRST — VIBRATING VS.
FIXED SOURCE; ~10% INCREASE
IN γ-RAY TRANSMISSION.

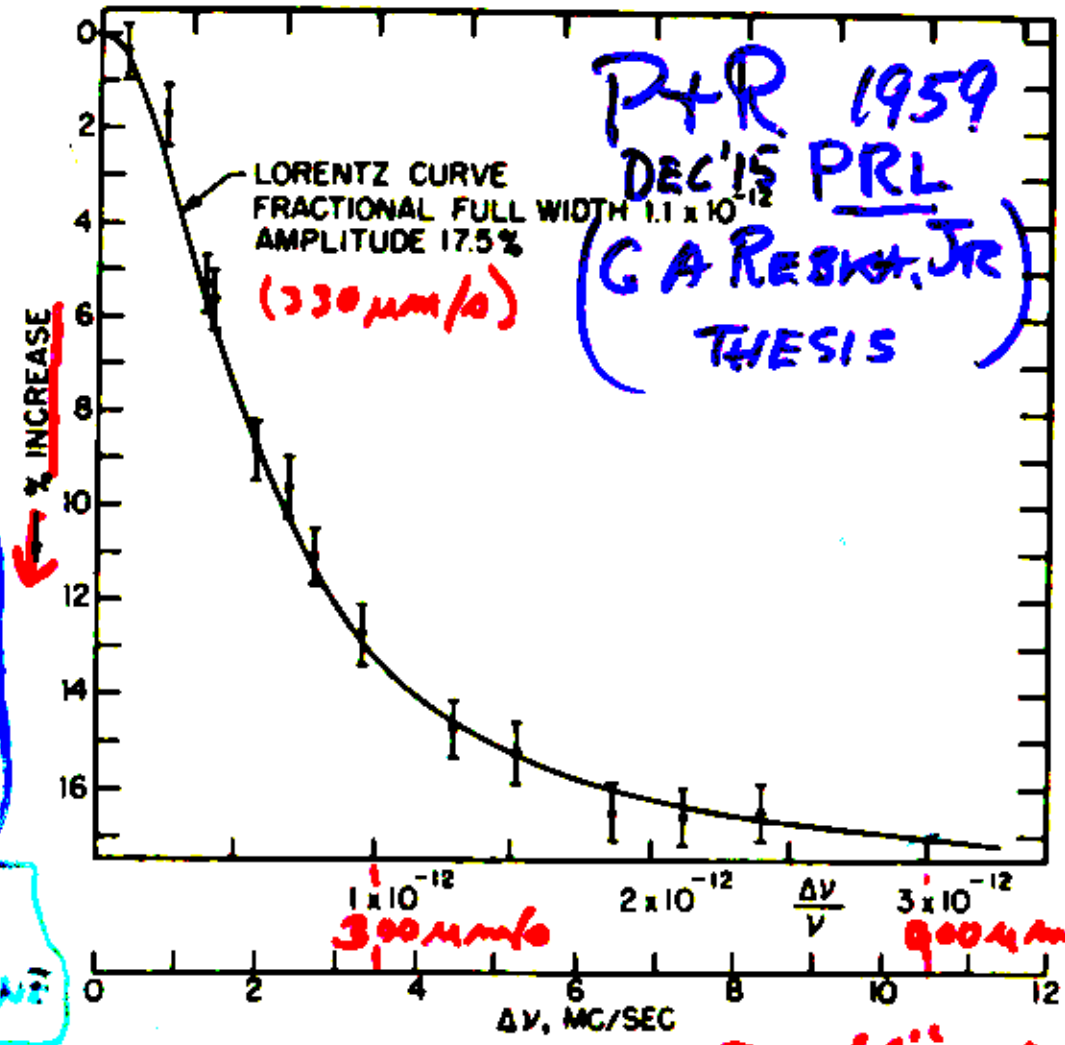
BORROWED FUNCTION GEN.
FROM MALLINCKRODT (CHEM.)

DISPLACEMENT → VELOCITY.

ABSORPTION VS. VELOCITY
+ h.f.s FROM MAGNETIC
FLUX AT NUCLEI.

G. REBKA DEVELOPED ELECTROPLATING
TECHNIQUE - CARRIER FREE ^{57}Co ONTO
IRON FOILS. DIFFUSING Co INTO IRON
FOILS BY HEAT TREATMENT IN QUEN.

1ST PUBLISHED DATA FOR ^{57}Fe



P+R 1959
DEC'S PRL
(G A Rebka, Jr
THESIS)

↑ ABSORB

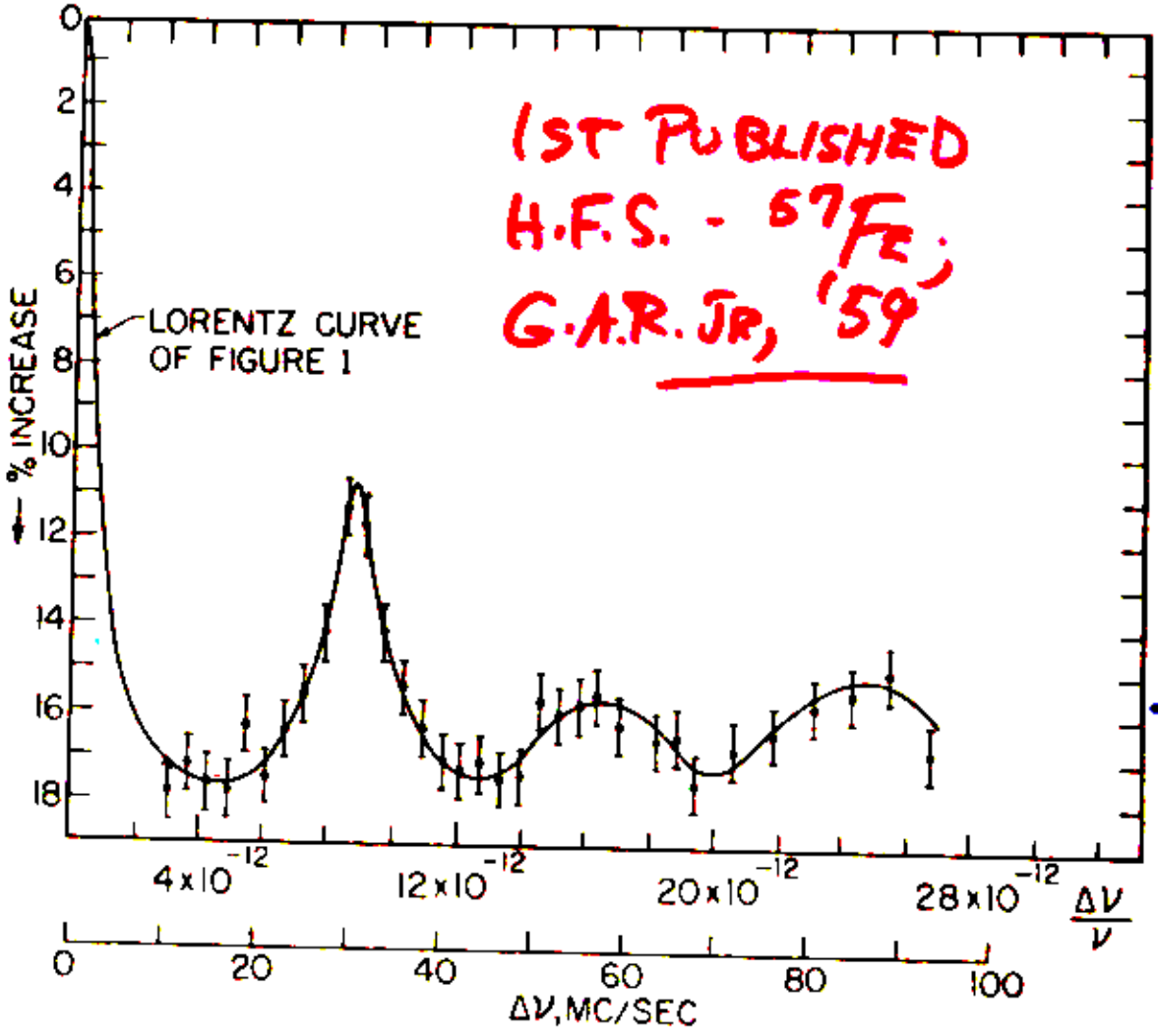
ALSO
SCHIFFER & MARSHAL
HARWELL
OBSERVED ONLY
INCREASED TRANS
SOURCE VIBRATING
VS
SOURCE STATIC
DEDUCED Δν, h.f.s.

WORK STARTED
BNL, ILL, ARGONNE
ETC.

NOW KNOWN AS THE MÖSSBAUER ISOTOPE

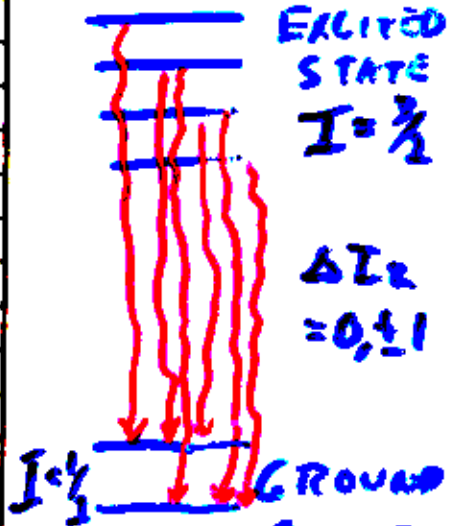
WORKHORSE ISOTOPE
>90% EXAMPLES

Figure 5



1ST PUBLISHED
H.F.S. - ^{57}Fe.
G.A.R. JR, '59

(POST FACTO)
LEVELS SPLIT BY INTERNAL MAGNETIC FIELD B.



SIX γ RAYS OF DIFFERENT ENERGY.

Figure 7

PALMER PHYSICAL LABORATORY
PRINCETON UNIVERSITY
PRINCETON, NEW JERSEY

(WE ARE NOT ALONE)

November 12, 1959

Professor R. V. Pound
Department of Physics
Harvard University
Cambridge, Massachusetts

Dear Bob:

I note from your recent note in Physical Review Letters that we have been inadvertently treading on each others research. For the past couple of months Ken Turner has been working full time on the very problem you discuss. We are hoping for a laboratory experiment with a few meters height difference. I am somewhat at a loss to know if we should continue. The problem is very difficult and perhaps should be tackled at more than one place. Perhaps a cooperative venture would be best. On some days I think that no one in his right mind should try to do such an experiment anywhere. I would be interested in your comments.

Sincerely,

R. H. Dicke

RHD:ljb

WA 44735
Home

November 17, 1959

Professor R.H. Dicke
Palmer Physical Laboratory
Princeton University
Princeton, N.J.

Dear Bob:

As you will have gathered, I am quite thoroughly involved in this project and have, now, reason to be optimistic about not only detecting but measuring the effect. I have put a considerable effort into it in the past six weeks and so far as I know we are ahead of others. In effect we have advanced the technique by about a factor of 100 so far and have a reasonably quantitative notion of when it works and doesn't. This week we expect to try for the other factor of about 600. Irrespective of the outcome, I have already put in motion the setting up of a baseline in the inner tower of the old Jefferson Laboratory where we will have about 27 meters of height.

I am sorry we found ourselves working on such similar projects. I have been surprised not to have found others before. We are submitting a letter on our results on Fe^{57} which now shows a basic width about 1.5 times theoretical, or 0.015 cm/sec and, so far, three hyperfine satellites. We will, with more data, improve the details of this latter feature.

For some time I have been meaning to bring up the point that it's about time I asked you for a colloquium talk again. Today I am writing Wigner, who I understand will be here in January anyway. Perhaps you would come and tell us something of all or some of your projects in February or so? I have heard [illegible] quote you as having improved the Eötvös experiment to 10-12 and would like to hear about it.

Let me know if you are interested to come. We can cover travel expenses, of course. My apologies to Mr. Turner. I dislike the business of publishing a proposal like that but, in this case, perhaps it is better to know at this end. Now that I feel we have advanced the art significantly ourselves I do feel a little bit better about it.

Regards,

R.V. Pound

RVP/ar

Columbia University
in the City of New York
(NEW YORK 27, N. Y.)
DEPARTMENT OF PHYSICS

Nov. 17th 1959

Dear Bob,

I've just seen your letter — Red-shifts etc. in Phys. Rev. Letters. I thought you might care to know that preparation for an experiment of this sort was started at Manchester some months ago by A.J. Boyle, D. Bunbury & others.

We have not made any secret of our plans (they were discussed at a seminar in Manchester in July or August last, and also with several people here, C.S. Wu, A.J. [illegible] in the past two or three months), but it did not seem proper to me to publish simply our intentions, particularly since the real work will lie in the accomplishment & in solving the many problems that will arise.

I hope to see you again sometime [illegible] time in the U.S.

Best wishes

Sam Devons

MAJOR PROBLEMS - GIVEN ^{57}Fe
(IN PARALLEL WE TRIED $^{67}Ga \rightarrow {}^{67}Zn$)

1. CONSTRUCT VERTICAL PATH
   USE He GAS IN PLASTIC BAG.
   (JEFF. TOWER 1884)

2. OBTAIN LARGE AREA ^{57}Fe
   ENRICHED ABSORBER (NAT 2%
   - RAISE TO ~ 50%).
   PLATING BERYLLIUM DISKS.
   (NO VIBRATION.) (NUCLEARMET)

3. HIGH INTENSITY SOURCE
   - OAK RIDGE 0.4 CURIES.
   - PLATING ^{57}Co ON IRON -
   DIFFUSING INTO IRON.

4. 7. SCINTILLATION NaI(Tl)
   DETECTORS (GLEN BUILDING CIRCUITS).

N.Y. TIMES

November 22,

Dr. Robert V. Pound
Department of Physics
Harvard University
Cambridge 38, Massachusetts

Dear Dr. Pound:

Thank you for discussing with me last week the experiment which you plan as a test of gravitational red shift. It seems to me a most interesting line of research.

It is possible that I may come to Boston next week to attend the Eastern Computer Conference, December 1,2,3, sponsored by the Institute of Radio Engineers and others.

If so I would greatly appreciate having a chance to meet you to discuss your report in Physical Review Letters and the red shift experiment As I mentioned during our telephone conversation I would like to write a story for The Times on the project. In the interests of accuracy I would very much prefer to do this on the basis of a more thorough interview than is possible over the telephone.

When my schedule becomes definite I will call you in the hope of arranging a meeting at some time convenient to you.

Sincerely yours,

Harold M. Schmeck Jr.

Harold M. Schmeck, Jr.

UNITED KINGDOM ATOMIC ENERGY AUTHORITY

TELEGRAPHIC ADDRESS: ATEN, ABINGDON, TELEX
TELEPHONE: ABINGDON 1220

Theoretical Physics Division,
ATOMIC ENERGY RESEARCH ESTABLISHMENT,
HARWELL,
DIDCOT, BERKS.

