Ferieninsel Lindau im Bodensee mit Bad Schachen

Prof. Otto Stern
759 Cragmont St.
Berkeley
California
U.S.A.

60 Jahre Cramers Kunstanstalt KG, Dortmund

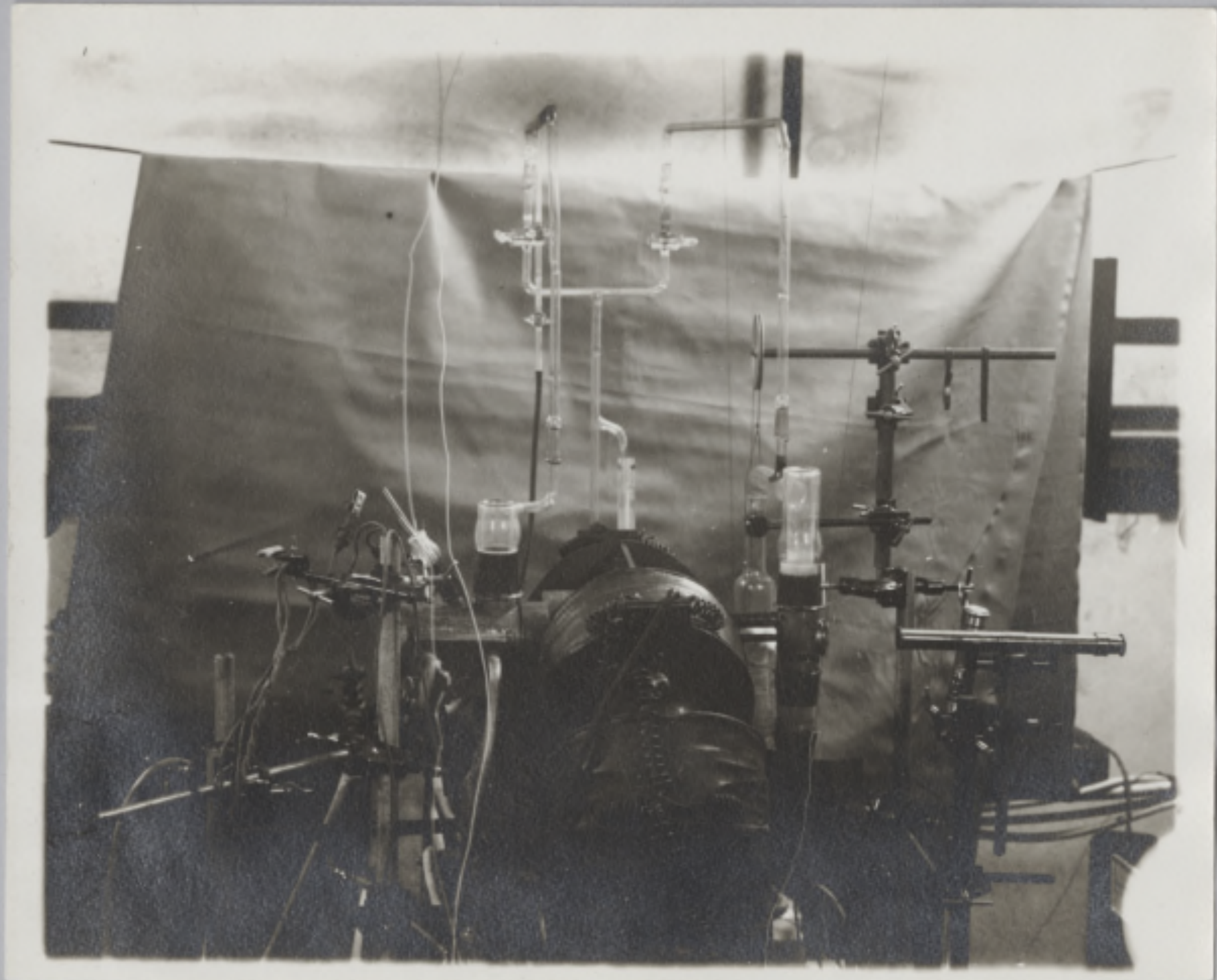
Lu 76
62/3

Lindau 26/VI 1962. Lieber Stern,
Zumitten einer Schar alter Freunde
sende ich Ihnen herzlichste Grüsse u.
hoffe es geht Ihnen gut! Ihre L. Meitner
We miss you I. I. Rabi.

