sent to

c/o The Clarendon Laboratory,
OXFORD.

12th January, 1936.

Dear Breit & Wigner,

Many thanks for your letters. I am sorry I did not make myself more clear. When I said that neutrons which go easily throught cadmium but are strongly absorbed in indium, have energies of about 3 volts, I concluded this from observing the absorbtion of these neutrons in boron.

The absorbtion of boron and lithium is due to a straightforward disintegration, and not to radiative capture. I see no reason to suppose that resonance plays any part in the absorbtion process of slow neutrons for these two elements, and, therefore, trust that the absorbtion of boron and lithium will be proportionate to the wave length of the neutron. I have compared the half-value thickness of boron for the unfiltered neutrons with the half-value thickness of boron for the neutrons which go easily through cadmium, and are strongly absorbed in indium. Their ratio is of the order of magnitude 1 : 10. I assume that the bulk of the unfiltered neutrons which activate indium has thermal energies (1/30th) volt), and get therefore 3 volts for the neutrons in which we are interested. This apparently means that the absorbing region of cadmium does not extend much over 3 volts (all allowances having been made for the fact that the absorbtion would fall off legitimately with $\sqrt{v}$). ~~xRoxxaxxthinkxitxis~~

Do you think it is worth while to establish that a number of elements have such narrow regions of absorbtion? This could be established in the same way as for cadmium, also for other elements which also seem to have their strongly selected absorbtion very close to Zero energy. For elements which have their lowest selective region at higher energies, the method is different but the experiment is also feasible.

If the nucleus can discriminate between neutrons, the energy of which differs by ΔE, we have to conclude that the virtual time which the free neutron has been in the nucleus is larger than $\frac{\hbar}{\Delta E}$.

I was very much cheered up by Wigner's idea, and wish he would make a more detailed statement. This is what he writes - (translated from Hungarian) "I think I can explain your experiment and other deviations from the theory. The explanation is obvious. One has only to assume discreet energy levels in the continuous spectrum of the neutron, i.e., we have to deal with the inverse of the Auger effect in a similar way to that which Polanyi and I have described in the Z. f. Phys."

I hope Breit will persuade Wigner to work out a few essential points and publish a Letter in the Physical Review. Everybody is interested in this question right now. Fermi, to whom I sent my manuscript, sent me a manuscript of his on the same subject and wrote that he is completely puzzled about these selective effects. His paper, in the meantime, has been published in the "Ricerca", and contains very many beautiful observations, but nothing which raises really new issues.

Here are a few questions which I should like to put to Wigner:-

1). What is the average distance of neighbouring resonance levels which he gets for an element of the atomatic weight 100 in the neighbourhood of Zero energy? How does this distance vary with the number of neutrons and protons (atomic wight) in the nucleus? Will he be able to explain it if we find that perhaps 10 heavier elements out of some 200 have resonances between Zero and 10 volts?

2). How does the ratio of absorbtion and scattering vary ~~in the neighbourhood of a resonance level?~~ across the resonance region Does it remain constant? I mean apart from the trivial factor $1/v$.-

3). What is the order of magnitude of the number of ~~actions~~ transitions from Wigner's energy level to deeper energy states which lead to γ emission?

Could Wigner account for narrow absorbtion regions of the order of magnitude of 1 volt width, and at the same time explain that the re-emission of the neutron is more than 100 times rarer than γ emission?

Even if Wigner cannot be persuaded to publish something on this matter right away, he might write down as much as he can state at present in the form of a manuscript, and send me a copy of it (which I should like, with his permission, to send on to Fermi).

I wonder whether, in your opinion, it would be worth while to make a closer study of the elastic scattering. I am primarily interested in it right now because it yields an value for the upper limit of the wave length of our selectively absorbed neutrons. If we find a cross section σ for selective capture, and if we find, for instance, that the ratio of scattering to capture is less than 1/100, then obviously we can conclude that the wave length λ is larger than $\sqrt{\pi \sigma \times 100}$ (assuming $l=0$)

I was in touch with Peirls in November about selective absorbtion, but the only thing we concluded was that there is little hope of improving the situation by assuming long range forces. It did not occur to us at all to seek the solution in the direction in which Wigner is moving. We overlooked this possibility in spite of the fact that, as early as ~~January,~~ June we reached a general agreement in England not to worry too much

about the lack of scattering in strongly absorbing elements; at that time, under the leadership of Teller who referred to a remark of Bohr's, it was tentatively resolved that a theory which takes into accout the composite nature of the nucleus (see Wigner and Polanyi) might lead us out of this difficulty. I mention this chiefly in order to show Wigner how important it would be to get from the stage of an apercu to the stage of a statement put forward with conviction, and if possible published.