OUR REFERENCE:

YOUR REFERENCE:

2nd December, 1959.

Professor Pound,
Department of Physics,
Lyman Laboratory of Physics,
Cambridge 38,
Massachusetts,
U.S.A.

Dear Professor Pound,

It is amusing that you ask if I am interested in these results because we have been trying the same experiment for some weeks now. We tried very hard to race you, so hard in fact, the experimentalists forcibly co-opted me as an assistant for some of the time. We started the experiment some considerable time ago, just after John Schiffer arrived here for a year's sabbatical leave from Argonne and introduced us to the subject, but we have been continuously frustrated by one "trivial" difficulty after another. We also spent two or three weeks under the delusion that we were the first to think of the red shift experiment. But then almost simultaneously yourPhysical Review Letter arrived and we heard that Boyle in Manchester had been contemplating the experiment for some months earlier.

There is no hope whatsoever of calculating the hyperfine field in a ferromagnet and so being able to deduce nuclear moments from h.f.s. splittings. I wrote a paper on this subject a year ago [*P.R. 110, 1280* No reprints left I'm sorry] and a glance at it will explain the horrible difficulties involved. You will notice that I say nothing about Fe in that paper for the good reason that Fe is even

UNITED KINGDOM ATOMIC ENERGY AUTHORITY

TELEGRAPHIC ADDRESS ATEN, ABINGDON, TELEX
TELEPHONE: ABINGDON 1220

ATOMIC ENERGY RESEARCH ESTABLISHMENT,
HARWELL,
DIDCOT, BERKS

OUR REFERENCE:

YOUR REFERENCE:

The enclosed letter is what we submitted two weeks ago. It seemed useful at the time but it is nothing compared to your results, but we can't withdraw it now.

Boyle (in Manchester) did not realise the enormous advantage of using Fe^{57} for the red shift experiment, he is using Zn and so is having a much harder time of it.

I am amused at the long derivations and discussions the nuclear physicists give for the probability of recoilless emission of γ rays. The probability is, of course, just the well known Debye-Waller factor which comes into x-ray and neutron scattering; but so small is the overlap between solid state and nuclear physicists that what is an extremely familiar concept to the former is something quite new to the latter - and also vice-versa at times I am sure.

We are at present working on the red shift experiment, and on the rotating centrifuge experiment and are embarking on a long series of solid series state experiments which I foresee will keep us busy for several years! I will keep you in touch with our progress.

As of 1982 "Sir Walter"

No. 1 of Manchester Group. Best wishes,

Walter Marshall

Unhappily now deceased.

UNITED KINGDOM ATOMIC ENERGY AUTHORITY

TELEGRAPHIC ADDRESS ATEN, ABINGDON, TELEX
TELEPHONE: ABINGDON 1220

OUR REFERENCE:

YOUR REFERENCE:

Nuclear Physics Division,
ATOMIC ENERGY RESEARCH ESTABLISHMENT,
HARWELL,
DIDCOT, BERKS.

11th December, 1959

Prof. R.V. Pound,
Lyman Laboratory of Physics,
Harvard University,
Cambridge 38,
Mass.,
U.S.A.

Dear Prof. Pound,

I was very much interested in your two letters concerning the red shift and Co^{57}. We had also found the big effect in Co^{57} in September and thought of doing the red shift, but we did not work at it very hard at the beginning. Seeing your first Letter provided a considerable stimulus but I still found that things went a little slow, primarily because of the obvious difficulties of a visitor in a strange place.

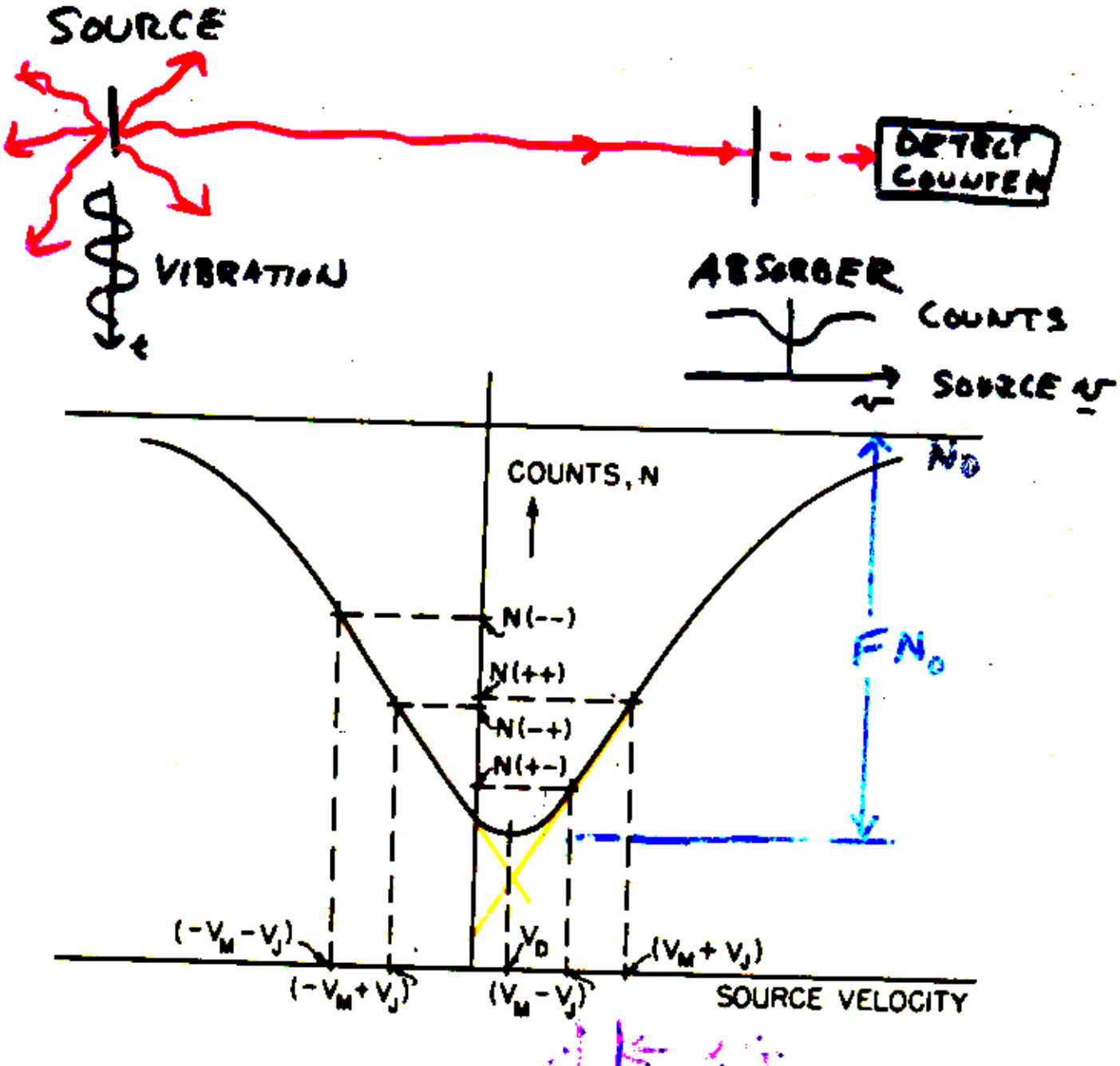
Walter Marshall mentioned to you the solid state experiments that we are interested in. We are also planning to measure the red shift in both a natural and artificial gravitational field.

I feel a little bit that perhaps it is better not to exchange detailed information on experimental technique at this point so that we can arrive at two truly independent determinations.

Enclosed is a copy of the Letter we sent to P.R.L.

Yours sincerely,

J. Schiffer



(INTERSECTION OF TWO LINES OF SLOPES $(\partial N/\partial V)_{\pm V_M}$ AS CENTER)

IMPORTANT PARAMETER V_J

(MUST BE MEASURED).

STATISTICAL STANDARD DEVIATION OF MEASURED FRACTIONAL SHIFT OF SOURCE-ABSORBER FREQ.

$$\rightarrow \delta a_0 = \left[(1 - \tfrac{3}{4}F)/3N_0\right]^{1/2} 8 v_H / 3Fc$$

F = FRACTIONAL DEPTH OF LINE

N_0 = NO OF COUNTS TO DETECTOR WITHOUT RESONANT ABSORPTION

v_H = SPEED REDUCES ABSORPTION TO 1/2 MAX.

$$N_0 = Rt(\epsilon b D)\Omega/4\pi$$

R = SOURCE STRENGTH - DECAYS / TIME

t = RUN TIME

$(\epsilon b D)$ = FRACTION FOR CONVERSION, DET. EFFICIENCY, DUTY CYCLE.

Ω = DETECTOR SOLID ANGLE

NOTE $\Omega \propto h^{-2}$ $\therefore \delta a_0 \propto h$

$\rightarrow$ $\therefore \delta a_0 / gh/c^2 \propto h^0$

i.e. INDEPENT OF h.

GIVEN N_0, FIGURE OF MERIT $\approx (v_H/F)^{-1}$

FOR GRS APPLICATIO

← WILL COME BACK TO THIS

[Reprinted from THE PHYSICAL REVIEW, Vol. XVII., No. 4, Oct., 1903.]

DO FALLING BODIES MOVE SOUTH?

BY EDWIN H. HALL.

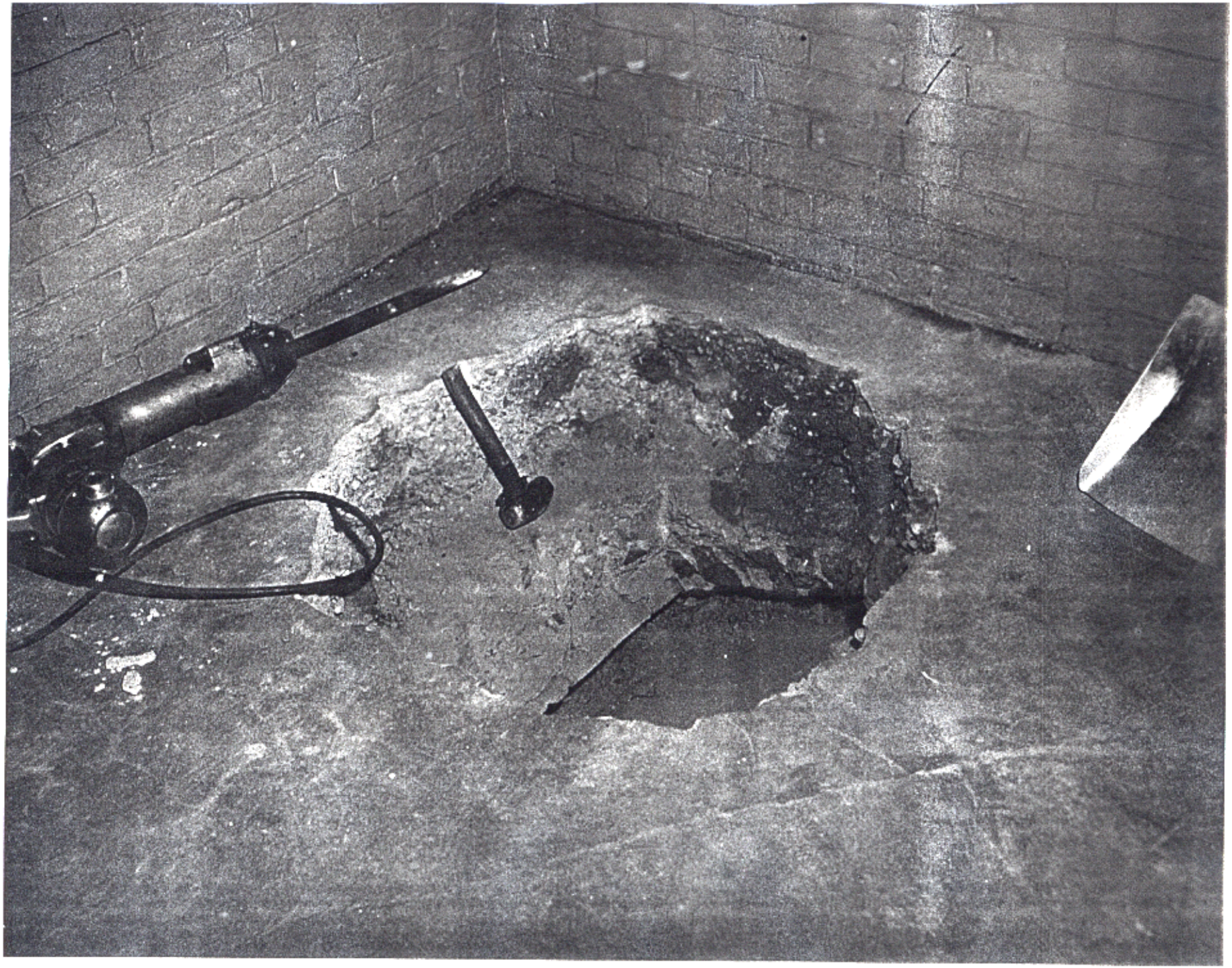
PART II.[1] METHODS AND RESULTS OF THE AUTHOR'S WORK.

THE exact place of my experiments was the axis of the inclosed, isolated tower of the Jefferson Physical Laboratory of Harvard University. The distance of fall available in this tower is very small compared with the vertical distances which most of my predecessors in this research have used; but some compensation for this disadvantage is found in the fact that the tower is very well protected from disturbances due to winds or lateral inequalities of temperature, and in the further fact that in working within a laboratory I could have at hand all needed appliances and assistance.

The general plan and arrangement of apparatus is indicated by Fig. 1. At the top a wooden box *a*, having a trap door on one side and a window on the other, carries on its upper surface a contrivance of brass, in contact with which the ball to be released, a sphere of bell-metal 2.54 cm. in diameter, is held (see Fig. 2). The box *a* is supported by means of two strong joists, not shown, the ends of which rest upon and are fastened to a railing, which crowns the tower. Beneath *a*, and separated from it by a layer of cotton, is a square of board, *b*, having a central circular hole about 34 cm. in diameter, from the circumference of which is suspended a tube, *c*, of seamless cotton cloth ("pillowcasing") of the same diameter as the hole. The board *b*, is supported from the roof over the tower, and therefore has no rigid connection with *a* save by way of the base of the tower. The cloth tube, distended by five circular ribs of stout wire, extends downward about 20 m. to *d*, a rectangular opening, about 30 cm. by 40 cm., in the vaulted ceiling of the con-

Fig. 1.

[1] A third paper on Preliminary Experiments, Variations of Method, etc., in the author's work will be published in the Proceedings of the American Academy.