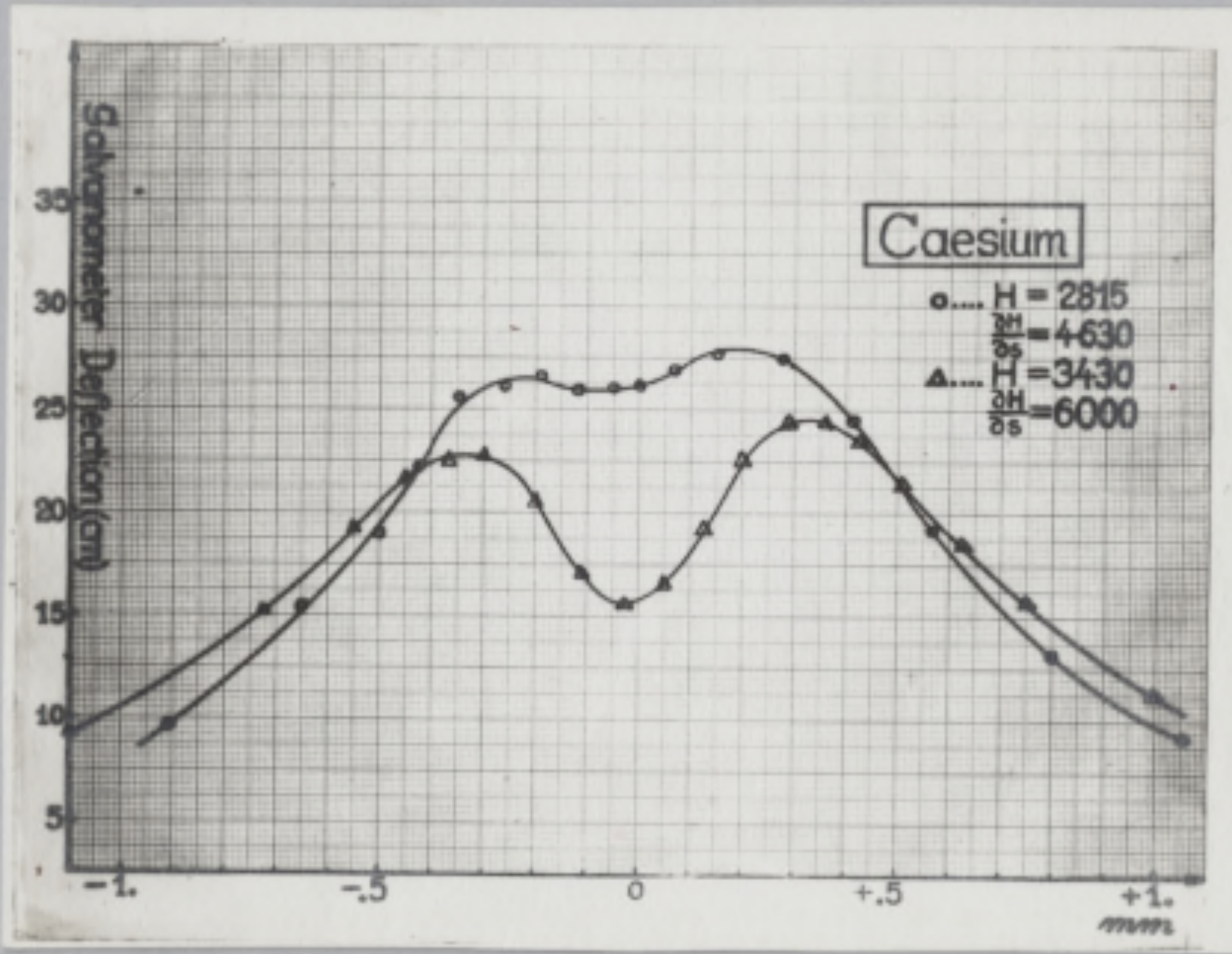
Herzl. Grüsse Ihr Aus Hahn

Herzliche Grüsse. Wann sehen wir uns
mal wieder Ihr James Franck
Elfriede Segrè
With warmest greetings Helen Rabi

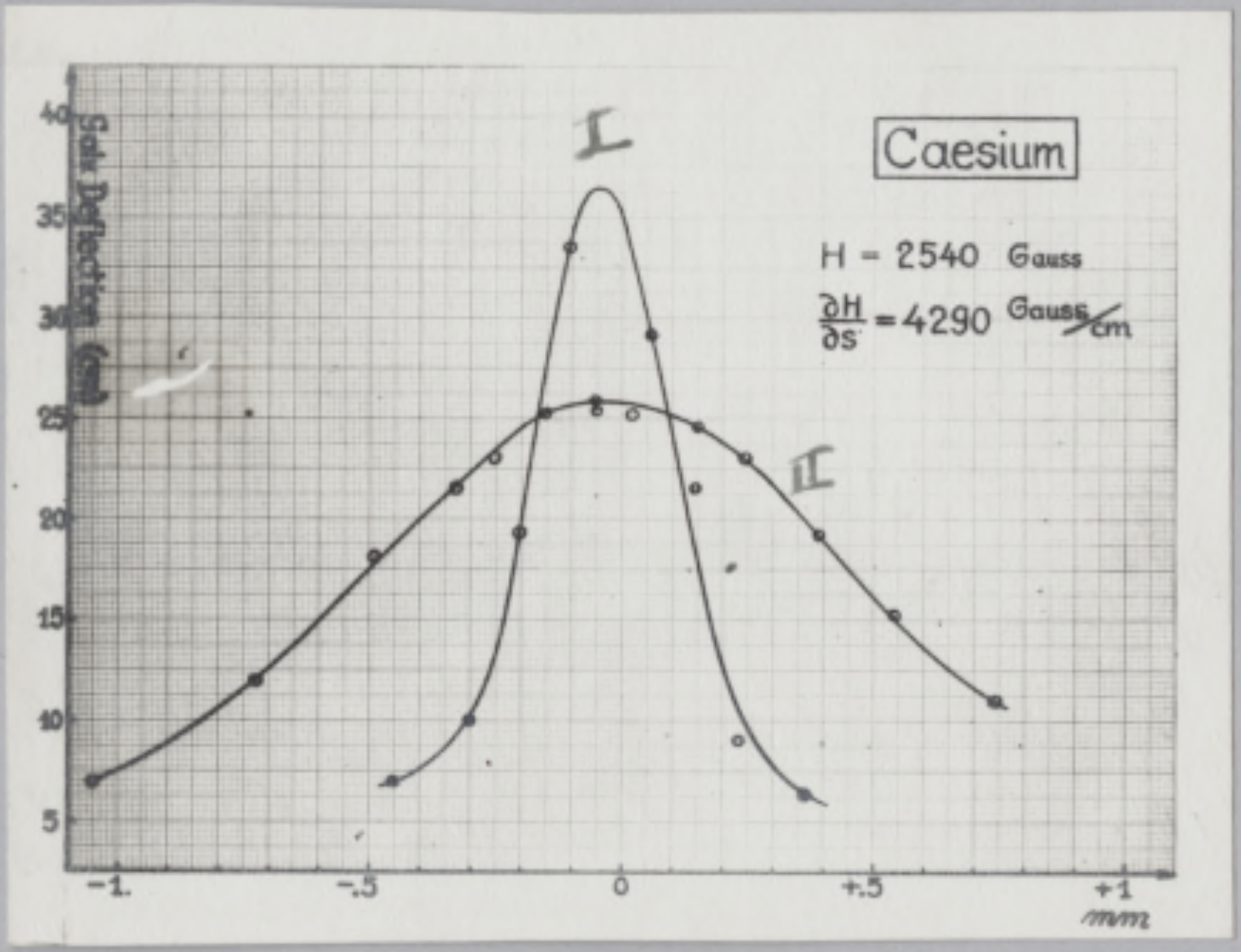
Freigeg. d. C.A.B.



4031


Caesium
o.... H = 2815
∂H/∂s = 4630
Δ.... H = 3430
∂H/∂s = 6000
Galvanometer Deflection (cm)
35
30
25
20
15
10
5
−1.
−.5
0
+.5
+1.
mm