Please do not think that I over-estimate the importance of slow neutrons, and if Breit finds that other more important work of Wigner's is in danger, if he writes down his ideas about slow neutrons, I shall refrain from further pressing this point.

With kind regards to you both,

Yours,

420 West 116th Street
New York City
June 7, 1940

Dr. G. Breit
Department of Physics
The University of Wisconsin
Madison, Wisconsin

Dear Breit:

Many thanks for your letter. I am enclosing a copy of Turner's first letter to me to which I replied that if he would be willing to have his paper delayed I would be glad to forward his manuscript to the appropriate authorities. I also enclose a copy of Turner's second letter of which you have apparently received a copy. Subsequently, I saw Turner. He expressed his willingness to have his paper delayed and assuming that the paper has already passed out of the hands of Tate, he proposed to advise the New York office of the American Institute of Physics (Miss Mitchell) accordingly. Meanwhile, I was supposed to forward his paper to the Government departments interested and ask them to notify Turner officially concerning their wishes in this matter. I take it that since, in the meantime, you have arranged with Tate to receive all papers on uranium, this somewhat clumsy procedure upon which Turner and I agreed need not take place and that, accordingly, I need not take any further steps in the matter of Turner's paper except communicating with you about it.

Clearly, for you to be in a position to fulfill your function, it is necessary that you should be fully informed of the work of Fermi and myself as well as other related work. It would be unsatisfactory for you to have Fermi's and my personal opinions without being informed of our reasons. This makes it necessary that we should be free to give you information concerning our work.

This and other considerations make it advisable that a small group of scientists should receive full information on the work which is being carried out and that you should be a member of this group. I have been lately taking a strong stand in favor of such a solution, and I understand that the 13th of June may be fixed as the time and Washington, D. C. as the place for a meeting. No doubt, you will receive official notice within the next few days from the proper authorities. It would be very useful if you could come to New York a day or two earlier so that we may have a number of informal discussions, in connection with the various complicated questions which will necessarily arise. If possible, thought should precede action.

I take it that as far as preventing publication goes you are already handling the situation efficiently, and I have communicated your suggestion, that the Journal of Chemical Physics and the American Chemical Society should fall in line, to Urey. I told him that you have already asked for such control through official channels.

Yours sincerely,

(Leo Szilard)

THE UNIVERSITY OF WISCONSIN
MADISON

DEPARTMENT OF PHYSICS

June 5, 1940

Dear Szilard:

I have received from Tate Turner's Letter to the Editor and a copy of Turner's letter to you. I do not know what you have written to Turner but it appears that you do not think the letter should be published, or at least that it should be delayed. I should like to know your opinion and Fermi's very much indeed.

As chairman of the committee of the Division of Physical Sciences on uranium fission I have written Tate concerning the advisability of control over such publications. Tate is very willing to cooperate and the present understanding is that he will send me all such papers so that the committee may decide on whether they should be published. The committee consists of Pegram, Beams and myself. I have also asked for Wigner to be appointed. It may be best for you and Fermi not to be officially on the committee but I plan, of course, to have the benefit of your advice.

I should suggest that Fermi speak to Urey asking for control of publications in the Journal of Chemical Physics and in the publications of the American Chemical Society. I have asked for such control through official channels but there are unavoidable delays.

Sincerely yours,

G. Breit

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Y

420 West 116th Street
New York City
June 7, 1940

Dr. G. Breit
Department of Physics
The University of Wisconsin
Madison, Wisconsin

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Yours sincerely,

(Leo Szilard)

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THE UNIVERSITY OF WISCONSIN
MADISON

DEPARTMENT OF PHYSICS

June 20, 1940.

Dr. Leo Szilard
Department of Physics
Columbia University
New York City

Dear Szilard:

I should like to thank you for the many discussions we have had in New York and for your hospitality. It seems to me that matters would be helped along very much if the intermediate experiment could be performed and if the set up could be kept flexible. My impression is that in work of this type practical success in a limited time may depend considerably on detailed planning regarding the ease of assembley and flexibility. I still think that more rapid progress will be achieved by arranging an intermediate or full scale experiment rather than by careful measurement.

Sincerely yours,

Gregory Breit

G. Breit

420 West 116th Street
New York City
July 6, 1940

Dear Breit,

Many thanks for your letter. Following the conversation we had on our way from Washington to New York. I have given some thought to the issue mentioned in your letter and I am now entirely convinced to your point of view. Consequently, I am taking a strong stand in favor of an experiment on as large a scale as possible. This large scale experiment, or some intermediate experiment, operating with at least five tons of uranium ought to have the right-of-way before the general survey of the neuclei values involved. Nevertheless, this general survey will also have to be carried out.

There is another point about which I became converted to your opinion. I now think that steps should be taken to prevent certain publications in Nature and the Proceedings of the Royal Society of London. With the collapse of France there is an immediate danger that Joliot and his co-workers will start publishing something of their previous work in these periodicals.