Nuclear Science and Engineering Corporation

P. O. BOX 10901, PITTSBURGH 36, PENNSYLVANIA

HOMESTEAD 2-4000

DEPARTMENT OF CHEMISTRY

November 30, 1959

Dr. R. V. Pound
Department of Physics
Lyman Laboratory of Physics
Harvard University
Cambridge 38, Massachusetts

Dear Dr. Pound:

Thank you for your letter of November 23, 1959 regarding Co^{57}.

The procedure which you outlined in your letter is entirely satisfactory. I have initiated the procedures for production of 200 millicuries of Co^{57}.

I intend to purify the nickel by ion exchange and electroplate onto a copper target backing.

The price of $4,000.00 covers preparation of an essentially cobalt-free nickel target, production, separation and purification of 200 millicuries of Co^{57}, and plating the 200 millicuries of Co^{57} onto an iron backing. It is my understanding that you will supply the iron and a plating procedure, and that the plating procedure will not require the expenditure of more than approximately four man-hours of effort.

I trust that the above meets with your approval, and thank you for your continued interest.

Very truly yours,

I. Gruverman
Group Leader
Radioisotope Sales

IG: rcw

OAK RIDGE NATIONAL LABORATORY
OPERATED BY
UNION CARBIDE NUCLEAR COMPANY
DIVISION OF UNION CARBIDE CORPORATION
UCC
POST OFFICE BOX Y
OAK RIDGE, TENNESSEE

22 December 1959

Professor R V. Pound
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts

Dear Professor Pound:

The Co^{57} that you will get from NSEC will have been made in a bombardment on the ORNL 86-Inch Cyclotron. Because of our interest in the results of your experiment, we have scheduled a special run all day on 24 December. We have the target on hand - NSEC did an excellent job of plating - and we expect no difficulty. When your work is published, we will appreciate receiving credit for the bombardment.

Sincerely yours,

John L. Need

John L. Need
Electronuclear Research Division

JLN:mlw

PUBLICITY HELPED FOR ONCE HERE,
HEAT TREATMENT - DIFFUSION UP
TO US. HYDROGEN FURNACE IN
TUBE SHOP AT MIT, F. ROSEBERRY.

NUCLEAR METALS, INC.
METALLURGICAL RESEARCH AND DEVELOPMENT

CONCORD, MASSACHUSETTS
EMERSON 9-5410

Professor R. V. Pound
Lyman Laboratory 235
Harvard University
Cambridge, Massachusetts

December 24, 1959

Subject: Coating of Beryllium with Fe^{57}
NDD No. 6290

Dear Professor Pound:

In accordance with your telephone discussions of December 11, 1959 with our Dr. W. B. Nowak, Nuclear Metals understands that you may require an Fe^{57} coating equivalent to 1 to 2 mg/cm^2 on a beryllium blank. The blank would be in the form of a 3-inch diameter disc approximately 1/32 inch thick.

We propose to investigate methods for obtaining such a coating. Two methods which appear to offer some possibilities are electroplating and the applications of fine particles from a slurry media. These alternatives will be investigated through the full-time efforts of one senior staff scientist for a period of one week. This exploratory work will be performed on a time and materials basis, and we propose that you authorize charges for professional services which will not exceed $800.00. Disbursed expenses such as for materials will be separately invoices, in addition, at cost to us.

If a potentially successful method is found, it is our understanding that you may wish to proceed further and that, in particular, you may require seven discs coated with 1 to 2 mg/cm^2 of Fe^{57}. We will be prepared to perform this work and will quote a price after completion of the present exploratory effort.

If the foregoing proposal and statement of understanding is satisfactory to you, please indicate your assent by countersigning the attached copy of this letter in the space provided below and return this copy to us.

Very truly yours,

NUCLEAR METALS, INC.

J. L. Klein

By: J. L. Klein
Vice President

Attest:

Accepted and Agreed

Harvard University

By:_____ ATTEST:

"PENTHOUSE"

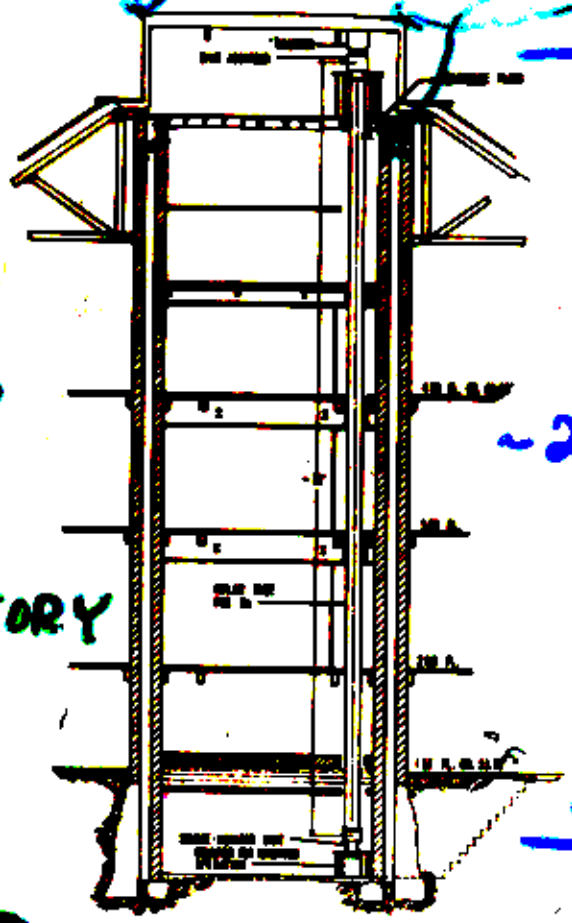
ISOLATED TOWER WALL

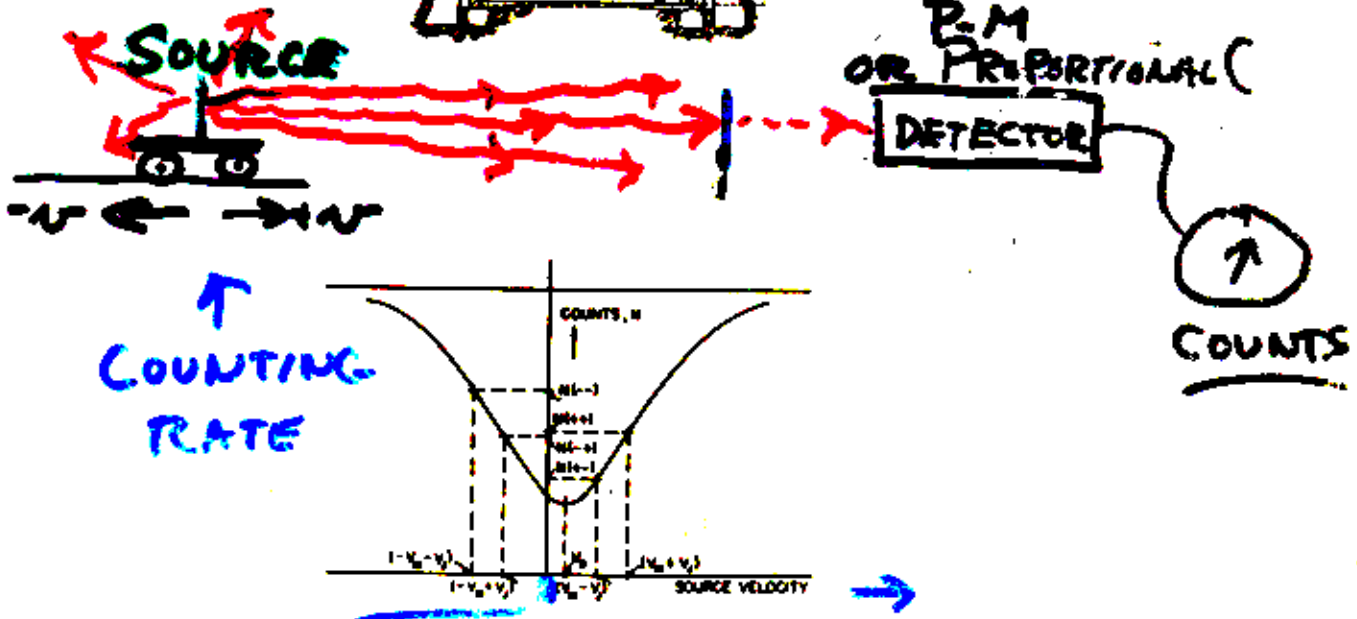
NO HEAT

BUILT 1884

~22.5'

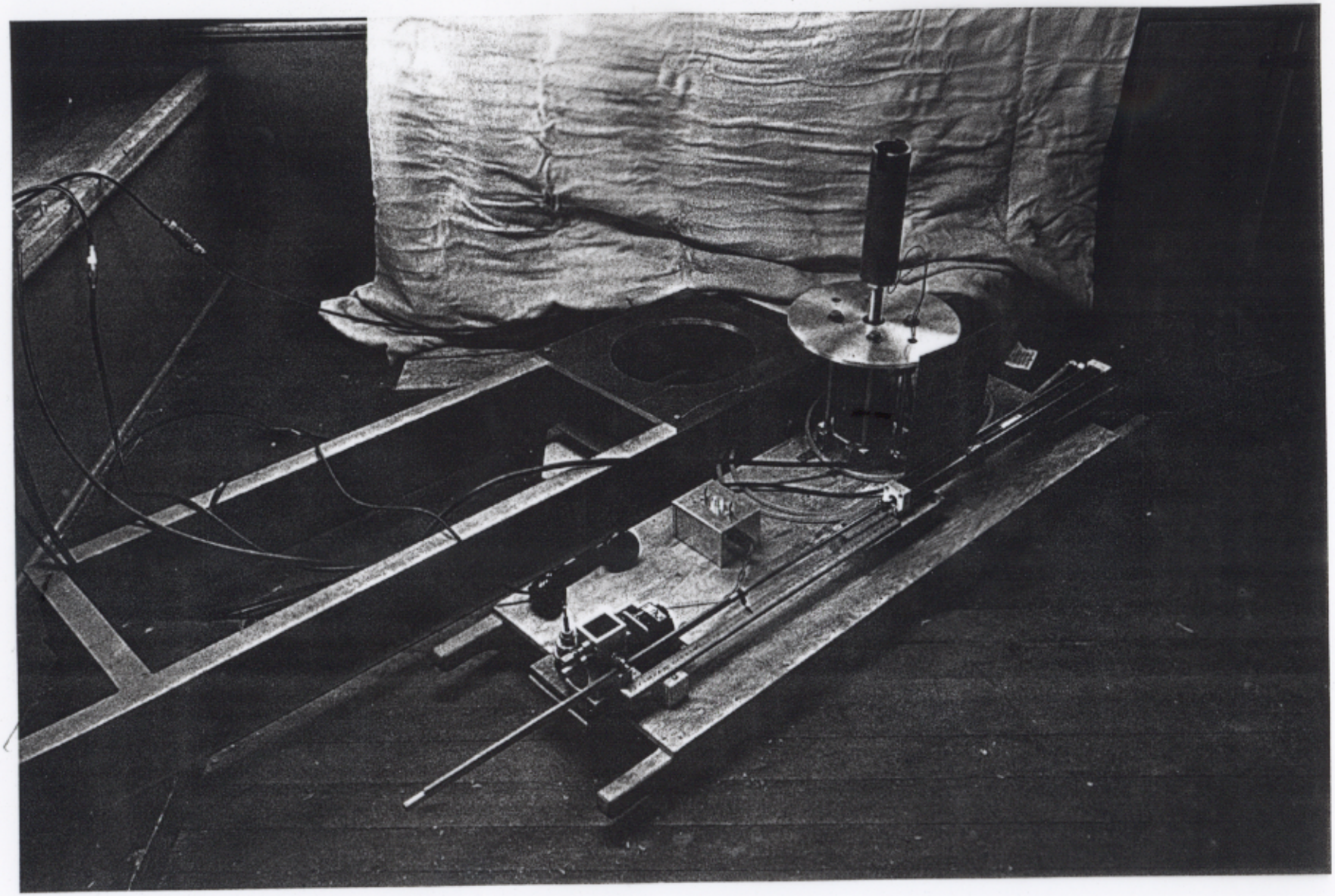
SECTION THROUGH WEST END JEFFERSON PHYSICAL LABORATORY