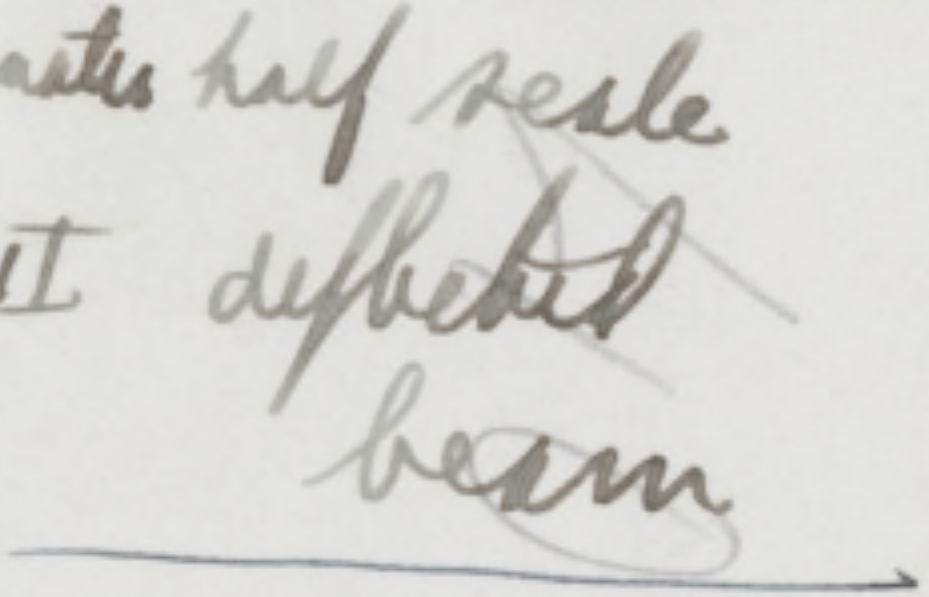
4037

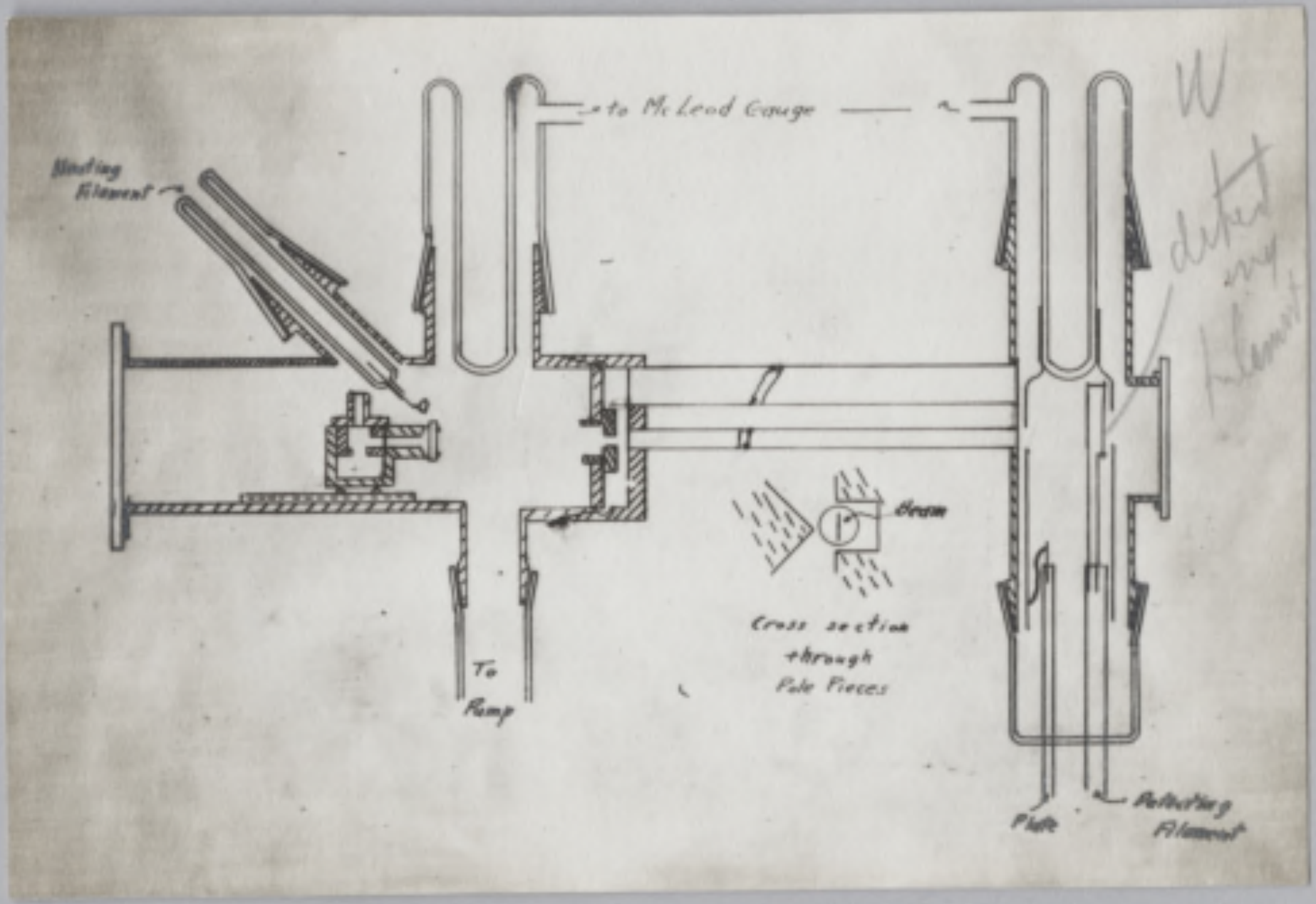

Caesium
H = 2540 Gauss
∂H/∂s = 4290 Gauss/cm
I
II
Spur Deflection (mm)
40
35
30
25
20
15
10
5
-1.
-.5
0
+.5
+1
mm