On the other hand I feel even more strongly than before that your attempt to prevent publication will break down unless we create a satisfactory substitute in the form of some private publication. If that is not done there will be a growing tendency towards indulgence and finally practically everything will be published as it has been in the past. I wonder whether you have given the matter further thought since your return to Madison.

With kindest regards.

Yours,

(Leo Szilard)

THE UNIVERSITY OF WISCONSIN
MADISON

DEPARTMENT OF PHYSICS

July 16, 1940.

Dr.Leo Szilard
420 W.116th St.
New York City

Dear Szilard:

I am sorry to be answering your letter with some delay. It is partly due to the fact that I have been waiting for a clarification of some official sides of the arrangements which have not materialized so far.

I wonder whether you would consider it satisfactory to have a request from the President of the National Academy to the Royal Society in which he would ask for a return of manuscripts from the United States having a possible military value. The request could mention several subjects so as not to stress any one of them unduly. I do not see what can be done about French work. In a way it is to our advantage if they publish it in England. If they do not the chances are large that they will publish in France or Germany.

I have written to Tuve, Lawrence, DuBridge and Condon asking their opinion regarding the procedure with papers and asking to be informed concerning the special fields that interest them. I have received no objections and some encouragement.

Personally I am in favor of having a fairly wide circulation of the papers. You will recall, however, that Briggs, Pegram and Urey are not and that their reasons are of an official character. It would help me very much if you were to let me know who, in your opinion, should be informed of the contents of papers and in which branch of the subject. I believe that in the consideration of special cases there will probably not be many objections.

I am very glad to learn that you are considering large scale experiments.

Sincerely,

Gregory Breit

G.Breit

Memo Breit. December 17, 1940.

It appears very probable that the capture cross section of carbon is so small that it is not an important factor for the purpose of this discussion. There has always been a good reason to believe that the capture cross section of natural carbon which contains less than 1% C^{13} is less than o.ool for carbonatum. C^{13} being a very light element ~~of an odd atomic number~~ will hardly have a cross-section above 0.o5 and its cross section may be considerably smaller. This forcast is to some extent supported by the fact that F^{19} has a cross-section of 0.01 and N^{15} has a cross section of less than 0.01 (Manley, Haworth and Luebke.) We thus expect almost certainly to have in natural carbon an absorbtion cross section due to C^{13} of less than 0.0005 per carbonatum. C^{12} having an even mass number and an even atomic number and the general experience shows that barring certain rare cases by resonance possible such an element absorbes perhaps 100 times less than the odd elements in its neighbourhood thus C^{12} if we expected to have a cross section of less than .0005 and possibly much lower. Consequently we can count with practical certainty on having a capture cross section in pure carbon of less than .001. Pure carbon is at present only in small quantities and at a high price obtainable. Consequently we are faced with the practical problem of securing at a reasnable price larger quantities of moderately pure carbon within an acceptable delivery time. This might lead to a compromise inasmuch as we might wish to carry out a large scale experiment with a graphite having an absorption above .001/ Of the possible geometrical arrangement of uranium in carbon have given particular attention to two different arrangements. The first arrangement is a lettice of uranium spheres embedded in carbon. This arrangement was chosen as the most efficient from the point of view of keeping the fraction of the neutrons which are absorbed by uranium at resonance as low as possible. The resonance absorption of uranium has been treated in the following way: We distinguish resonance neutrons of low energies say below 200 volts and resonance neutrons of higher energies above 200 Volts. Concerning the resonance neutrons of low energies we calculate as if the uranium were within a certain region say between 2 and 20 volts or between 2 and 200 volts black for neutrons of this energy. Concerning the high energy resonane neutrons we are not able to estimate the ~~fact~~ effect due to the absorption of uranium in any of the cases in which this ~~fact~~ effect becomes important. Instead we chose to consider such arrangements in which the absorption of high energy resonane

neutrons is negligible. This is the case for arrangements in which the ration of uranium to the effective amount of carbon is of the order of magnitude of 1:10 by weight. ~~In calculating this ratio~~ however, one must only by effective amount of carbon we mean the amount of carbon which is effective in producing resonance neutrons rather than the total amount of carbon. This will be elaborated further below.

The arrangement in which small spheres (of uranium) are embedded in graphite has both the advantage of making it possible to use larger ratio of carbon to uranium and also to have a small fraction of the neutrons absorbed as low energy resonance neutrons.

In the case of such a lettice one may use uranium spheres of a fixed and make a ratio of uranium to carbonate eff. as low as desired by using graphite of a sufficiently low density. There is, however, in this case no reason to reduce this ratio below a value which can be obtained already using ordinary graphite of the density of 1.7 and even graphite of the density 1.9 could be used if desired. If, however, a different arrangement is desired it may be advisable to use graphite of lower density. This second arrangement which we have in mind consists of long cylinders of uranium embedded in graphite and that in cross section one obtains a plain lettice of uranium rings and circles

Memo Breit. Second draft December 17, 40.