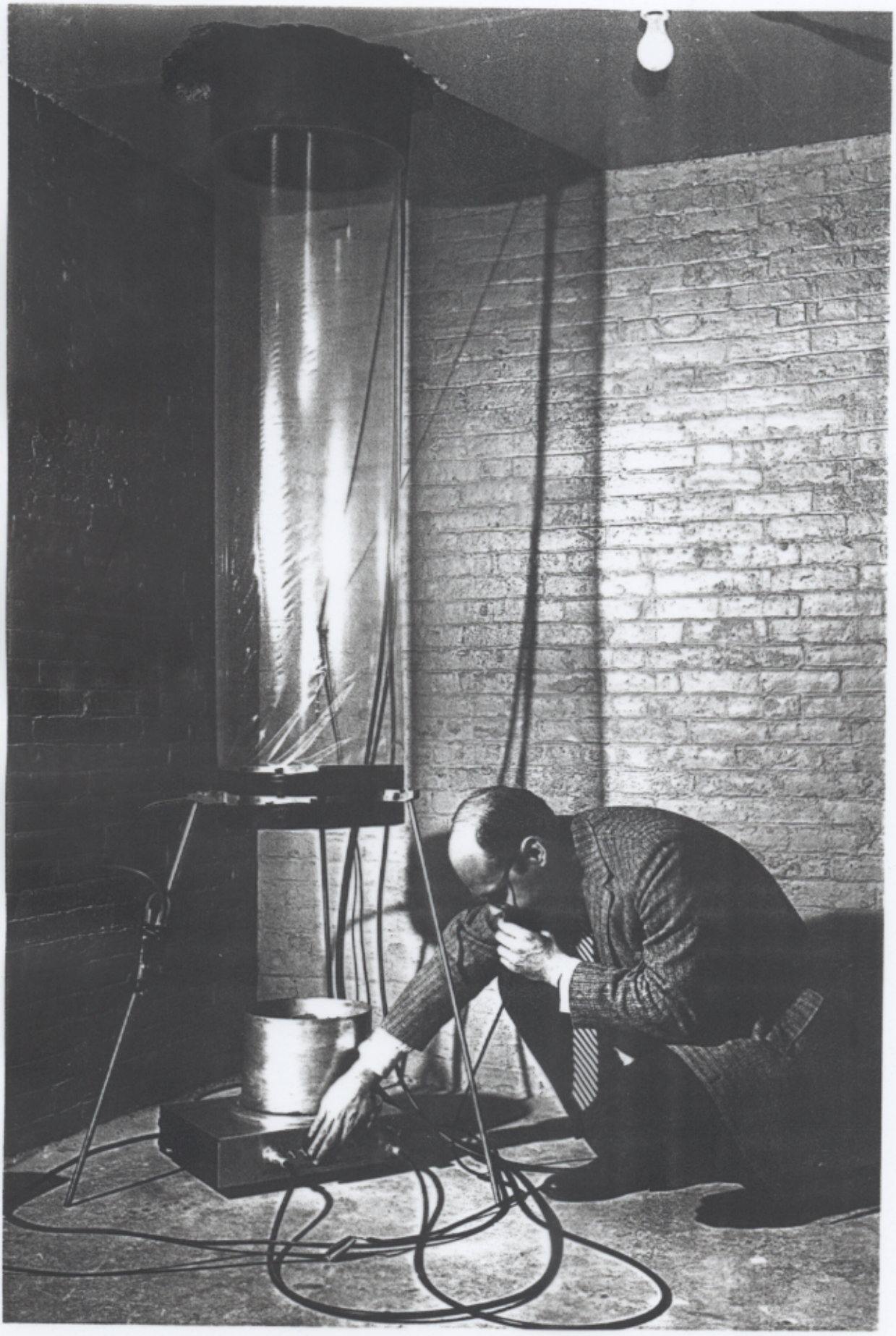


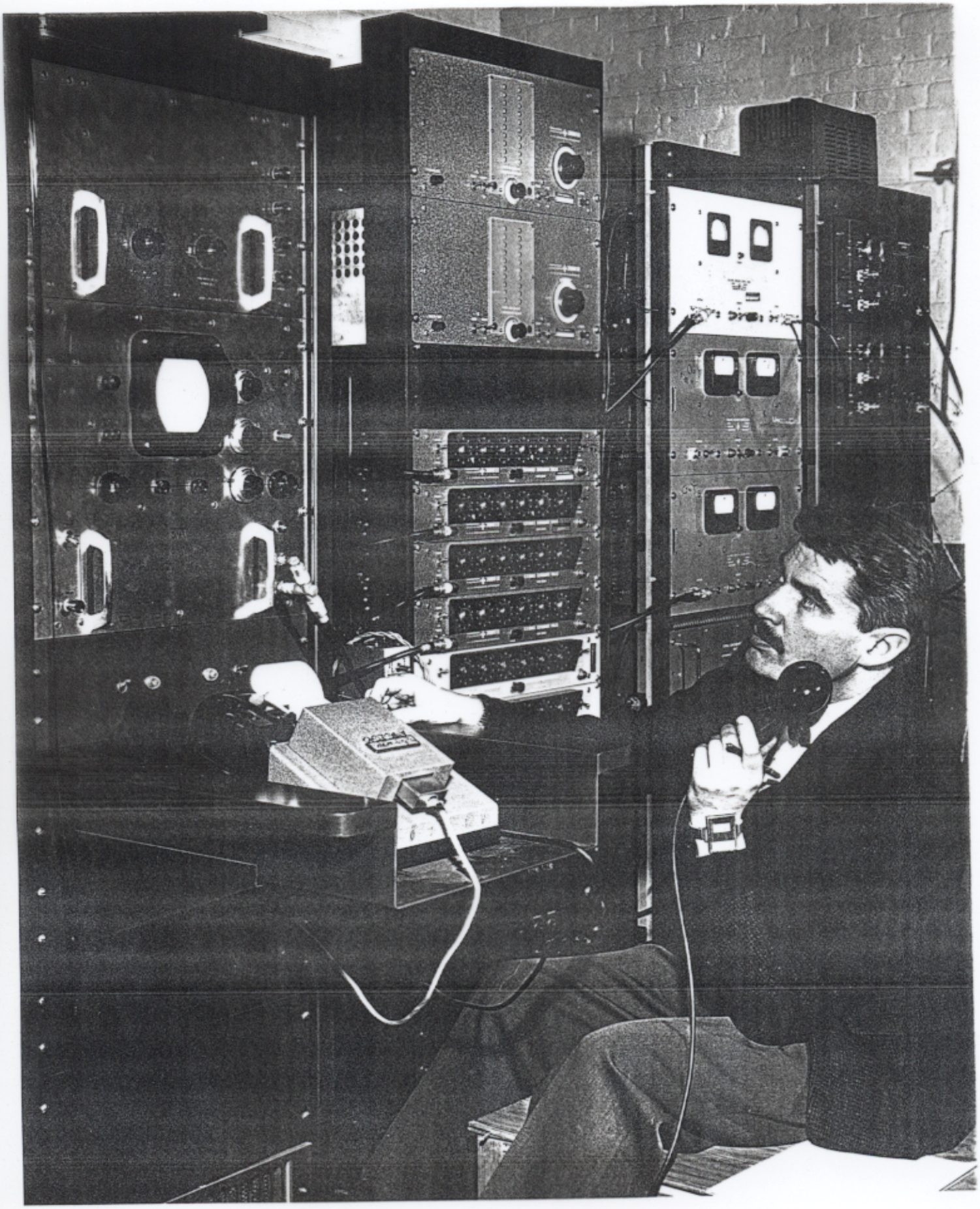


SOURCE VELOCITY

V_D DETERMINED IN TERMS OF V_T







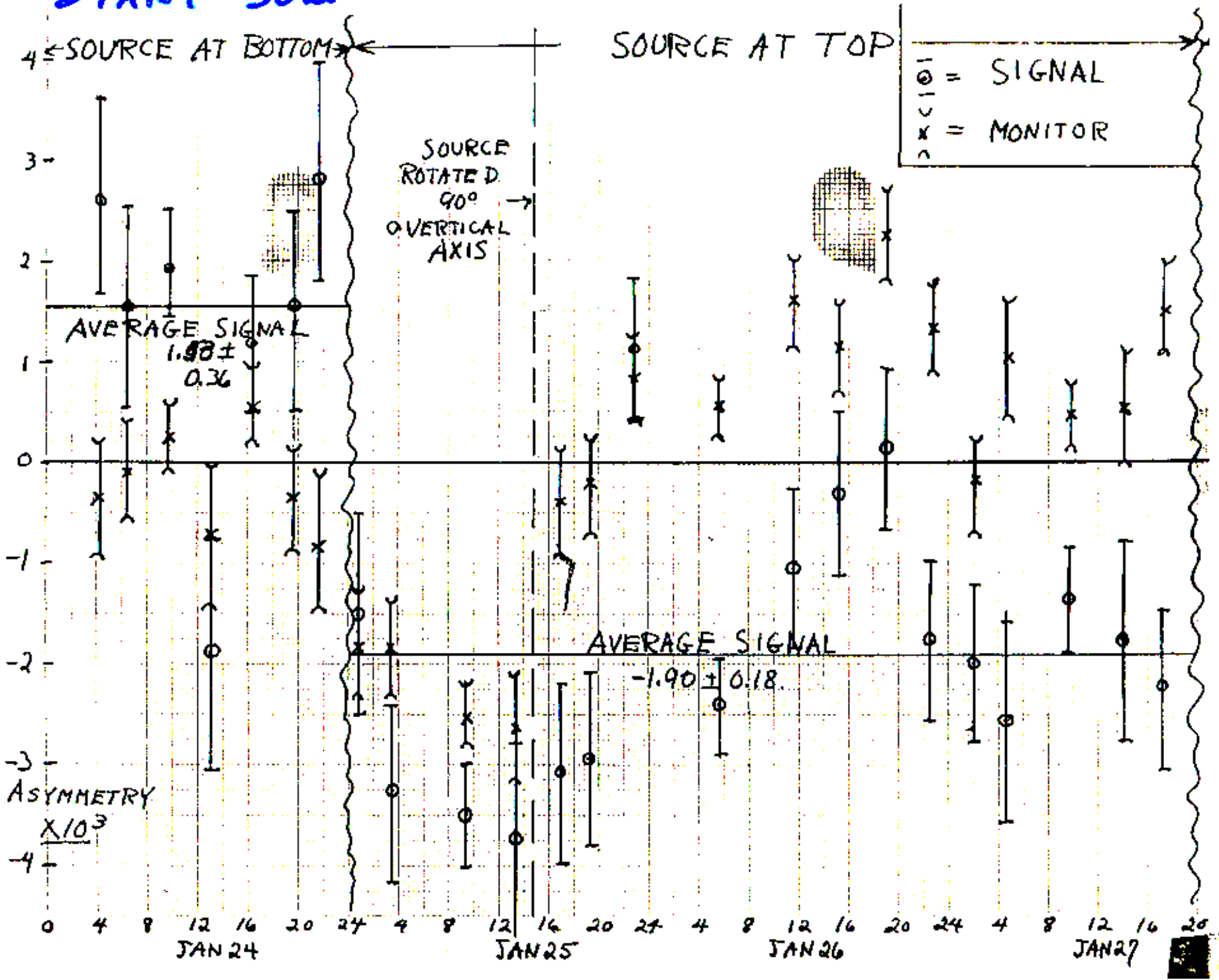
INITIAL OPERATIONS - JAN 20'60

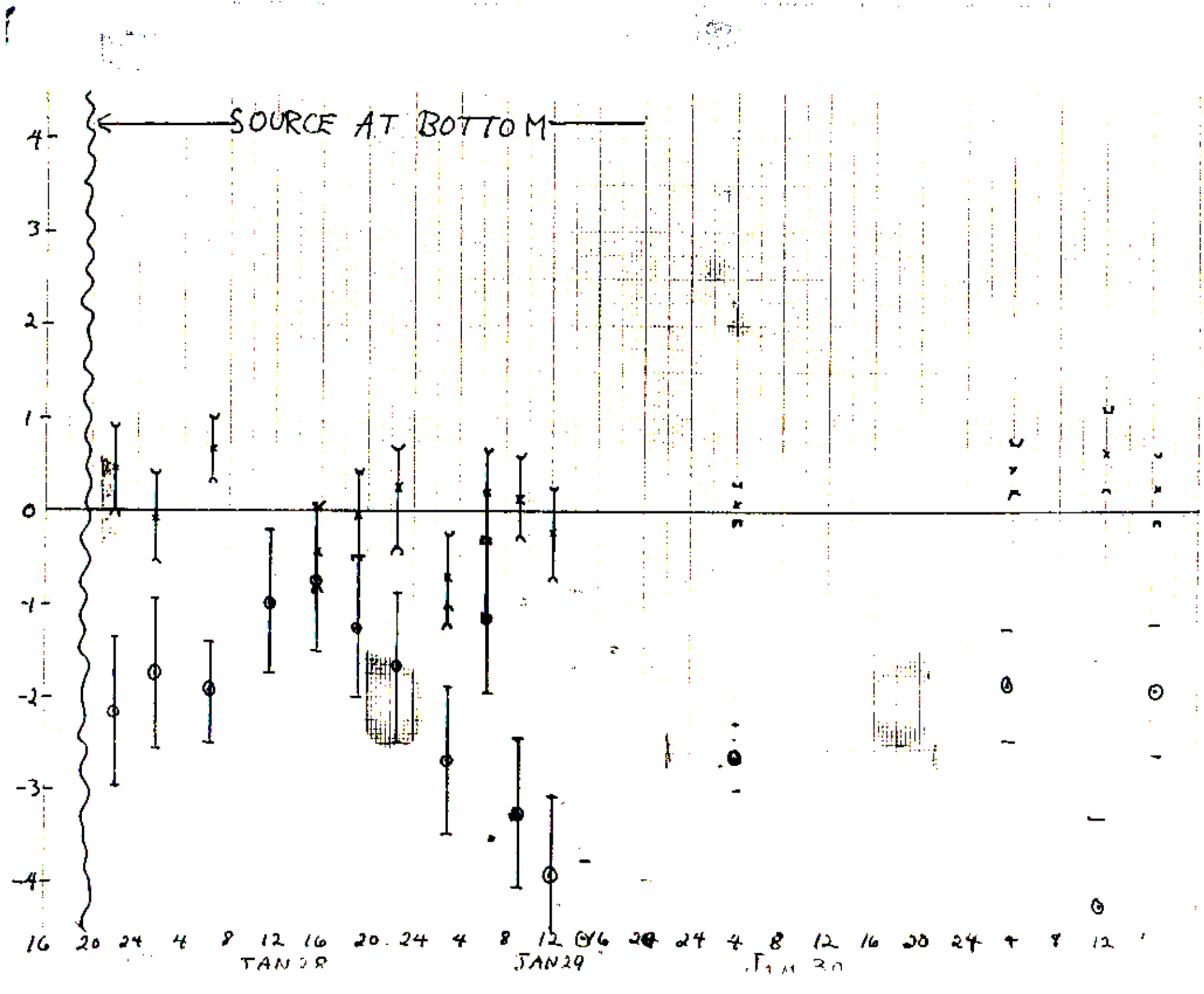
FOUR (OF SEVEN) ABSORBERS

PUBLICITY — APS WANTS NEWS CONFERENCE - SAT.

UNIV. NEWS OFFICE PUSHING FOR RELEASE. FRI - NOT STABLE ENOUGH. STATISTICS OF COUNTS SHOULD LEAD TO $\Delta V/V < 0.10$ BUT DAY TO DAY UNSTABLE

APS MEETING — HARWELL — (J.P. SCHIFFER, T. CRANSHAW) - 30 DAYS .93 ± 0.43 ← STAT. ONLY — COULD NOT HAVE SEEN DAILY FLUCTUATIONS


START - SOU
SOURCE AT BOTTOM
SOURCE AT TOP
⊙ = SIGNAL
× = MONITOR
SOURCE ROTATED 90° OVERTICAL AXIS
AVERAGE SIGNAL 1.98 ± 0.36
AVERAGE SIGNAL -1.90 ± 0.18
ASYMMETRY X10³
4
3
2
1
0
-1
-2
-3
-4
0 4 8 12 16 20 24 4 8 12 16 20 24 4 8 12 16 20 24 4 8 12 16 20
JAN 24
JAN 25
JAN 26
JAN 27


SOURCE AT BOTTOM
4
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16 20 24 4 8 12 16 20 24 4 8 12 16 20 24 4 8 12 16 20 24 4 8 12
JAN 28
JAN 29
JAN 30

ANOTHER ONE!

M.I. Podgoretsky
F.L. Shapiro

Joint Institute for
Nuclear Research
Head Post Office
P.O. Box 79
Moscow, U.S.S.R.

January 20, 1960.

Prof. R.V. Pound

Lyman Laboratory of Physics
Harvard University, Cambridge,
Mass., U.S.A.

Dear Professor Pound,

We enclose a preprint of a letter to the Editor of JETP on possible use of resonant gamma-ray scattering for measurements of fine displacements of nuclear energy levels (Zeeman effect, gravitational red shift, etc.). Apparently, following Mössbauer's findings, analogous ideas occurred independently to a number of physicists in several different countries.

Since we have initiated some experiments along the indicated lines, we would highly appreciate any exchange of preprints pertaining to these studies.