I - Undeflected Beam

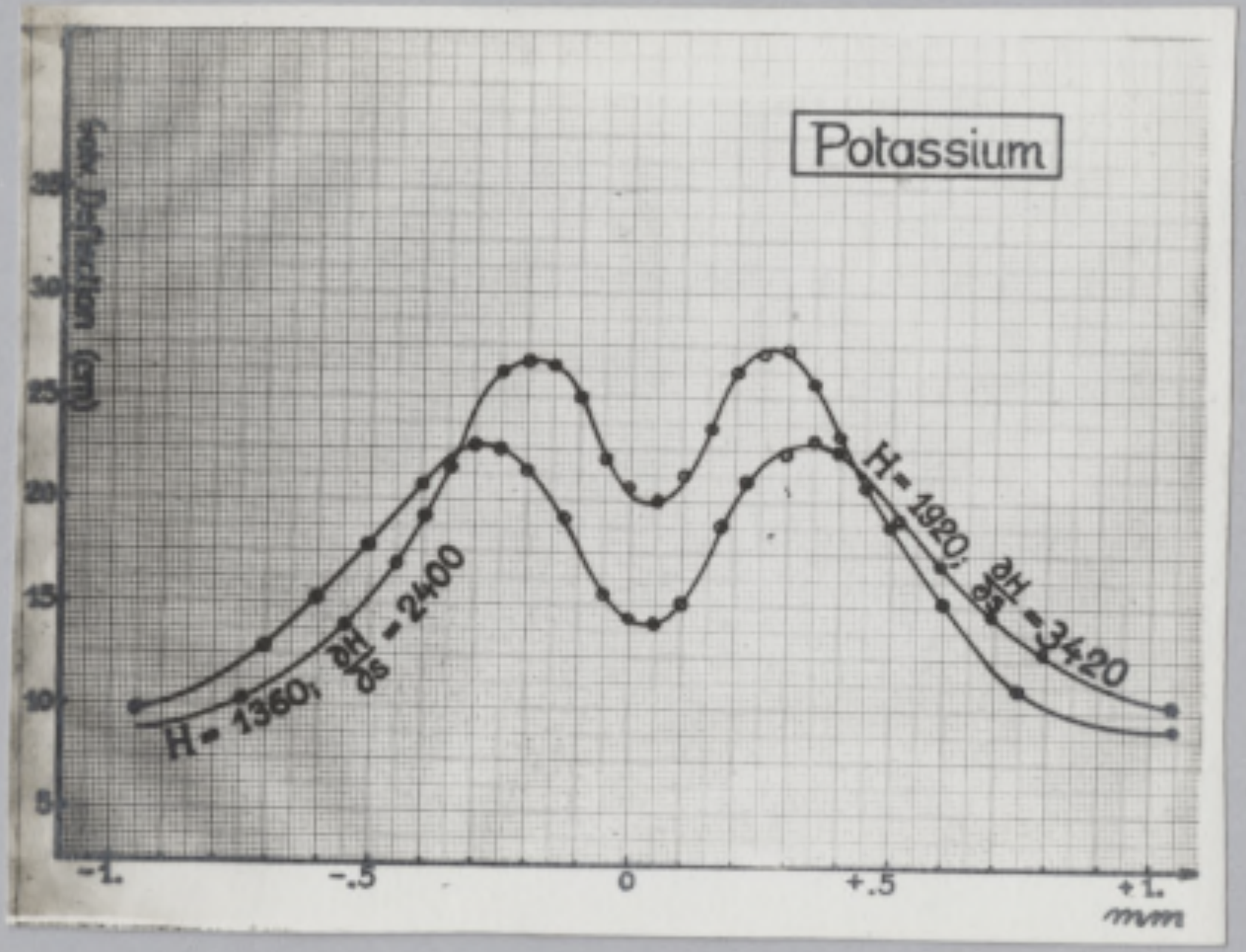
indicates half scale

II deflected beam

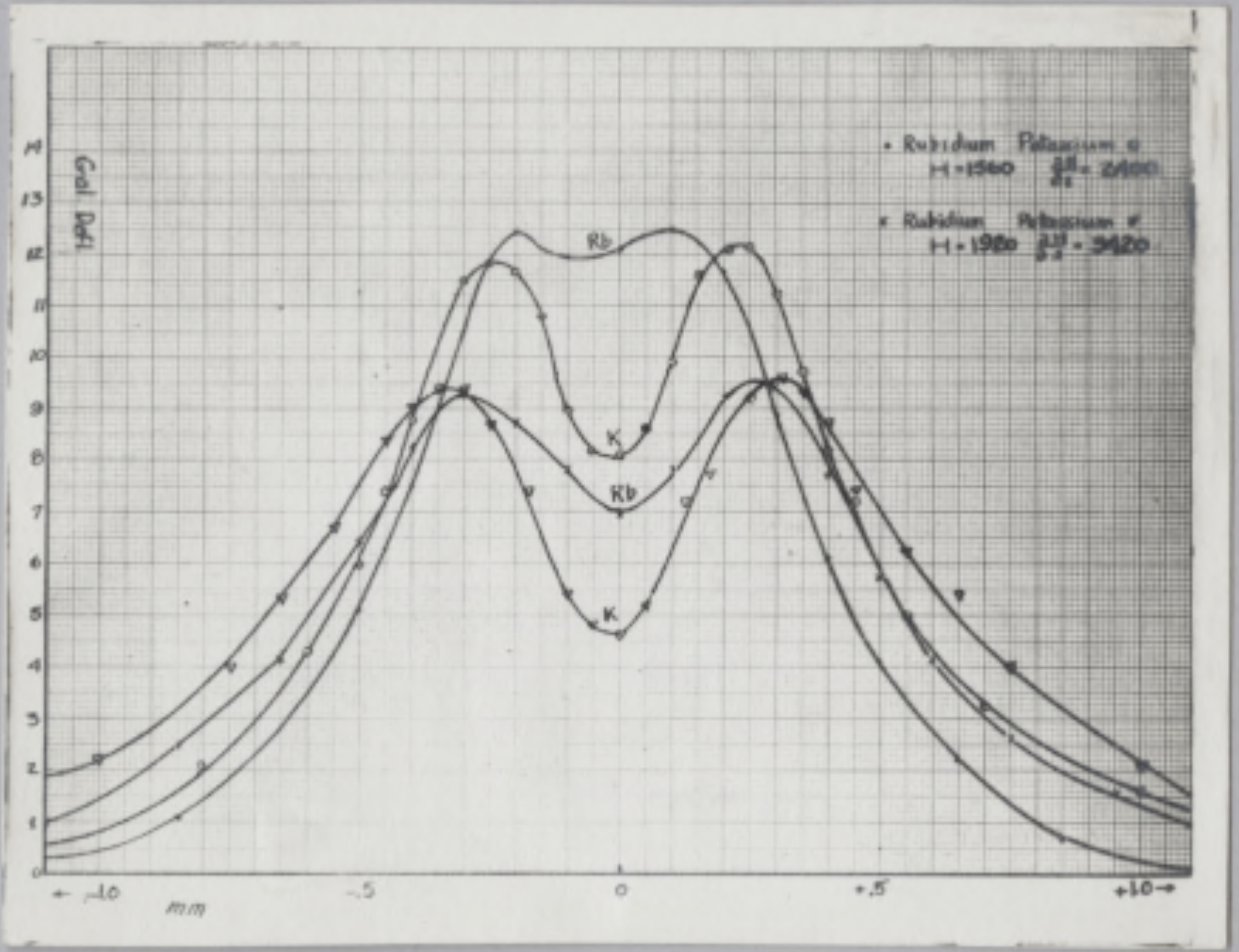



to McLeod Gauge
Heating Filament
Beam
Cross section through Pole Pieces
To Pump
Plate
Reflecting Filament

The total beam
is 36 cm long.
18 cm in the field
and 10.7 beyond
the field.


Potassium
Peak Deflection (cm)
35
30
25
20
15
10
5
H = 1360; $\frac{\partial H}{\partial s}$ = 2400
H = 1920; $\frac{\partial H}{\partial s}$ = 3420
-1.
-.5
0
+.5
+1.
mm

4034.



Gal Defl
14
13
12
11
10
9
8
7
6
5
4
3
2
1
0
Rb
K
Rb
K
• Rubidium Potassium
H = 1560
× Rubidium Potassium
H = 1980
← -1.0
-.5
0
+.5
+1.0 →
mm

Comparison of
K & Rb. both
have been reduced
to the same
undeflected beam

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

Lieber Professor Stern

Die Cäsium Versuche gehen ausgezeichnet. Wir haben schon ~~es~~ einige Versuche gemacht und der effect des Kern spins ist ungeheuer. Bisher is alles mit vergleich mit Kalium gemacht, aber jetzt messen wir das Feld. Unser strahl is 36 cm lang. Der

Uran aller Molekularstrahlen.
Sobald wir das Feld gemessen
haben (und es giebt Schwierig-
keiten) werden wir Rubidium
machen und dann werde Ich
zeit have Ihnen alle resultaten
ausführlich mit zuteilen.

Bitte schreiben sie
mir doch mal. Sie können
es ihre Sektretärin diktieren
und ich ~~bin~~ bin so neugierig
zu wissen was da alles in
Hamburg geschieht.

Grüsse von meine Frau, Nancy
und mich an Ihnen und Ihre Schwester
und die Herren des Instituts.

Ihr ergebener,

Rabi

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

For I am working with a field of about 700 gauss and a $\frac{\partial H}{\partial z}$ of 2400 and have a path of 40 cm (Um Gottens Willen). This should give me the same deflection as Taylor obtained. The apparatus is almost finished and if I'm lucky I'll have results by the end of the summer. Please consider this confidential as I would not like to have to hurry with this experiment.