It appears practical certain that chain reaction can be maintained in a system composed of a lattice of uranium spheres which are embedded in pure graphite. There is no evidence at present to indicate that carbon has an appreciable capture cross-section for thermal neutrons and that the absorbtion observed in graphite is due to carbon rather than to the impurities of the particular form batch of graphite which was used for such experiments. On the other hand there is good reason to believe that C^{13} will most likely have a capture cross section below 0.05 and probably much smaller. Whereas C^{12} will most probably have a capture cross section below 0.005 and probably much smaller. We may therefore count on pure graphite having a thermal neutron capture cross section below 0.001. There may be practical difficulties in obtaining pure graphite and it is possible that as a result of the compromise (due to high price or long delivery time) we shall have to accept graphite which has a higher capture cross section.

Two geometrical arrangements have been considered in some detail: a lattice of uranium spheres and long cylinders of uranium embedded in graphite. The cross section of which is represented by a plane lattice. The first of these arrangements is the most favorable from the point of view that the fraction of the neutrons which are absorbed by uranium at resonance in this arrangement can be made very small. The second of these arrangements is less favorable from this point of view but has very great practical advantages and might therefore ultimately prevail.

A third arrangement, namely that of plane sheets of uranium embedded in graphite so that a graphite layer is sandwiched between two uranium layers will also be occasionally mentioned in order to illustrate certain points, but is not considered as a practical possibility.

Memo Breit. Dec. 27, 1940.

If we have ~~a lattice of uranium spheres or cylindrical~~ bodies of uranium embedded in graphite we may write

$$q = \frac{\gamma^{th}/\gamma^{res}}{1+\gamma^{th}/\gamma^{res}}(1-\alpha)$$

If we have (lattice of) spheres of cylinders the radius of which is small compared to the distance of two neighbouring spheres of cylinders ~~we can write~~ it is useful to write

$$\frac{\gamma^{th}}{\gamma^{res}} = \varepsilon f(\alpha)\alpha \qquad f(\alpha) > 1$$

For small radii of uranium spheres or cylinders f will approach 1. In those cases in which we are interested f will be closer to 1 in the case of the spheres than in the case of the cylinders. Since f varies (with α) ~~will~~ only slowly ~~be~~ we obtain a reasonable estimate of the value of α_m at which q becomes the maximum by considering f as constant ~~if~~ in differentiating q after α. We thus obtain $\alpha_m = \frac{1-q_m}{2}$

and

$$\varepsilon f = \frac{4q_m}{(1-q_m)^2}$$

~~Using this expression we can now estimate V in the following way:~~

ε this means that half of the neutrons which are not absorbed in the thermal region by U are absorbed by carbon. Consequently the other half is also by U at resonance and we have

$$\gamma^{res} = \frac{1-q_m}{2} Q V_m \rightarrow \text{In case of } V_m \text{ we have to work out that ....}$$

In case of spheres we have

$\gamma^{res} = \gamma^{res} = 4\pi Q R B^2 (1+R/B)$ and for the ratio of Volumes:

$$V/\frac{4\pi R^3}{3} = \frac{6}{1-q_m}\frac{B^2}{R^2}(1+R/B)$$

for $B=6.5$, $R=3$, $q_m = 0.8$ we have for instance $\frac{V}{\frac{4\pi R^3}{3}} = 205$

In case of cylinders we have

$$\gamma^{res} = \gamma^{res} = 2\pi B^2 \frac{R}{B}\frac{K_1(R/B)}{K_0(R/B)} Q \quad \text{and}$$

for the ratio of Volumes

$$V/\pi R^2 = \frac{4}{1-q_m}\frac{B^2}{R^2} \times \frac{R}{B}\frac{K_1(R/B)}{K_0(R/B)}$$

i.e. almost 1.5 times less than in case of spheres

$B = 6.5$, $R = 3$, $R/B = 0.67$, we have for instance $q_m = 0.8$, $\frac{V}{\pi R^2} = 142$

It is perhaps of interest first of all to compare these three different arrangements in case there is no carbon absorption to be considered. We find then for these three cases for the number of resonance neutrons captured by single sphere, cylinder, or plane embedded in an infinite amount of graphite for one fast neutron emitted by the sphere by one centimeter of the cylinder or one square centimeter of the plane the following expressions:

In these expressions q^* stands for the number of resonance neutrons produced per cc and second in the graphite in the neighbourhood of the sphere, the cylinder, or the plane.