Sincerely yours,

M. Podgoretsky

F. Shapiro

THERMAL TIME DILATION

DISCOVERED IN COURSE OF EFFORTS!

$$f_{OBS} = f_0 \left[1 - \left(\frac{v}{c}\right)^2\right]^{1/2} \approx f_0 \left(1 - \frac{\overline{v^2}}{2c^2}\right)$$

$$\frac{1}{f_0}\left(\frac{\partial f}{\partial T}\right) \approx -\frac{1}{2c^2}\frac{\partial \overline{v^2}}{\partial T} = -\frac{1}{Mc^2}\frac{\partial\left(\frac{1}{2}M\overline{v^2}\right)}{\partial T}$$

$$\therefore \frac{1}{f_0}\left(\frac{\partial f}{\partial T}\right) \approx -\frac{C_L}{2Mc^2}$$

C_L = LATTICE SPECIFIC HEAT

DEBYE, IN DETAIL;

6 CAL/MOLE DULONG + PETIT - CLASSICAL LIMIT

$$\frac{1}{f_0}\left(\frac{\partial f}{\partial T}\right) \rightarrow -2.45 \times 10^{-5}/^\circ C$$

FOR $M = 57$

FOR Fe.

(MEASURE!)

(POUND + REBKA, '60; JOSEPHSON '60!),

ADDITIONAL TEMP DEPENDENCE IN ISOMER SHIFT, $(\partial V/\partial T)_p$ ETC.

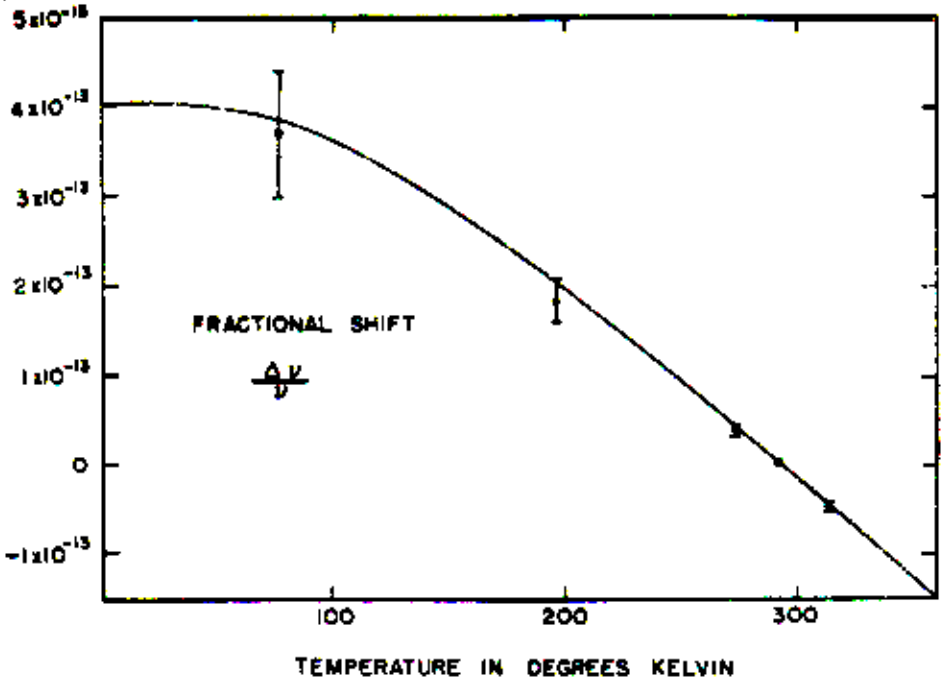


FIG. 1. Fractional shift of energy of 14.4-kev gamma-ray absorption of Fe^{57} vs absolute temperature of the metal. The solid line is derived from assuming a Debye temperature of 420°K.

ble I. Data from the first four days of counting. The data are expressed as fractional frequency differences een source and absorber multiplied by 10^{15}, as derived from the appropriate sensitivity calibration. The tive signs mean that the γ ray has a frequency lower than the frequency of maximum absorption at the ab- er.

Period	Shift observed	Temperature correction	Net shift
	Source at bottom		
Feb. 22, 5 p.m.	-11.5 ± 3.0	-9.2	-20.7 ± 3.0
	-16.4 ± 2.2[a]	-5.9	-22.3 ± 2.2
	-13.8 ± 1.3	-5.3	-19.1 ± 1.3
	-11.9 ± 2.1[a]	-8.0	-19.9 ± 2.1
	-8.7 ± 2.0[a]	-10.5	-19.2 ± 2.0
Feb. 23, 10 p.m.	-10.5 ± 2.0	-10.6	-21.0 ± 2.0
		Weighted average =	-19.7 ± 0.8
	Source at top		
Feb. 24, 0 a.m.	-12.0 ± 4.1	-8.6	-20.6 ± 4.1
	-5.7 ± 1.4	-9.6	-15.3 ± 1.4
	-7.4 ± 2.1[a]	-7.4	-14.8 ± 2.1
	-6.5 ± 2.1[a]	-5.8	-12.3 ± 2.1
	-13.9 ± 3.1[a]	-7.5	-21.4 ± 3.1
	-6.6 ± 3.0	-5.7	-12.3 ± 3.0
Feb. 25, 6 p.m.	-6.5 ± 2.0[a]	-8.9	-15.4 ± 2.0
	-10.0 ± 2.6	-7.9	-17.9 ± 2.6
		Weighted average =	-15.5 ± 0.8
		Mean shift =	-17.6 ± 0.6
		Difference of averages =	-4.2 ± 1.1

ese data were taken simultaneously with a sensitivity calibration.

THE 7 IRON COATED BERYLLIUM ABSORBERS RANGED FROM −8 TO −39 $\times 10^{-15}$ SHIFTS RELATIVE TO THE SOURCE.

NET $\Delta\nu/\nu$ FOR 2 WAYS FROM GRAVITY (10 DAYS) 5.13 ± 0.51 VS EXPECTED 4.92×10^{-15}

TRINITY COLLEGE

CAMBRIDGE (England)

17th Mar. 1960

Prof. R.V. Pound,
Harvard University,
Cambridge, Mass. U.S.A.

Dear Professor Pound,

Thank you for the papers on the temperature shift and gravitational red-shift of γ-rays. We thought that in view of the somewhat erratic fluctuations in your preliminary experiment on the red-shift you might have hit on the correct explanation of the effect.

Anyway, I was glad to hear that your experiments confirm the theoretical predictions quantitatively.

Yours sincerely,

B. D. Josephson

FALL, 1960. NEW START.

GLEN REDRA TO YALE

JOINED BY R.W.P DREVER

1960 – 61

J.L. SNOEK 1961–64.

IMPROVEMENTS:

A NEW SITE? – NO.

NEW ABSORBER: 2g

ENRICHED ^{57}Fe – ROLLED

FOILS – ON MYLAR – HEXEL.

DOUBLE SOURCE

TEMPERATURE CONTROL

~1mk°. ABSORBER, SOURCE

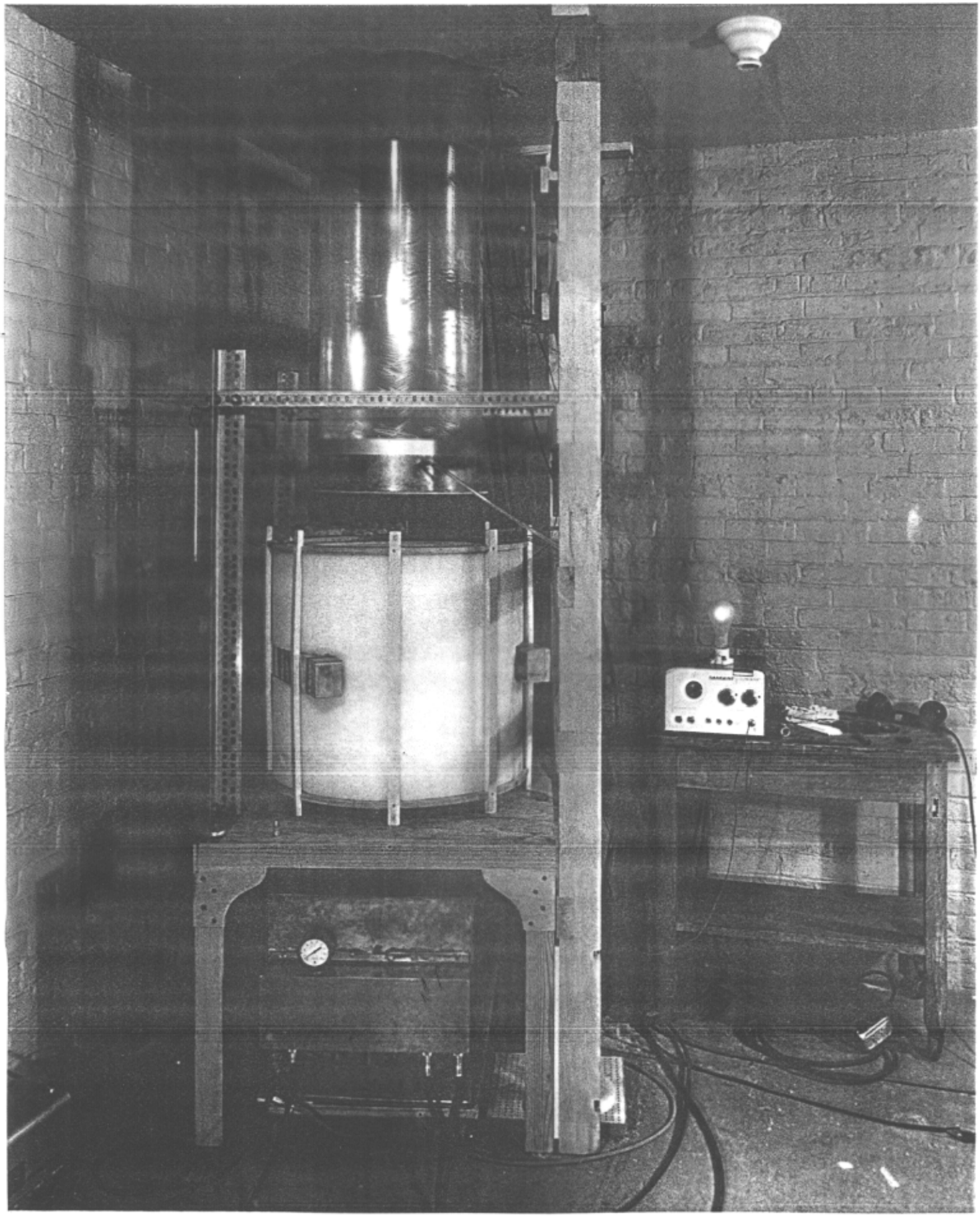
+ MONITOR.

LARGE PROPORTIONAL COUNTERS.

CAMERA RECORDING COUNTERS

EVERY ~2HRS.

RESULTS – N.G. – 0.9 – LYMAN



солнечного диска или что одно и тоже от угла θ между нормалью к солнечной поверхности и направлением светового луча в точке наблюдения. Поэтому можно ожидать такого же эффекта и в земных условиях, если считать, что процессом, происходящие на Солнце, не влияют на гравитационное красное смещение. То есть я хочу сказать, что гравитационное смещение частоты на единицу высоты падения - луча зависит от угла θ между вертикалью и направлением их распространения:

$$\Delta\nu/\nu_0 = g/c^2 \, \Delta h \cdot \Gamma(\theta)$$

где $\Gamma(\theta)$- некоторая функция угла θ. Не обнаружили ли вы систематического расхождения между теорией Эйнштейна и вашими точными экспериментами, которое лежит за пределами случайных ошибок. Если такое расхождение действительно имеет место, то это не следует объяснять систематическими ошибками экспериментальной установки, которая повидимому является безупречной.

Я буду глубоко благодарен Вам, если Вы сообщите мне о Ваших результатах, сомнениях и дальнейших планах в последовательской работе в этом направлении.