The magnetic Monochromator is wonderfull for this. Because if the deflection is first made in a strong field and all the states lie very close together one gets a uniform velocity and if another deflection is made in a weaker field the different states are ~~deflect~~ deflected very different amounts as seen in the diagram.

There are two other experiments in progress. One the magnetic moment of O_2 and O. using Langmuirs idea of the change of electron emission from an oxygen caesium coating as a detector. I expect to be be very sensitive. We are also applying it to the diffraction of O_2 and O from Lithium fluoride.

I have a fine idea about a scattering experiment but as yet we have no student to carry it out. The idea is this. The scattering is best done for low velocities but using the beam itself as a scattering. The de Broglie wave length is determined by the relative velocity and corresponds to a very much lower temperature than the temperature of the beam, about $\frac{1}{5}$. Furthermore the relative velocity is in a fixed direction and the results should therefore be much clearer.

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

I hope to start somebody on this experiment soon. The other experiment with neon had to be abandoned because the student lost his money and had to go to work.

I remember that you and Pauli had Nishina calculating some thing on scattering of molecules. I'm doing this now with the assumption that the molecules are rigid spheres. Is that the same thing. I am interested in the region where the de Broglie wave length is of the same magnitude as the diameter of the molecules. Of course I realize that the physical assumptions are poor but I think it is an interesting beginning to the real problem.

We are also building a velocity sorter for high resolution. We have

obtained a grant from the university of $800 to build it. We want it to go at 500 r.p.s. and obtain a resolution of about 8%. The construction is just beginning.

Please give my regards to the ~~x~~ Institute and particularly to Gordon. I hope to write him soon.

We are expecting Pauli on Saturday and are ~~st~~ 'sehr gespannt'.

Helen and the baby sent their love.

Ihr ergebener

Rabi

I. I. Rabi
Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

JUN 1931

Herrn Prof. Dr. O. Stern
Institut für Physikalische Chemie
Jungiusstrasse 9
Hamburg. Germany

Via S.S. Europa

I. I. Rabi
Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

Professor O. Stern
~~Institut für Physikalische Chemie~~ „Chantarella"
~~Jungiusstrasse 9~~ St. Moritz
~~Hamburg~~ ~~Germany~~ Schweiz.

I. I. Rabi
Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

Herrn Prof. Dr. O. Stern
Institut für Physikalische Chemie
Jungiusstrasse 9
Hamburg
Germany

I. I. RABI
Columbia University
New York 27, N.Y.
DEPARTMENT OF PHYSICS

VIA AIR MAIL

Prof. O. Stern
759 Cragmont Ave
Berkeley 8
California

6.3.II.63

I.I. RABI
Columbia University
New York 27, N.Y.
DEPARTMENT OF PHYSICS
NEW YORK, N.Y.
9:30 PM
12 AUG
1963
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U.S. AIR MAIL
VIA AIR MAIL
Professor Otto Stern
759 Cragmont St
Berkeley
California

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DR OTTO STERN=

:DLR 759 CRAGMONT AVE BERKELEY CALIF(RTE KY)=

=ON THE OCCASION OF YOUR SIXTIETH ANNIVERSARY WE WANT TO PAY OUR RESPECTS AND EXPRESS OUR BEST WISHES FOR A LONG AND PLEASANT LIFE=

:BESSEY ELLETT ESTERMAN FRANCK GOUDSMIT HEITLER KELLOG KUSCH LADENBURG LONDON MANEY MILLMAN OPPENHEIMER PLAZVECK RABBI RAMSAY RODEBUSH SEITZ SIMPSON AND ZACHARIAS PHYSICS DEPARTMENT=

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I. I. Rabi
450 Riverside Drive
N. Y. 27 N. Y.

AIR
MAIL

Dr. O. Stern
759 Cragmont Ave.
Berkeley
California

AIR
MAIL

Columbia University in the City of New York

DEPARTMENT OF PHYSICS

538 West 120th Street
New York 27, N.Y.

Jan. 29, 61

Dear Stern,

I am sending you a larger copy which I had made and it seems even better. The negative comes in a spool continuous roll and is hard to handle individually without damage. If you will tell me how many copies you would like to have I will have them made and you should have them in a few weeks.

I am off to Paris on Monday Feb. 1st but will be back in about a week. All sorts of new physics seems to be happening but I don't understand it as yet.

Best wishes Rabi

I. I. RABI

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10 COLUMBUS CIRCLE

NEW YORK 19, NEW YORK

450 Riverside Drive

New York 27 N.Y.

Columbia University in the City of New York

DEPARTMENT OF PHYSICS

Jan. 20, 1965

Dear Stan,

Sometimes these pictures come out better than expected.

I have just been reading a fascinating correspondence between Schrodinger and Einstein Planck and Lorentz edited by Przibram (Wien Springer).

We hope to see you in N.Y.

Sometime soon

As ever

Rob.

Dear Rabi,

I thank you very, very much for the copies. I am very that I bothered you when you were sick. I hope you are quite all right now.

With my best regards to Mrs. Rabi and yourself sincerely

your old

O.S.

Columbia University in the City of New York

DEPARTMENT OF PHYSICS

Aug. 12, 1963

Dear Stan,

I am sorry we didn't meet in Zurich. We had a pleasant dinner with Franka. Copenhagen was really wonderful. Quite in the old spirit. Bohr would have enjoyed it.

I am sending you a letter which I hope you will agree to sign. It is self-explanatory. We are proposing to send it to the chairman of the Foreign Relations Committee

the Majority and Minority Leaders of the Senate and the presiding officer (the Vice-President).

If you agree please sign the letter and mail it to me.

Cordially,

Robi

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New York, N. Y. November 10, 1944

Dr. Otto Stern

The Molecular Beam world rejoices in the turn of events.

Rabi
Columbia Molecular Beam Lab.

April 8, 1936

Dr. I. I. Rabi
Department of Physics
Columbia University
New York, New York

Dear Rabi:

I should be very glad to have Dr. Cohen with me, but unfortunately I don't see any possibility of giving him any position even at the smallest salary. Our laboratory is in an especially unsettled condition now, because all decisions have to wait for the new president.

I don't think I shall be in New York before Washington, but I expect to be at the meeting most certainly.

Our experiments are progressing slowly but converge definitely towards our old value.

With best regards.

Sincerely yours,

Otto Stern

OS EB

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

April 2, 1936

Professor O. Stern
Carnegie Institute of Technology
Schenley Park
Pittsburgh, Pennsylvania

Dear Professor Stern:

I have just received a letter from Dr. V.W. Cohen who worked with me on Sodium and Caesium. He has been with Ellett for two years and would like to go elsewhere for further experience. Ellett thinks highly of him and would be glad to keep him. However, Cohen would like to work with you for a while and asked me to write you if you have a possible opening. I can recommend him most highly as a very good worker. He was the first real student I had in molecular beam problems and his efforts were very important in the development of our laboratory. He is quite young, unmarried and does not require any large sum to keep him going. He has one of the most likable personalities I've ever met. He is very quiet and reserved and independent in his ideas and has the most sincere love of experimental research of any young man I have ever met.