Writing for these three cases

For we write

assuming as the initial energy of the neutrons and as the energy of the resonance neutrons 2o volts and using them for the value of 2.44 cm we obtain

Since will be rather larger than the value quoted which holds for thermal neutrons the above values for are a rather conservative. If there were higher resonances to consider such low values of might be reckoned with, but using metal spheres of 3 cm radius or cylinders of two cm radius will hardly make it necessary to ~~increase~~ decrease the value for on this account.

Memorandum Breit December 28, 1940

Though little is known about the higher resonance of uranium it should be possible to estimate the behavior of a system composed of uranium and carbon. If the uranium is used in a form of high density in the shape of spheres or cylinders which have a radius of a few centimeters and if the ratio of uranium to carbon is of the order of magnitude of 1 to 10 by weight, we then have a system in which it is fairly safe to assume that the higher resonance will no longer play an important role and we therefore can treat the resonance absorption by assuming that the uranium is black for neutrons in an interval stretching from 1/5 of the resonance energy to the double of the resonance energy, corresponding to 15 collisons in carbon or a range of B = 6.5cm in graphite of density of 1.7cc per gm. This may be rather high value for the range and lead to too pessimistic values in particular if we assume that the resonance neutron density varies at the surface of the sphere or cylinder.

If such small bodies of dense uranium are used (uranium metal has a density of about 20 gm per cc) a thermal neutron which passes through the uranium sphere or cylinder without being absorbed has little chance to pass through the same uranium body again. We would therefore not lose anything in this respect if instead of graphite of density 1.7 gms per cc we used graphite of a lower density. A lowering of the density of graphite would on the other hand have the advantage of reducing the resonance absorption of the uranium body in case of a lattice of spheres or cylinders. In the case of a lattice of spheres the resonance absorption may be already low enough but in the case of the cylinder a reduction of the resonance density should perhaps be considered. The use of very low graphite density is naturally impractical because it would lead to rather large total amounts of graphite.

The situation is rather different is uranium oxide is used both because uranium oxide has a lower density and because its scattering cross section per uranium atom is larger. If

Memorandum Breit December 28, 1940

In case of a very small capture cross section of carbon the optimum value of α corresponds to a very large value of V. It is then no longer permissable to consider the production of resonance neutrons in the carbon as uniform and to treat f as constant in differentiating after α. In the limit of no carbon absorption we should obviously have instead of No. 5

(6) $$\frac{J^{res}}{QV} = 1 - q_m$$

As long as we can treat the resonance neutron production in graphite if uniform we may use expression No. (5) and thus obtain:

(a) In the case of spheres

$$J^{res} = S^{res} = 4\pi Q R B^2 (1 + R/B)$$

and for the ratio of volumes

$$\frac{V}{\frac{4\pi R^3}{3}} = \frac{6}{1-q_m} \frac{B^2}{R^2} (1 + R/B)$$

for B = 6.5cm, R = 3cm, q = 0.98 we have for instance $\frac{V}{\frac{4\pi R^3}{3}} \simeq 200$

(b) In case of cylinders we have

$$J^{res} = S^{res} = 2\pi Q B^2 \frac{R}{B} \frac{K_1(R/B)}{K_0(R/B)}$$

and for the ratio of volumes we have

$$\frac{V}{\pi R^2} = \frac{4}{1-q_m} \frac{B^2}{R^2} R/B \frac{K_1(R/B)}{K_0(R/B)}$$

for B = 6.5cm, R = 2cm and q = .98 we have for instance $\frac{V}{\pi R^2} =$

Memorandum Breit December 28, 1940

If we have a lattice of uranium spheres or some other bodies of uranium embedded in graphite we may write in case of an infinitely extended system

(1) $$q = \frac{\gamma^{th}}{\gamma^{th} + \gamma^{res}} (1-\alpha)$$

We wish first to consider the case in which the production of resonance neutrons in carbon can be considered as approximately uniform. It is then useful to write

$$\frac{\gamma^{th}}{\gamma^{res}} = \alpha \varepsilon f(\alpha)$$

and accordingly

(2) $$q = \frac{\alpha \varepsilon f}{1 + \alpha \varepsilon f} (1-\alpha)$$

We are interested in the case in which the distance between the uranium spheres in the lattice is large compared to the radius of the sphere and in this case f will approach one. Since in our case $f(\alpha)$ changes only slowly with α we may determine the value of α for which q becomes the maximum by considering f as constant in differentiating q. We thus obtain

(3) $$\alpha_m = \frac{1-q_m}{2}$$ and (4) $$\varepsilon f = \frac{4 q_m}{(1-q_m)^2}$$ those

The first of these two equations means that half of the neutrons which are not absorbed in the thermal region by uranium (half) are absorbed by carbon. Accordingly, the other half must be absorbed by uranium at resonance and we have