Вашрение Вам

В. П. Ляхов

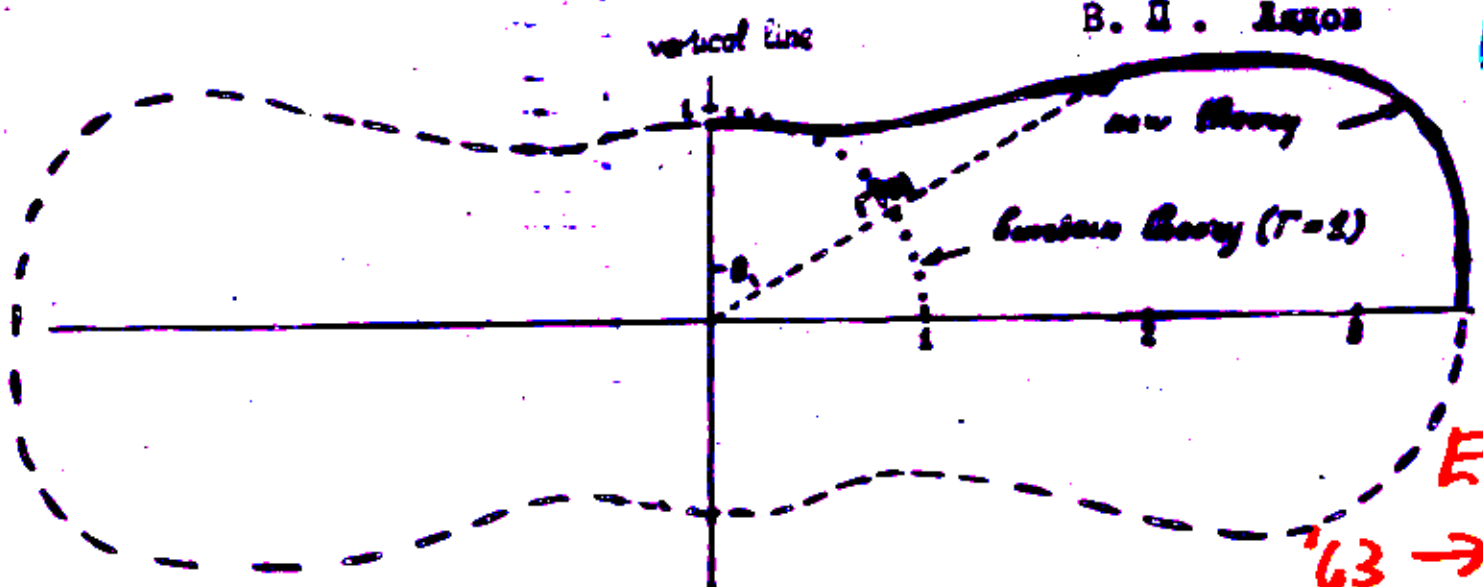
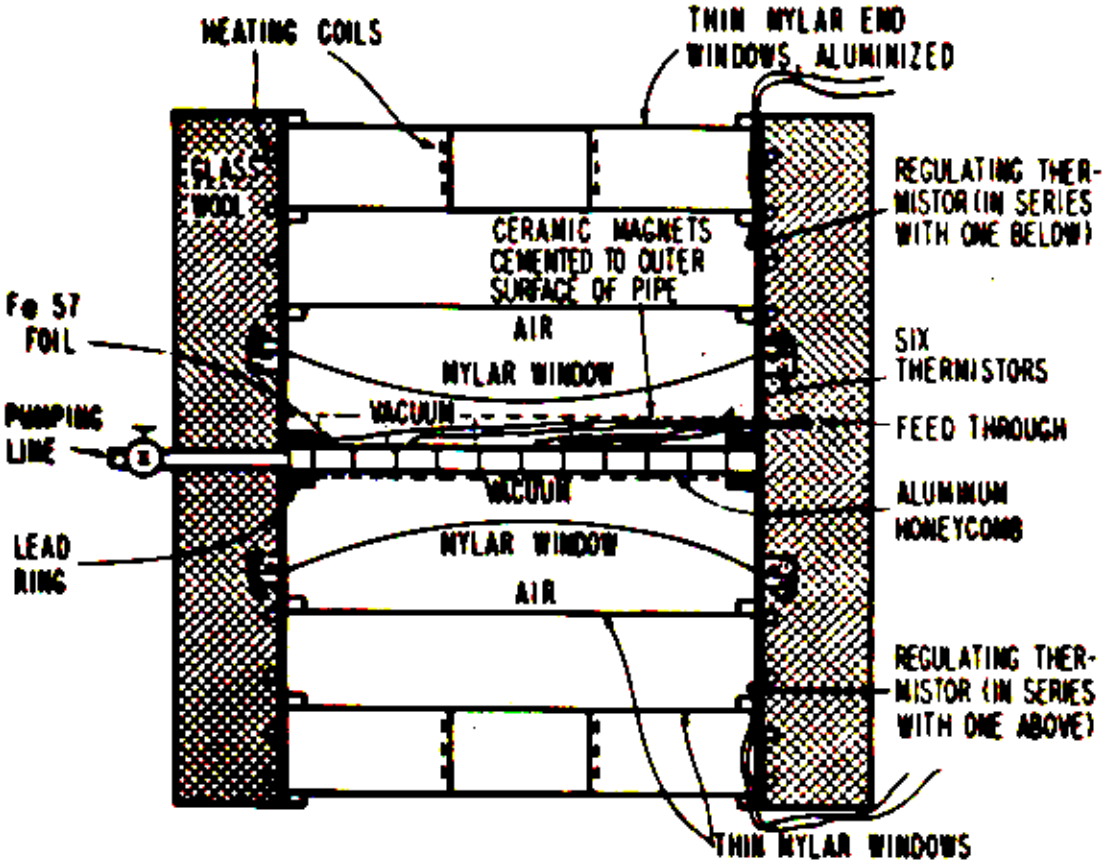
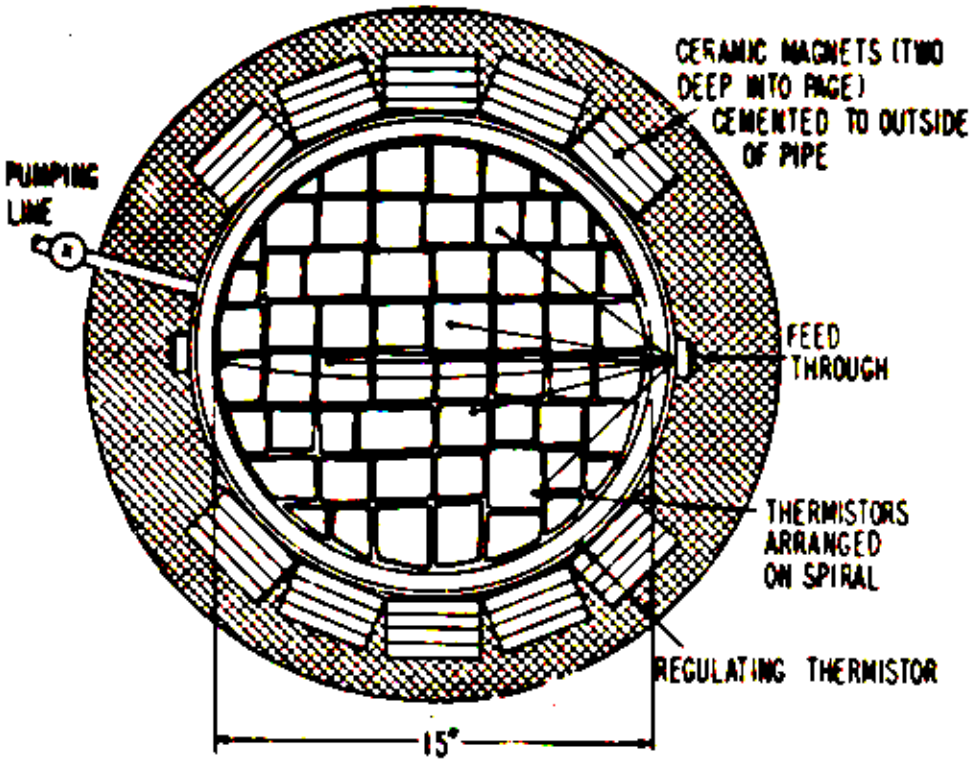


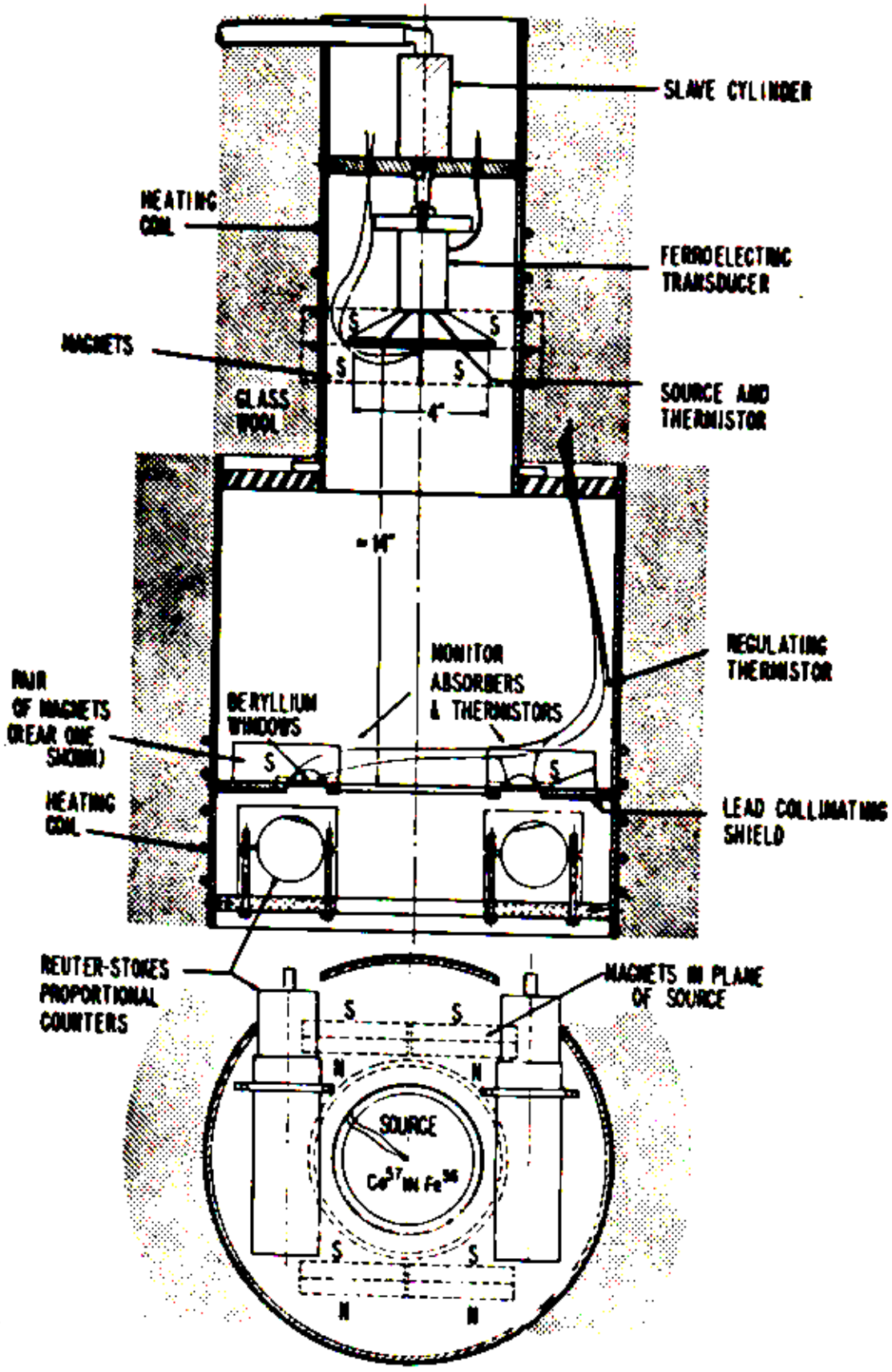
FIG. 1 ($\Gamma = \Gamma(\theta)$)

V.P. LYAKOV
KAZAN STATE UNIV.
6 NOV '63

EARLY DATA '63 → 0.88 ± ?
LED TO BETTER DESIGN

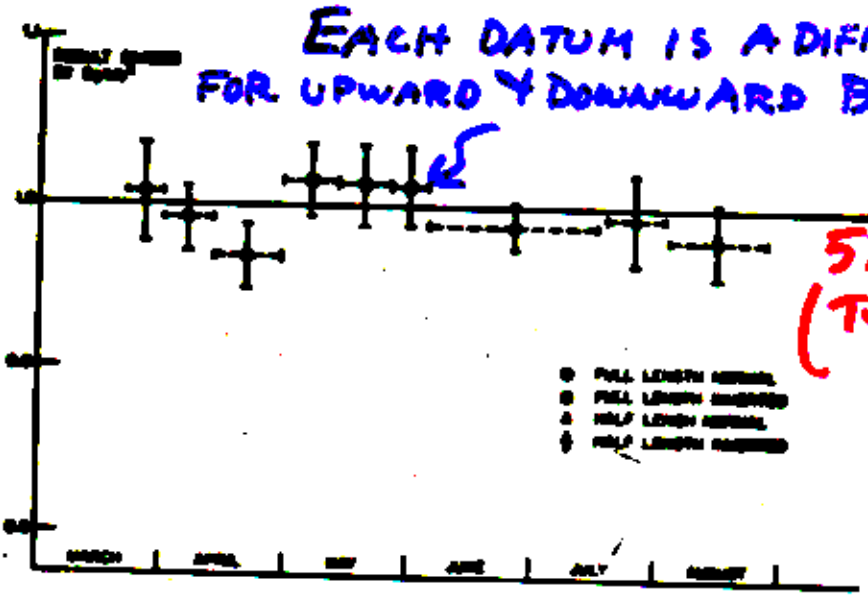

HEATING COILS
THIN MYLAR END WINDOWS, ALUMINIZED
GLASS WOOL
REGULATING THERMISTOR (IN SERIES WITH ONE BELOW)
CERAMIC MAGNETS CEMENTED TO OUTER SURFACE OF PIPE
Fe 57 FOIL
AIR
MYLAR WINDOW
SIX THERMISTORS
VACUUM
PUMPING LINE
FEED THROUGH
VACUUM
ALUMINUM HONEYCOMB
LEAD RING
MYLAR WINDOW
AIR
REGULATING THERMISTOR (IN SERIES WITH ONE ABOVE)
THIN MYLAR WINDOWS


CERAMIC MAGNETS (TWO DEEP INTO PAGE) CEMENTED TO OUTSIDE OF PIPE
PUMPING LINE
FEED THROUGH
THERMISTORS ARRANGED ON SPIRAL
REGULATING THERMISTOR
15"


SLAVE CYLINDER
HEATING COIL
FERROELECTRIC TRANSDUCER
MAGNETS
GLASS WOOL
SOURCE AND THERMISTOR
4"
~14"
REGULATING THERMISTOR
MONITOR ABSORBERS & THERMISTORS
PAIR OF MAGNETS (REAR ONE SHOWN)
BERYLLIUM WINDOWS
HEATING COIL
LEAD COLLIMATING SHIELD
REUTER-STOKES PROPORTIONAL COUNTERS
MAGNETS IN PLANE OF SOURCE
SOURCE
Co^{57} IN Fe^{56}
S
N

FROM P+S ; P.R 1965

EACH DATUM IS A DIFFERENCE FOR UPWARD + DOWNWARD BEAMS



5×10^{-15} (TWO WAY)

Fig. 6. The final data. The vertical bars indicate statistical standard deviations and the horizontal bars the period of time covered by data contributing to a given point.

OVERALL RESULT 0.9990 ± 0.0076
(STATISTICAL ERROR ONLY ?)

TABLE II. A list of estimated limits of sensitivities to systematic error.

Origin	Amount
Path length	±0.0005
Temperature coefficient	±0.0005
Temperature distribution	±0.0020
Velocity calibration	±0.0020
Nonlinearity	±0.0020
Simple sum	±0.0100

← LARGEST EST. SYST.

ESTIMATED LIMITS OF ERRORS

OVERALL ~ ±0.01 SYS + STAT

FROM
L. I. SCHIFF.
AM JOURN. PHYS.
28 340 (1960)

We now argue that in a nonuniform gravitational field we can replace the quantity gh in Eq. (3) by the difference in gravitational potential between the positions of clocks A and B. This implies that we make a series of intercomparisons between a number of clocks so

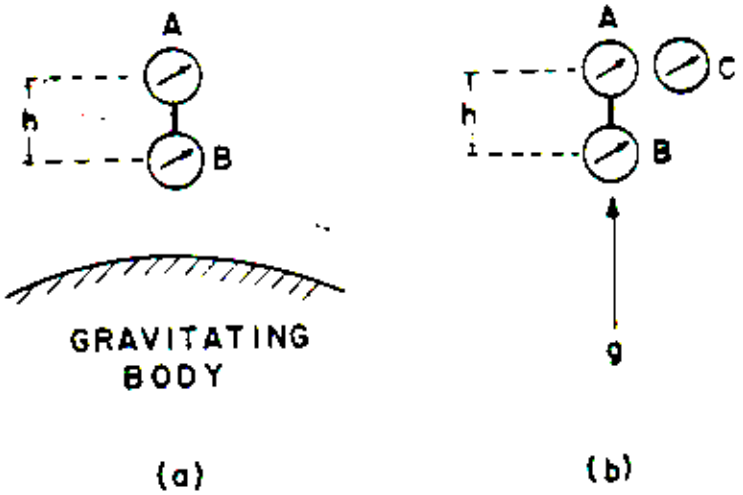


FIG. 1. (a) Two identically-constructed clocks, A and B, are at rest in a gravitational field. (b) The gravitating body is replaced by an upward acceleration g of clocks A and B, and a stationary clock C is introduced to compare their periods.