We have very little that's new to tell about, but I hope to see you at the meeting in Washington, if not before. Please give my best regards to the Estermanns.

Sincerely yours,

I. I. Rabi

I.I. Rabi

P.S. If you happen to be in New York before the Washington meeting I should like to have the opportunity of discussing the question of mechanical velocity selection with you.

IIR

THE ROCKEFELLER FOUNDATION
49 WEST 49th STREET, NEW YORK

THE NATURAL SCIENCES
WARREN WEAVER, DIRECTOR
FRANK BLAIR HANSON, ASSISTANT DIRECTOR

April 30, 1935

Dear Professor Stern:

I have seen a memorandum of your visit to the Foundation offices on Saturday, April 27, in regard to Professor Rabi of Columbia. I regret sincerely that, apart from the grant-in-aid committee of the National Research Council, I do not know of any other source to whom Professor Rabi could apply. As you know, our interest in the natural sciences is now limited to certain fields of experimental and quantitative biology.

With best wishes, I am

Sincerely yours,

Frank Blair Hanson

Frank Blair Hanson

Professor Otto Stern
Carnegie Institute of Technology
Pittsburgh
Pennsylvania

FBH:FEB

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

May 14, 1935

Dear Professor Urey,

Thank you very much for your efforts to get us some money. I was just about to write you that yesterday I received word that we will get $3500 from the Carnegie Institution of Washington. This was arranged through Urey, who also got a somewhat larger sum to build a mass spectrograph for analysis of isotopes. He is trying to concentrate O_2 and N_2.

We have already obtained some preliminary results with the new apparatus. The new discharge tube gave 80% atoms at first. It now gives 50%. We resolved the two peaks in hydrogen very clearly. We have not been able to keep the current steady enough to locate the peaks very exactly. We have only one run that is worth any thing and that yields a mass value of about 2.9 for the proton moment. However I cannot say what the precision is. It is nice however to know that the spin is ½ by this direct method.

Our difficulty with the Pirani is chiefly when the discharge tube is running. There seem to be ~~as~~ currents induced in various place which cause the galvanometer to jump. Up to the present our batteries have not been of sufficient capacity to allow us to take enough readings at any point to locate our peaks with high precision. It may be that we will be able to stabilize the Pirani and get some better results. Otherwise we have nothing new.

Thank you again for the trouble you have taken, and please give our best regards to Estermann and the other people in the Institute.

Sincerely

I. I. Rabi

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

January 30, 1935

Professor O. Stern
Carnegie Institute of Technology
Schenley Park
Pittsburgh, Pennsylvania

Dear Professor Stern:

Could you let me know the correct reference with regard to the suggestion of the type of field employed by Frisch and Segre. I am writing a little paper on the general question of using this method for measurement of the sign of nuclear moments. I do not know which people to refer to for the suggestion of this type of field.

My best regards to the people in the Institute.

Sincerely yours,

I. I. Rabi

I.I. Rabi

IIR:BM

Royal Society Mond Laboratory
Cambridge University
February 4,1935.

Dear Dr. Rabi:

The shop foreman here told me several weeks ago that he was preparing a set of plans of Kapitza's liquid hydrogen apparatus for Stern's use at Carnegie Tech. Upon inquiring into the matter further Fraser tells me that Stern plans to use it in connection with his molecular ray research.

In view of my experience with the hydrogen apparatus here and the necessity of a job next year, Ihave been wondering if there might not be a possibility of getting on the Carnegie Tech. staff with the object of being of material assistance to Stern. Fraser has already approached me on doing some work with him after this term so if Stern thought favorably of my application and there were an opening, I think I could come with enough experience to be of some aid in the actual beam work as well.

Inasmuch as a letter from me to Stern would mean very little, I feel the best way of approaching the situation would be through you. If you could drop him a line saying a good word for me, I would be indebted indeed.

Hoping this finds you, Mrs. Rabi and the children tip-top, and research going equally well, I am

Very sincerely,

Boorse

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

February 13, 1935

Professor O. Stern
Carnegie Institute of Technology
Schenley Park
Pittsburgh, Pennsylvania

Dear Professor Stern:

I am enclosing a letter which I just received from Dr. Henry A. Boorse. He is evidently under a misapprehension as to the extent of my influence with you as you can read from his letter.

However, I can state that Dr. Boorse is an excellent research man, having done a fine piece of work for his dissertation on the magnetic double refraction of liquids. He is at present in England on an excellent fellowship from Columbia.

He is a hard worker, very skillful and intelligent. He has an excellent record as a teacher having been both an assistant and an instructor in our department for a number of years.

Please convey our best regards to the people in the Institute.

Sincerely yours,

I. I. Rabi

I.I. Rabi

IIR:BM

October 12, 1934.

Dr. I. I. Rabi,
Department of Physics,
Columbia University,
New York, N.Y.

My dear Rabi:

As far as I remember, the selector slit was on the side of the weak field ("Furche"), but I am absolutely not sure of it. I suggest you write to Frisch , Universitets Institut of theoretisk PhysiK, 10 Bledgamsvej, Kopenhagen.

We had noticed the influence of the nuclear spin just after the paper was printed, but as we were sure that nothing fundamental would have to be changed, we decided to publish nothing about it. Of course, this influence causes less "tunde over" atoms. We never tried to make an accurate theory, and the paper of Majorana was unreadable for me.(The elaborate paper was, as far as I remember, in Italian.)

I had an awful lot of trouble hunting for living quarters. I hope, that is settled now.

I hope, you and Mrs. Rabi are allright.

With best regards,

Sincerely yours,

O.Stern

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

October 4, 1934

Professor O. Stern
Department of Physics
Carnegie Institute of Technology
Schenley Park
Pittsburgh, Pennsylvania

Dear Professor Stern:

In reading the paper of Frisch and Segrè I find no mention of the positon of the selector slit, that is whether it was toward the strong or weak field side of the position of the undeflected beam. If you happen to remember I shall be greatly obliged if you will let me know.

I have a feeling that this experiment has been misinterpreted because no regard was taken of the nuclear spin. At the low values of the field which they used the coupling between nuclear and atomic spin is very strong compared to the energy of the electron in the external magnetic field. It seems obvious to me therefore that the theory of Majorana does not apply in unaltered form. If one takes account of the nuclear spin it is obvious that the fraction of atoms which change their orientation in such a field should never be greater than of the order of magnitude of about 38%.

We have had some difficulty in reading the paper of Majorana. Either it is full of very serious misprints or of very elementary errors. Would like to know whether you will have had the same experience.

Best regards to your sister and the Estermanns.

Sincerely yours,

I. I. Rabi

I.I. Rabi

May 15, 1934.

Dear Rabi:

How about Mr. Kellogg? I have to know it within the next few days, becaus e we consider also another man for the same job and have to make a decision soon.

I am enclosing herewith the mentioned reprint of Fermi's paper. Please hand it over to Pegram, if he is still interested. I should like to get the reprint back sometime.

I hope to see you and Mrs. Rabi on June 12 or 13 before my sailing.