(5) $$\gamma^{res} = \frac{1-q_m}{2} \alpha V$$

Memorandum Breit December 29, 1940

If the radius of the sphere is not large compared to the mean free path in graphite we are no longer justified in using the diffusion equations. It is however, easy to show that for a sphere is is black for thermal neutrons as well as black for resonance neutrons have the radius of which is small compared to the mean free path we also have

$$\varepsilon = \frac{A^2}{B^2}$$

We shall therefore also use for small spheres the formula

$$\varepsilon = \frac{A^2}{B^2}\,\frac{1+R/A}{1+R/B}\,\varphi$$

Instead of regulating calculating φ from the diffusion equation we shall however for uranium spheres of 2 to 3 cms radius calculate φ by neglecting both the scattering of thermal neutrons by uranium and also the fact that a thermal neutron which passes through a uranium sphere will occasionally return to the sphere after being scattered by carbon atoms. We thus introduce two sources or error which will to some extent balance each other and in calculating φ

simply as probability that a thermal neutron which reaches the surface of the uranium sphere will be absorbed within the sphere rather than pass through it. (In the first approximation φ would be proportionate to the ratio of the volume of the sphere to its projection πR^2. ~~In the second approximation which we propose to use we have~~

$$1-\varphi = \frac{1}{\pi R^2}\int_0^R e^{-a\,2\sqrt{R^2-r^2}}\,2\pi r\,dr$$

$$= \int_0^1 e^{-2aR\sqrt{y}}\,dy$$

$$= 1 + \sum_1^\infty \frac{(2aR)^n}{n!}\,\frac{1}{\frac{n}{2}+1}\,(-1)^n$$

$$\varphi = -\sum_1^\infty \frac{(2aR)^n}{n!}\,\frac{(-1)^n}{\frac{n}{2}+1}$$

where $a = \frac{\sigma_a(u)}{\sigma_s(u)}\,\frac{1}{\lambda_{sc}(u)}$ for $\lambda = 2$ cm $a = \frac{1}{4}$ cm

for comparison see page 16

By carbon atoms we thus introduce two sources of error which will to some extent balance each other, in calculating φ simply as the probability that a thermal neutron which reaches the surface of the uranium sphere will be absorbed within the sphere rather than pass through it. We thus find

From this we find for $\lambda_{mc}^{a} = 2$ cm which corresponds to uranium density of 18 gm per cc and leads to value of $a = \frac{1}{4}$ $\varphi(R=2) = 0.47$ $\varphi(R=3) = 0.61$ and accordingly

$$\varepsilon(R=3) = \frac{A^2}{B^2}\underbrace{(1+R/A)}_{1.05}\frac{1}{2.4}; \quad \varepsilon(R=2) = \frac{A^2}{B^2}\underbrace{(1+R/A)}_{1.03}\frac{1}{2.8}$$

$A = 60$ cm

For very small spheres or cylinders exposed to an isotropic neutron radiation the number of neutrons absorbed is proportionate to the volume. Since the ratio of volume to surface is 3/2 times larger for a sphere than for an infinitely long cylinder of equal radius, the value of φ for a cylinder will be by the factor 3/2 times larger than the value of φ for a sphere of equal radius.

$$\varphi_{cyl}(R) = \tfrac{3}{2}\,\varphi_{sph.}(R)$$

For spheres and cylinders of finite radius we have

$$\varphi_{cyl}(R) < \tfrac{3}{2}\,\varphi_{sph}(R)$$

for a uranium cylinder of 2 cm radius ~~we have~~ we may use as an approximate value $\frac{3}{2} \times 0.47 = 0.7$

Memorandum Breit of December 29 continued January 5, 1941

For a black cylinder we have $\varepsilon_c = \frac{A^2}{B^2} \frac{Z(R/A)}{Z(R/B)}$

and using the values of $R/A = \frac{1}{30}$ $R/B = 0.3$ ($R = 2$ cm, $B = 6.5$ cm)

$Z(R/A) = \frac{1}{3.55}$; $Z(R/B) = 0.67$

($A = 60$)

we obtain $\varepsilon_c = \frac{A^2}{B^2} \frac{1}{3.55} \frac{1}{0.67} = \frac{A^2}{B^2} \frac{1}{2.38}$

For a cylinder which is not black which has a small radius of the order of 2 cm we shall write

$$\varepsilon_c = \frac{A^2}{B^2} \frac{Z(R/A)}{Z(R/B)} \varphi$$

with $\varphi \cong 0.7$ we thus obtain (for $R = 2$ cm)

$$\varepsilon_c = \frac{A^2}{B^2} \frac{1}{3.4}$$

$$Z(x) = x \frac{K_1(x)}{K_0(x)}$$

LATTICE OF URANIUM SPHERES

If we have now an infinitely large number of uranium spheres forming a lattice embedded in an infinite mass of carbon and want to calculate the ratio of the number of thermal neutrons and resonance neutrons absorbed by the uranium spheres, we shall again assume for the time being that everywhere in the carbon the same number Q of neutrons enter the resonance region and the thermal region per c.c and second.