AS IT PASSES C OBSERVES RATE OF A

CAN'T ACTUALLY BE DONE.

$$f_A = f_c\left(1 - \frac{v_A^2}{2c^2}\right) \quad \text{(TIME DILATION)}$$

TRANSVERSE DOPPLER NOT SO SIMPLE!

$$f_B = f_c\left(1 - \frac{v_B^2}{2c^2}\right)$$

$$\frac{f_A - f_B}{f_c} = \frac{\Delta f}{f} = \frac{v_B^2 - v_A^2}{2c^2}$$

$$\therefore \frac{\Delta f}{f} = \frac{gh}{c^2}$$

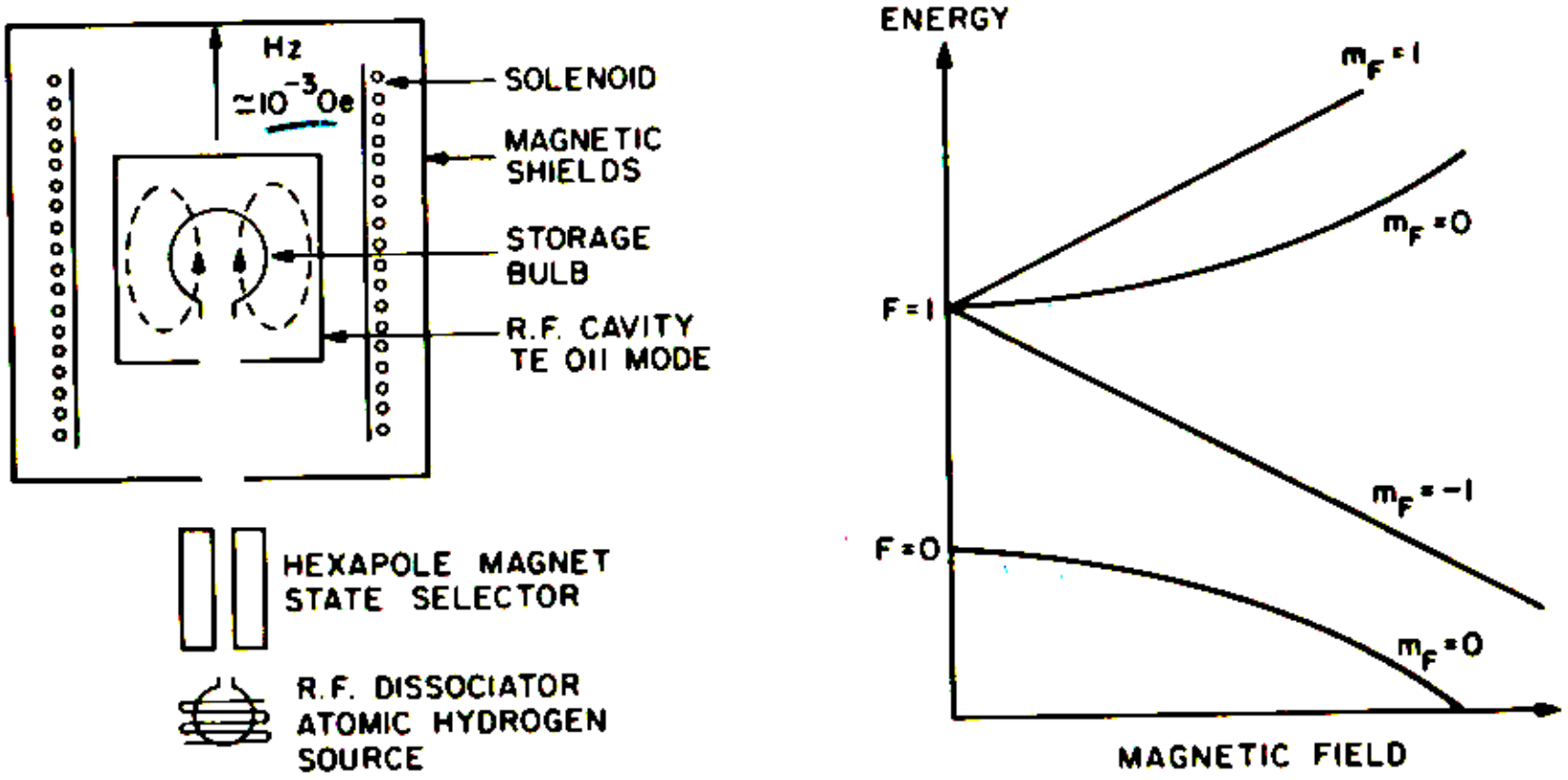
SINCE $v_B^2 - v_A^2 = 2ad$

RFC VESSOT, SAO

H Hfs

MASER 1421 MHz

007-95



2.18

Figure 2-11. Schematic diagram of hydrogen maser and energy-level diagram of atomic hydrogen.

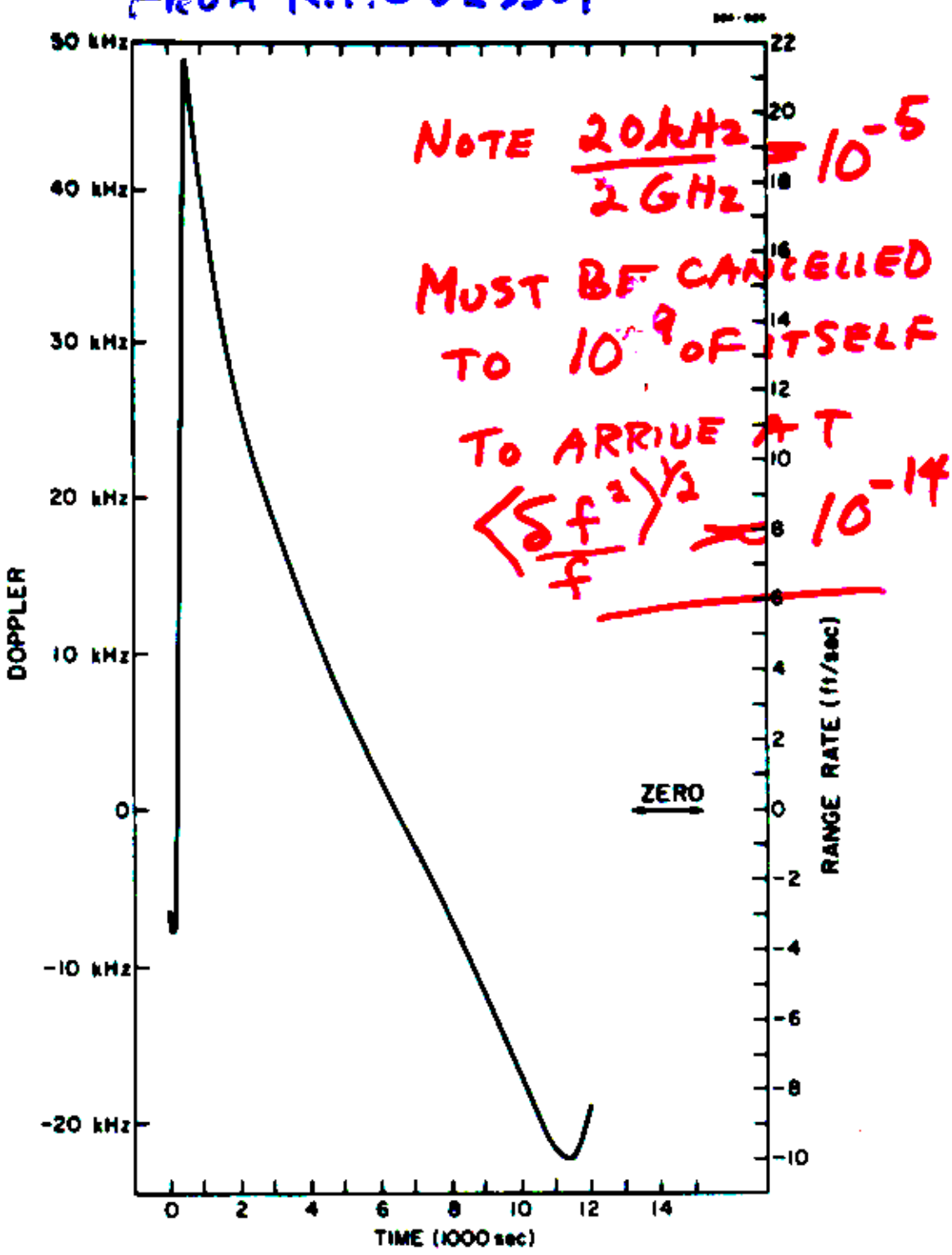


Figure 2-5. Range rate and doppler shift for a 2-GHz carrier vs. time as seen from Bermuda. Payload = 150 lb, azimuth = 90°, elevation = 88°.

FROM R.F.C VESSOT & M.W. LEVINE (SAO)
RPT TO NASA

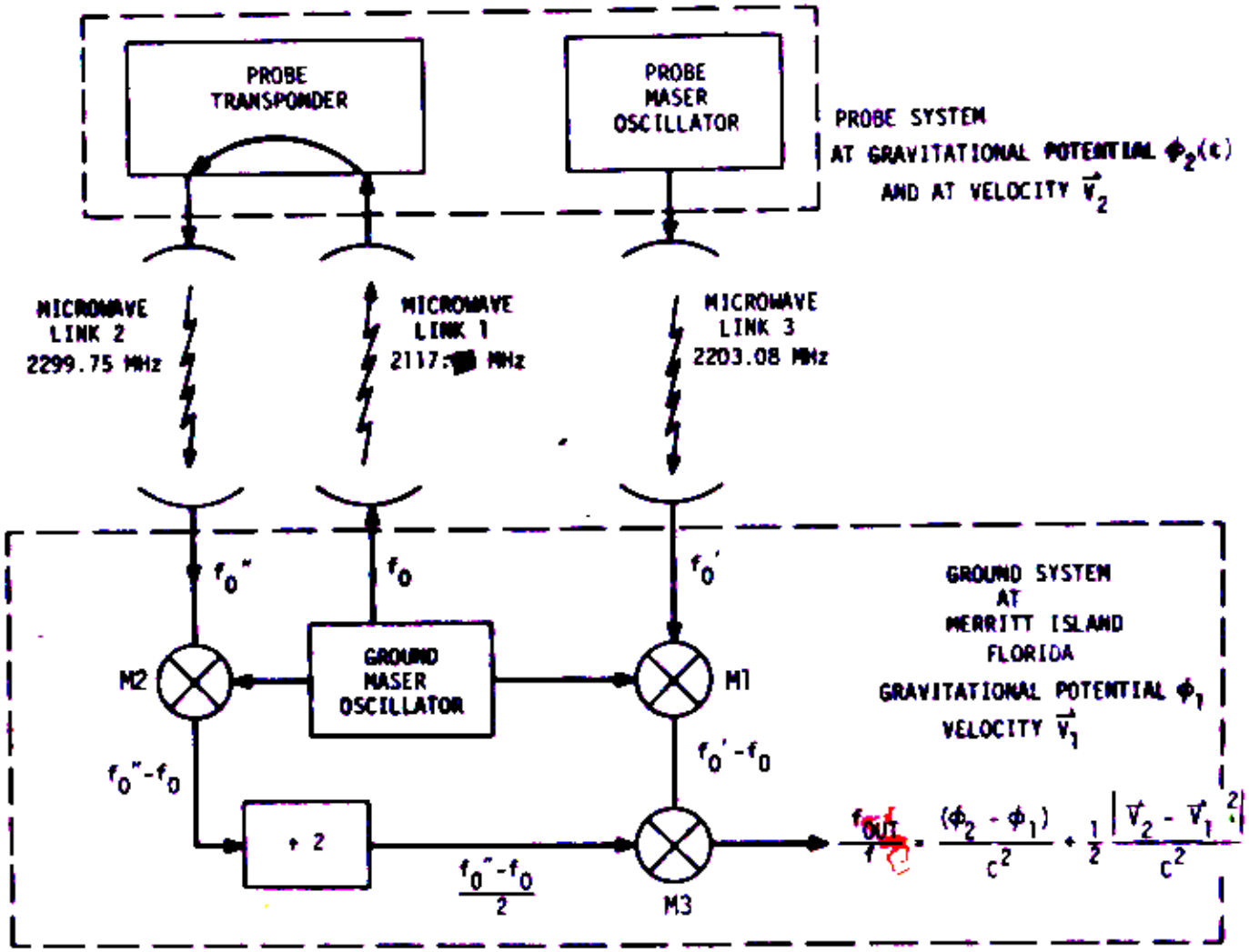


Figure 8. Doppler cancelling system concept.