With best regards,

Sincerely yours,

Columbia University
in the City of New York

DEPARTMENT OF PHYSICS

Dec. 23, 1934

Dear Professor Stern,

At last I have definite information. The dean of the graduate faculty has just informed me that I would be given an assistant for next year. It is not 100% certain because it has not yet been approved by the board of trustees. However I think it is definite enough. Under the circumstances I think it would be best for Kellogg to stay at Columbia. I hope you will be able to find somebody equally good. Many thanks for the patience you have shown.

We have been doing further work on the proton but I will not bother you with details now. There is no other news. We have just discussed Bohr's last paper in the proceedings of the Royal Society. It seems to be a really good idea.

We are looking forward to seeing you in New York. Please call me up as soon as you are free. We will try to arrange real weather for you.

Sincerely yours,

I. I. Rabi

{ Ist es nicht war dass Hamburg ein excellent climate hat, wie ein sinner behauptet? }

April 23, 1934.

Dear Rabi:

Due to a mistake in the calculations (by the inevitable factor 2) , the Deuton becomes a different moment, namely between ½ and 1, probably about 0.8.

Auf Wiedersehen in Washington!

Mit besten Grüssen

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

March 29, 1934

Professor O. Stern
Carnegie Institute of Technology
Schenley Park
Pittsburgh, Pennsylvania

Dear Professor Stern:

Congratulations on your experiment. It must have been very difficult to make Kruger confess his sins this time.

We succeeded with the proton on the very first run and obtained a fine straight split beam. The new field works beautifully. The moment as we find it is 3 ± 0.3. The second experiment under different field conditions checks this result. We will do one more and then try deuterium. The experiment has turned out to be fairly simple. It is a great pleasure not to have to work with the alkalis and clean the apparatus after each run.

We have not yet succeeded in getting final results on the caesium spin due to various leaks which developed in our apparatus.

I expect to see you in Washington and I hope to bring with me definite results on the deuton moment.

Best regards from myself and family to you and the Estermanns.

Sincerely yours,

I. I. Rabi

I.I. Rabi

IIR:BM

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

Dec. 7, 1933

Dear Professor Stern,

Would you please send us some reprints of the work on the proton moment. Our Z. Phys. is at the binders at present.

Progress here is steady but slow. Will you come to New York City before the Cambridge meeting? We may have something to show you by then.

Prof. Franck showed me your Old American Telegram. I hope you really feel that way.

Best regards to the Estermanns in toto.

Sincerely yours,
I. I. Rabi

P.S. If you can come a few days before the meeting I should like to ask the Taylors to come down from Schenectady.

March 27, 1934.

Lieber Rabi,

wir wollen Ihnen nur mitteilen, dass wir gerade die ersten Messungen mit schwerem Wasserstoff gemacht haben. Das Moment des Deutons ist ungefähr 1,5 bis 2 Kernmagnetonen. Zuerst ging natürlich alles schief, deshalb hat die Geschichte solange gedauert.

Was machen Ihre Messungen und wie geht es Ihnen sonst? Kommen Sie nach Wahington? Wir wollen beide hinfahren.

Mit herzlichen Grüssen

December 11, 1933.

Dear Rabi:

I expect to be in New York about Monday the 25th and to stay there until the Boston meeting begins. I would be very glad to me the Taylors. Estermann will probably be in New York at the same time. It would certainly be very nice to have a "Hamburg meeting".

Under separate cover, I am sending you the reprints of the papers on the Proton Moment.

Our work proceeds very slowly.

With best regards

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

Jan. 15, 1932

Dear Prof. Stern,

I am sending some pictures of our experimental curves with Co & Rb. We have also done K and Na. All except K show the effect of the nuclear spin very strongly.

We are now using your method to determine the number of molecules of Rb & Co. The calculations are not yet complete & I cannot therefore tell what the spin is but it seems to be quite large. Probably over 5/2

We are now constructing a magnetic monochromator and we

hope to get more definite results very soon.

These pictures show the striking effect of the nuclear spin.

The effect is best seen by a comparison of the Rb & Cs curves with the K curves. The undeviated beam is the same for all and the temperature is a little lower for Cs & Rb than for K.

Please give my best regards to your sister and to the rest of the institute.

~~Ihr~~

Ihr ergebener

I. I. Rabi.

I'm sorry I have no photographs of the Na results but they are just as striking but at lower fields.

$H = 1080$ and 1360

$\frac{\partial H}{\partial s} = 1900$ and 2400

The cur

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

Dec. 13, 1932

Dear Professor Stern,

I am sending you two short things from the "Patentblatt." We have obtained beautiful curves for the scattering of K atoms in xenon gas, but we have not yet calculated the real scattering curve. The data looks very good. We are continuing the work with much narrower beams. I have hopes that we will be able to say something also about the "Walker" that is the scattering of the beam by the beam itself.

In the other work we have at last been successful and find a spin of $\frac{1}{2}$ for Ra. I am not publishing this result yet because I want to check it a few times first. We obtained a very marked splitting of the monochromatic beam but only in a very weak second analyzing field of about 300-400 Gauss and with very slow atoms. The 53 cm. beam is of the geometrical dimensions and 0.1 mm wide at the receiver.

Please give my best regards to the people in the Institut. I know that I cannot expect you to write but that is no reason why Estermann can't send a few lines to an old alumnus about the work that is going on at the Molekularstrahl-centrum.

Please give out regards to Miss Stern. My wife and little Nancy send their regards.

Sincerely yours

I. I. Rab.

30. A magnetic velocity selector for molecular beams. I. I. RABI AND V. W. COHEN, *Columbia University.*—In experiments on the direct measurement of nuclear spin by magnetic deflection of molecular beams it was found convenient to use a magnetic velocity selector. A portion of a beam of neutral sodium atoms, spread out in a velocity spectrum by an inhomogeneous magnetic field, is selected by a movable selector slit for further analysis. Beams 53 cm long and velocities 1/2 the average for the corresponding temperature have been used. For a given width of selector slit resolution varies inversely as the selected velocity. The beam was detected with the Langmuir-Taylor surface ionization detector. An original beam intensity of 30 cm, galvanometer sensitivity 1 mm $=10^{-12}$ amperes is reduced to 1/300 by the selector with a resolution $dv/v=1/10$. Detector fluctuations are of this order. However, we found it possible to accumulate sodium for 8 minutes on the cold detector wire. By sudden heating we obtain 15 cm ballistic throws corresponding to complete ionization of the accumulated sodium. Residual gases and impurities give no appreciable effect. With the accumulation method it was relatively simple to perform further experiments with the very feeble monochromatic beam.