In the absence of uranium the thermal neutron density in the graphite is given by $\rho_0 = \frac{Q}{\sigma(C)}$. If a lattice of uranium spheres is embedded in the carbon, the average neutron density $\bar{\rho}$ in the carbon is reduced by some factor α .

$$\bar{\rho} = \alpha \rho_0$$

Since the number of neutrons captured per second by carbon is proportionate to the average neutron density, and since in the absence of uranium all the neutrons produced are captured by carbon, the fraction of the neutrons which is captured by carbon in the presence of the uranium lattice is given by α . Correspondingly, the fraction of the neutrons which are absorbed by the uranium lattice is given by $1-\alpha$.

In order to determine the number of thermal neutrons J^{th} absorbed per second by one uranium sphere within the lattice, we may consider the following: a single uranium sphere, which is embedded in carbon, does not appreciably affect the thermal neutron density at distances which are large compared to R. Equation No. 5 shows that even for a "black" uranium sphere at a distance of 2 R from the center of the sphere ρ has already reached the value of $1/2 \rho_0$. For this reason, the uranium spheres within the lattice affect each other with respect to their thermal neutron absorption only in so far as the presence of these spheres in the carbon determines the average neutron density, and we have

$$J^{th} = \alpha J^{th} \qquad (20)$$

Further, since the distance L between neighboring uranium spheres within the lattice will be large compared to B, the range of the resonance neutrons in carbon, we have for J^{res} , the number of resonance neutrons absorbed by a uranium sphere within the lattice

$$J^{res} = J^{res} \qquad (21)$$

Multiple overlap
multisphere

$$\gamma = -\sum \frac{(2aR)^n}{n!} \frac{1}{\frac{n}{2}+1} (-1)^n$$

O.K.

$$e^{z}(1+z)$$

$$-\frac{z^5}{5!} + \frac{z^6}{6!}$$

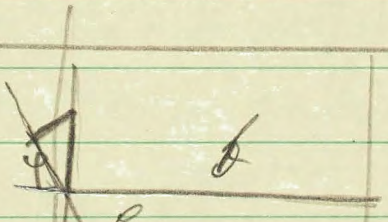
$$-\frac{z^6}{5!}$$

$$z^6\left(\frac{1}{6!} - \frac{6}{5!6}\right)$$

$$-\frac{5z^6}{6!}$$

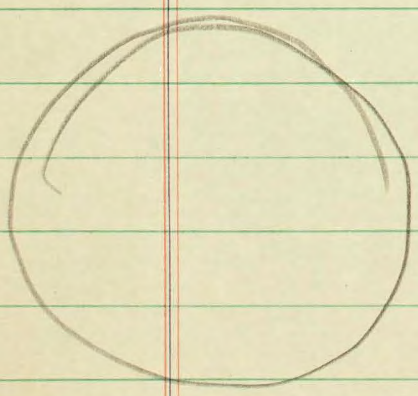
sec.

$$-\frac{4a^2R^2}{2} \quad \frac{1}{3}$$



$$\int_0^{\pi/2} \cot\varphi \, d\rho \qquad -\frac{ad}{\cos\varphi}$$

$$(-1)^n \left(\frac{1}{\cos\varphi}\right)^n \frac{\cos\varphi}{n!}$$



Adams Bashforth doc 29

$R=2$

$$\varphi = + \frac{1}{1}\,\frac{1}{\frac{1}{2}+1} - \frac{1}{2}\,\frac{1}{2}$$

$$+ \frac{1}{6}\,\frac{1}{2.5} - \frac{1}{24}\,\frac{1}{3}$$

$$+ \frac{1}{120}\,\frac{1}{3.5} \cong$$

$R=3$		$R=3$
$\cong$ 0.666	1.5	0.25 2.25
+ 0.0666	3.35	0.014 5
0.0024	7.6	
0.7350		0.264
− 0.264		
0.471		