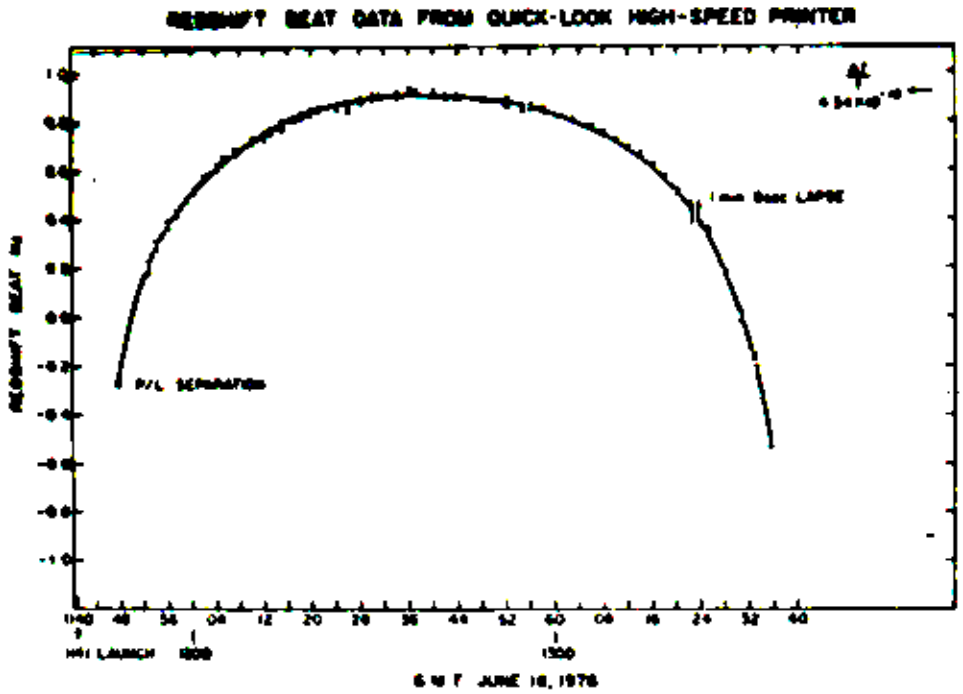
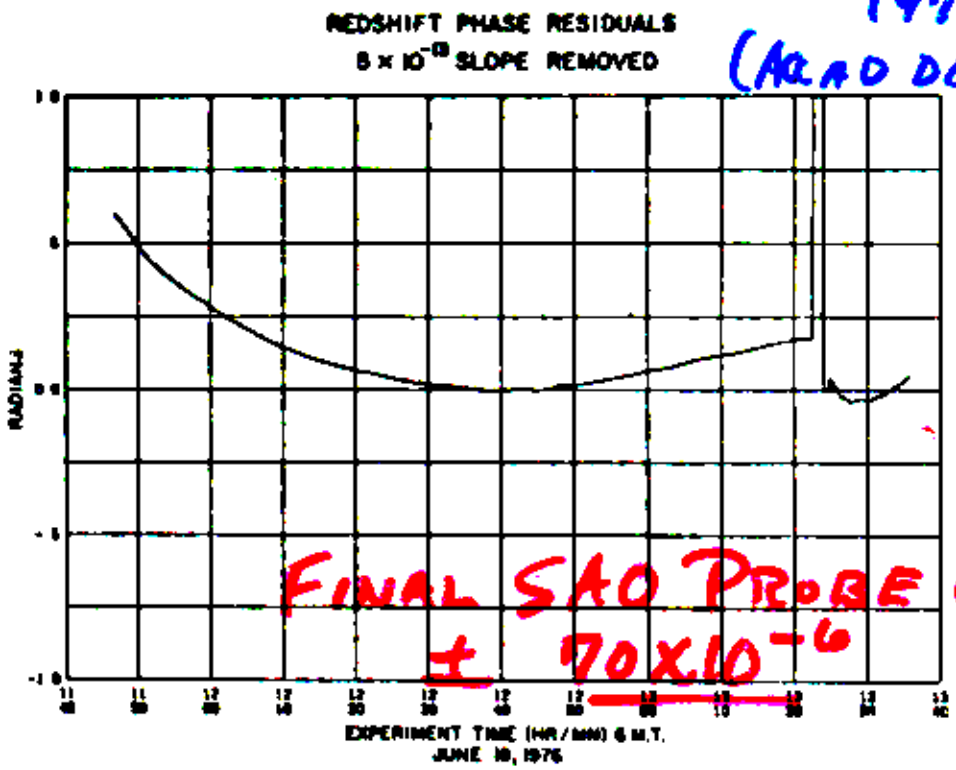


Fig. 11. — Redshift beat data from the quick-look high-speed printer.

VESSOT & LEVINE PAVIA CONF. 1976 (ACCAD DEI LINCEI '77)



FINAL SAO PROBE OK TO ± 70 X 10⁻⁶

Fig. 13. — Uncorrected residual phase data.

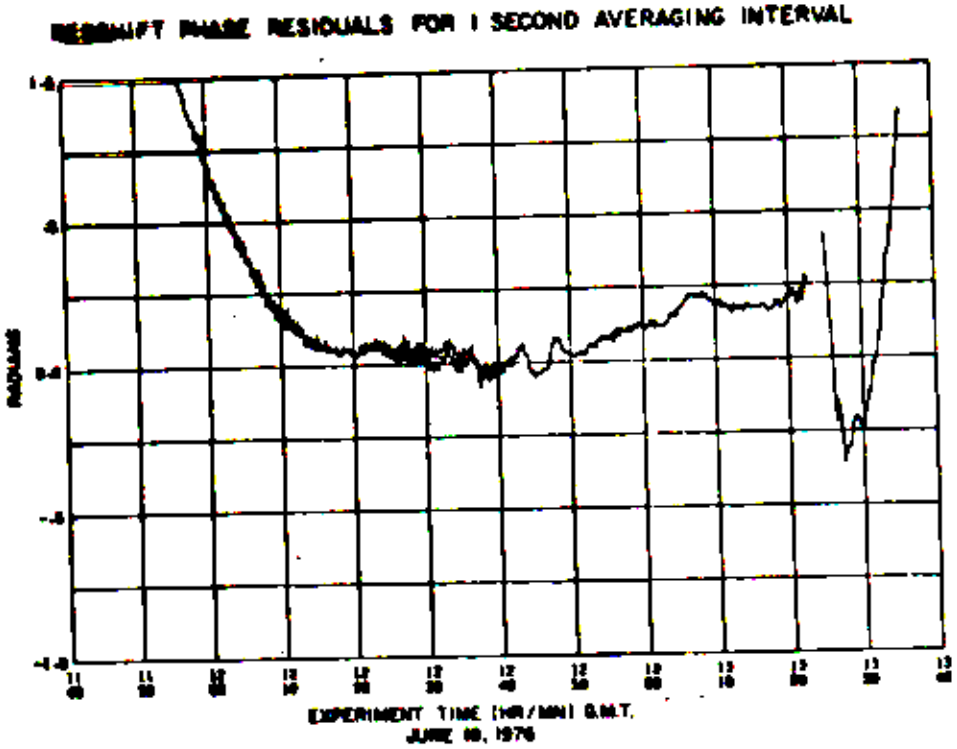
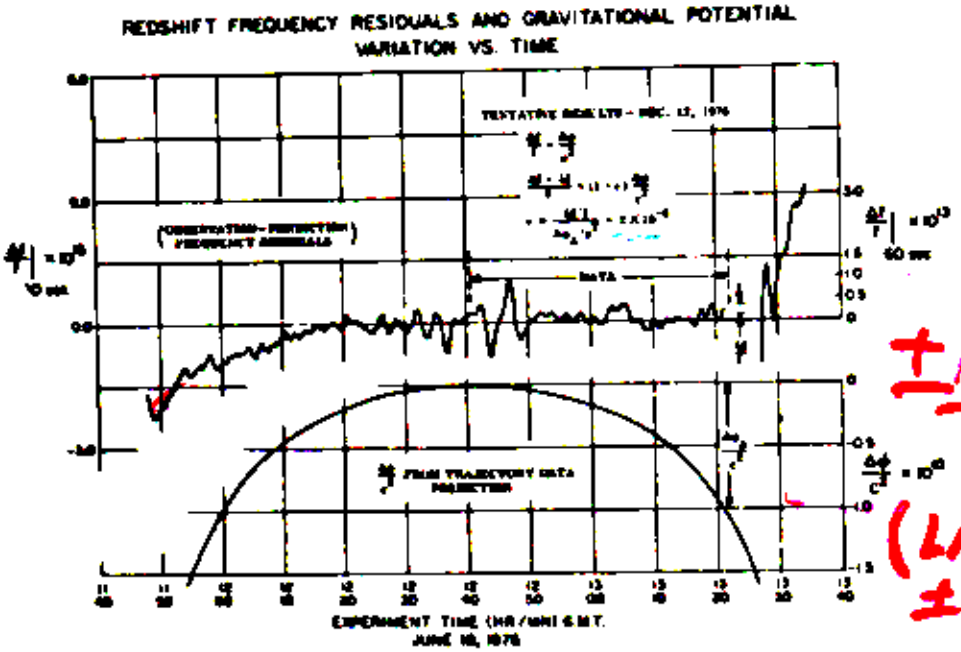


Fig. 16. — Corrected residual phase data.

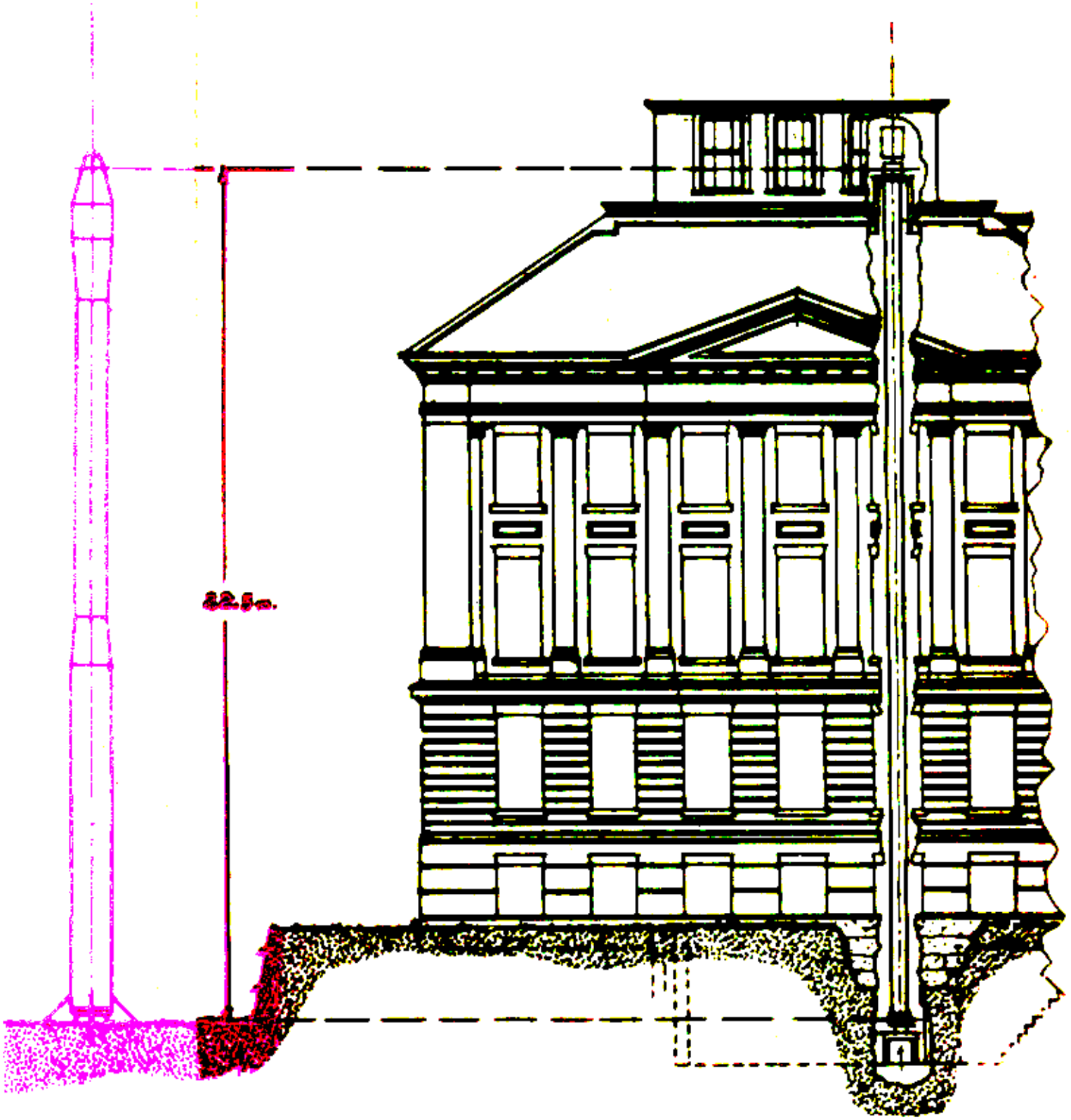


$\pm 1.4 \times 10^{-4}$

(LATEST CLAIM $\pm 70 \times 10^{-6}$)

Fig. 17. — Frequency residuals.

VESSOT + LEVINE, (ACCAD DEI LINCEI '77)


22.5 m.

What is Time?

G. J. WHITROW

THAMES AND HUDSON
LONDON 1972

The third of Einstein's tests has the most direct bearing on the properties of time. He found that a gravitational field has a slowing-down effect on natural clocks, analogous to the time-dilatation effect of motion in special relativity. This effect is most readily studied in the spectra of light emitted by a massive body. For the gravitational field of the body will slow down the frequency of this light and so cause its colour to become redder. We call this effect the *gravitational red-shift*. Attempts to detect it in light emitted by the Sun have been made but are difficult because the proportional changes in frequency and wavelength are only of the order of two parts in a million. But, surprisingly, just over ten years ago it became possible to test this effect much more accurately by a laboratory experiment. R. V. Pound and G. A. Rebka of Harvard successfully used a new, highly sensitive technique, called the Mössbauer effect, to measure the change in frequency of light falling from the top to the bottom of a 74-foot tower that had been installed for a different purpose many years before at Yale University. They confirmed with remarkable accuracy the frequency change predicted on the basis of Einstein's theory, which was of the order of only one part in a thousand million million.

WHAT IS VERIFIED BY MEASUREMENTS OF THE GRAVITATIONAL FREQUENCY SHIFT?

V. L. GINZBURG

Usp. Fiz. Nauk 81, 739-743 (December, 1963)

IT was only three years ago that it became possible to reliably measure [1] the gravitational frequency shift (to weigh photons), although the effect had been predicted by Einstein already in 1907. [2] The measurement of the gravitational frequency shift (one talks more commonly of the red shift, having in mind the shift of the lines in the spectrum of the sun and of stars) is usually viewed as one means of testing the general theory of relativity. In connection with the experiments of Pound et al [1], however, there have appeared in the literature statements to the effect that these experiments do not properly introduce anything new and do not constitute a test of the general theory of relativity. Precisely such a point of view has been clearly expressed, for example, in the article by Ya. A. Smorodinskiĭ, [3] published recently in UFN (it is indicated in this article that the experiments [1] "test nothing beyond the law of energy conservation").

It seems to me that this conclusion is incorrect and that the experiments of Pound et al do solve a problem, which is in fact the problem posed by Einstein in 1907, and thereafter in a more complete form in 1911;* moreover, these experiments have a direct bearing on the general theory of relativity.

The problem was formulated by Einstein with characteristic precision and clarity: [4]

Honourable Sir!

NOV 2 0 1967

A few months ago I send the letter to Sirs Pound's and Rebka's Jr., inn which I paid attention to bad interpretation the experimential results publicated in Physical Rev. Letters (1960) („Apparent weight of photons").
This experiment ought to show existence the weight of photons by demonstration influence of the gravitation for weight of photons. In reality the experiment suggest, that photons haven't the masses. The authors added the temperature corrections to experi=mential results instead of to take away. As a consequence they obtained results confor=mable with the relativity theory.
I supposed, that Sirs Pound and Rebka will confesse to errors and will appreciate in exchange for remark. I asked to contact with laser specialist, Unfortunately, I hadn't the answer. In the face of them, I must to suspect the authors of intentionally falsification the experimential results. I am however convinced that the American physicists are the honests people. Therefore I suppose, that Pound and Rebka are emigrants, whose came to America to Lyman Institute from foreign country. I please the better to carry the selection of admitted workers, lest the Institute does not happen the harbour of the swindlers.

(Sincerely) Leszek Ilnicki