31. Direct measurement of the scattering of potassium atoms by other atoms and molecules. W. H. MAIS AND I. I. RABI, *Columbia University.*—A narrow beam of neutral potassium atoms (angular width 20′) was scattered by hydrogen, helium, nitrogen, and carbon dioxide under low pressure. The scattering was investigated by means of a surface ionisation detector, as a function of angle with the original beam from 0° to 60°, as a function of pressure of the scattering gas, and of the temperature of the potassium beam. The results allow a determination of the scattering function or f curve within these limits. The small angle scattering expressed in terms of collision radii gives much larger collision radii than either kinetic theory radii or radii obtained from lattice distances. However, if one defines as a collision a change of direction of the potassium atoms of one-half degree, then the computed radii approach more closely the values to be expected. For the temperature of the potassium atoms the DeBroglie wave-length is of the order of 2×10^{-9} cm and we consequently would hardly expect any contribution to the scattering of the potassium atoms due to diffraction by the scattering gas atoms. The results may therefore allow of an evaluation of the mutual interaction between the potassium atoms and the atoms of the scattering gas.

Gouds mith
Haag
Koninginne Gracht 134

Tl: Grundzustand
mit Hyperfeinstruktur
im Stern-Gerlach-Effekt

20 von Sam		25000 Ges
$+\frac{1}{3}$		$+\frac{1}{3}$
$+\cdot 23$		$+\cdot 25$
$-\cdot 23$		$-\cdot 25$
$-\frac{1}{3}$		$-\frac{1}{3}$
$\cdot 33$		
$\cdot 23$		
0		
$-\cdot 23$		
$-\cdot 33$		

Samuel Goudsmit

Ann Arbor, Michigan.

N.B! Vorläufige Mitteilung!!!
S.G.

$$\frac{\partial E}{\partial H} = \frac{1}{2} g\sigma \qquad \pm \frac{1}{2} \cdot \frac{1}{2} \frac{2 g^2 \sigma^2 H}{\sqrt{A^2 + (g\sigma H)^2}}$$

$$-\frac{1}{2} g\sigma$$

$$= \pm \frac{1}{2} \frac{g^2 \sigma^2 H}{\sqrt{A^2 + (g\sigma H)}} = \pm \frac{1}{2} g\sigma \frac{g\sigma H}{\sqrt{A^2 + (g\sigma H)^2}}$$

$g = 2/7$

$\pm l \quad \pm \frac{1}{3}$

$\quad\quad -\frac{1}{3}$

$\pm \frac{1}{3} \cdot$ ~~250~~

$20000 \rightarrow \pm \frac{1}{3} \frac{.67}{\sqrt{.50 + .44}} = \pm \frac{1}{3} \frac{.67}{.97} = \pm .23$

$25000 \rightarrow \pm \frac{1}{3} \times \frac{.84}{\sqrt{.50 + .72}} = \pm \frac{1}{3} \frac{.84}{1.11} = \pm .25$

$\sigma H = 2$

$\sigma H = 1$

$A^2 = 0.5$

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

June 3, 1931

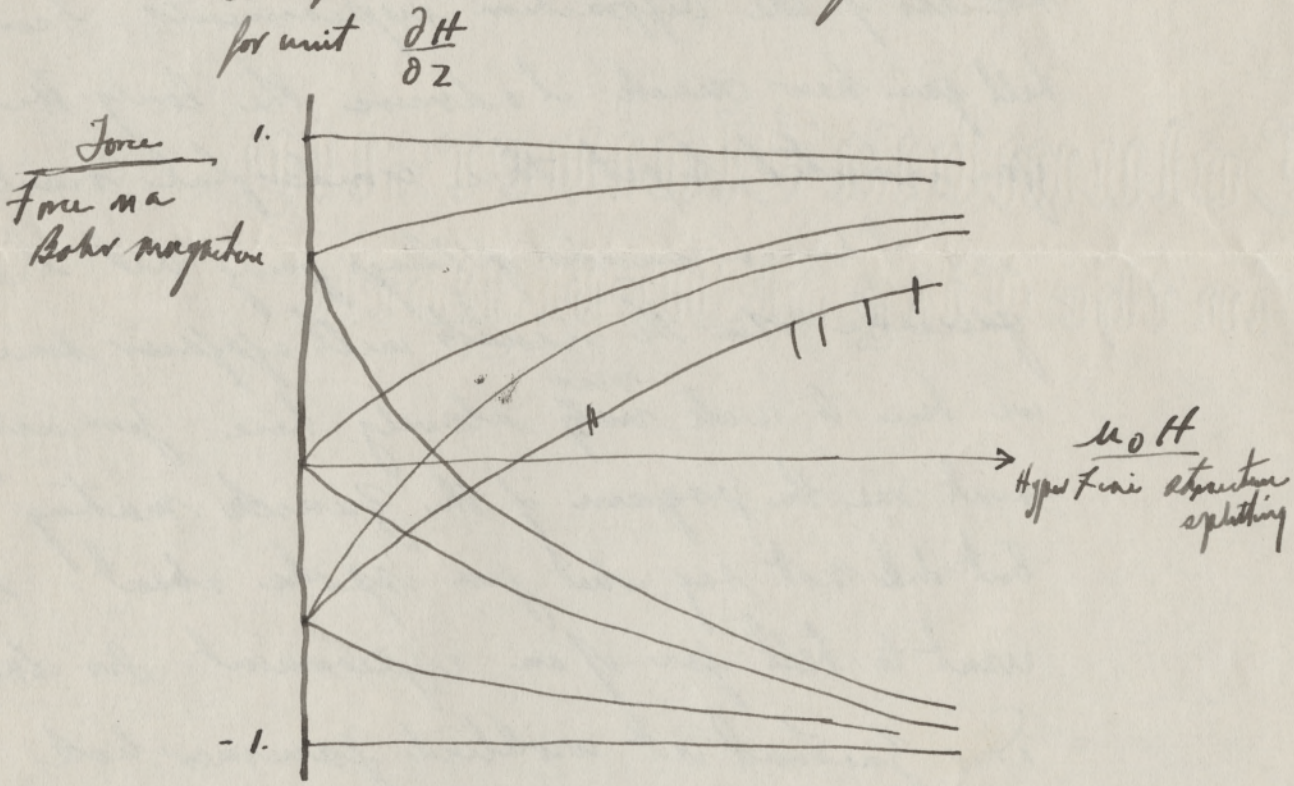
Dear Professor Stern,

I haven't written you since I saw you and want to apologize. As in some other things such as my love for Molecular beams I resemble you in that respect.

Everything is going very nicely here. Helen and the baby are fine. We were very glad to see the Estermann's and see the beautiful results of the diffraction experiments. I cannot tell you how much I admire the lovely tricks you invented to get these wonderful results.

I have a number of things going but I cannot guarantee when the results will appear since we have to work ~~very~~ very slowly here. Zemansky sent me the program of the Zurich meeting but did not say what you spoke about. I want to tell you of an experiment I'm doing. Der Zustand ist wirklich jammerlich. Ich muss den Experiment selbst machen-

The idea is to determine nuclear spin. Not nuclear moment. I'm hoping to do Caesium Rubidium and Sodium. It is simply this. If the deflections are made in small fields such that the energy $\mu_0 H$ of the electron in the field is less that the hyper-fine structure splitting one gets the incomplete Paschen-Back effect for the interaction of nuclear spin and electron spin and the pattern is quite different. If the beam is monochromatic the picture is like this



This picture is, for a nuclear spin of $3/2$.