$\rightarrow 0.471 = \varphi(2)$

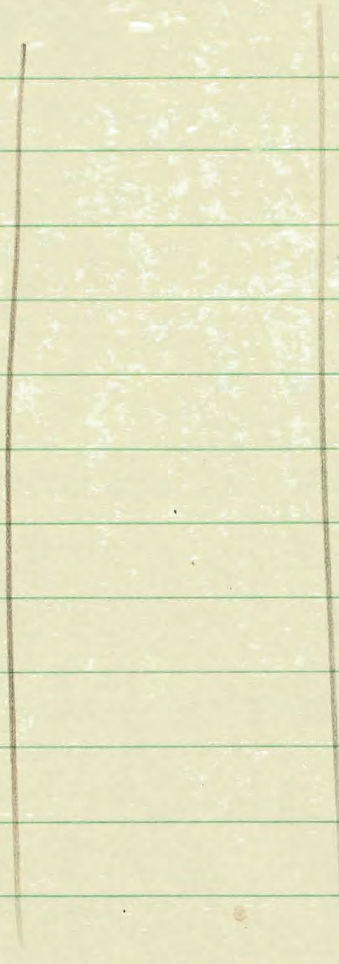
~~$\frac{11}{3}(aR) = (aR)^2 \frac{1}{6}$~~

for $R=3$

1.000	− 0.562
0.223	0.07
0.0182	
1.2412	.632
− 0.632	

$\varphi(3) = .6092 \cong .61$

3rd p. Break ad small sphere



$$\frac{\text{Volume}}{\text{Fläche}}\left(\text{Sphere}\right) = \frac{\frac{4\pi R^3}{3}}{4\pi R^2} = \frac{R_S}{3}$$

" $\left(\text{Cylinder}\right) = \frac{R_C}{2}$

therefore for <u>small</u> spheres or cylinders

$$\varphi(R)_{\text{cylinder}} = \frac{3}{2}\,\varphi(R)_{\text{Sphere}}$$

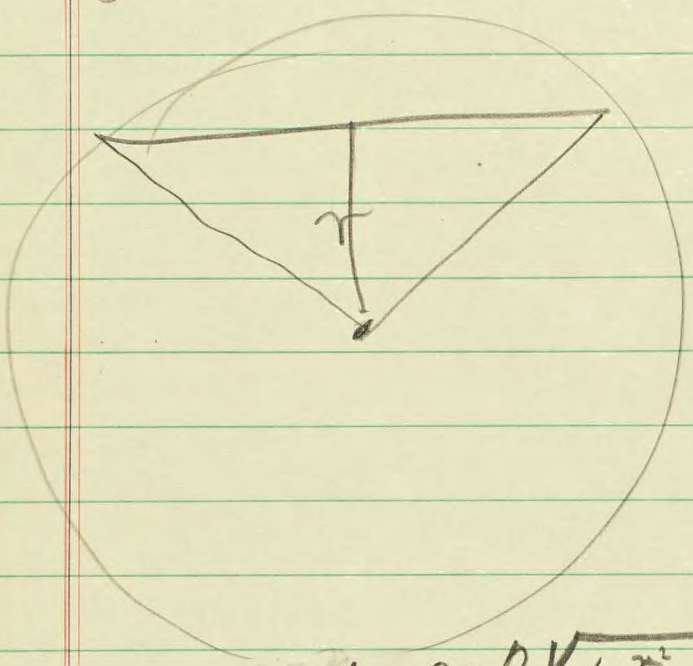
To sphere of 3 cm corresponds cylinder of 2 cm radius, for larger absorption or Radius cylinder is less favourable than $\frac{3}{2}\frac{R_{Sp}}{R_{Cy}}$

ad Mirror of Brent small house

Pepto-Bismol

$$\gamma_t = \int_0^{R-2a\sqrt{R^2-r^2}} e \; 2\pi r \, dr$$

$$1-\varphi = \frac{\int_0^{R-2a\sqrt{\;}} e \times 2\pi r \, dr}{\pi R^2}$$



$$1-\varphi = \int_0^{1} e^{-2aR\sqrt{1-\frac{r^2}{R^2}}} \, d\left(\frac{r}{R}\right)^2$$

$$1-\varphi = \int_0^{1} e^{-2aR\sqrt{1-x}} \, dx \qquad y = 1-x$$

$$= \int_0^{+1} e^{-2aR\sqrt{y}} \, dy$$

$$= \int \sum_{n/2} \frac{(-1)^n (2aR)^n}{n!} (1-x) \, dx$$

$$1-\varphi = \int_0^{1} \sum \frac{(-1)^n (2aR)^n}{n!} y^{n/2} \, dy$$

$$\frac{\Gamma(1-x)^{\frac{n}{2}+1}}{\frac{n}{2}+1}$$

$$= 1 + \sum \frac{(2aR)^n}{n!} \frac{1}{\frac{n}{2}+1} (-1)^n$$

O.K.

$R = 2$

$\varphi = 0.47$

$R = 3 \quad \varphi = 0.61$

~~$\frac{1}{1.31} \times 0.47 = \frac{1}{2.8}$~~

$\frac{1}{1+R/B}$

$R = 3$ ~~$\frac{1}{1.31} \times 0.47 = \frac{1}{2.8}$~~ $= \frac{1}{1.46} \times 0.61 = \frac{1}{2.4}$

$\frac{1}{1+\frac{R}{B}}$

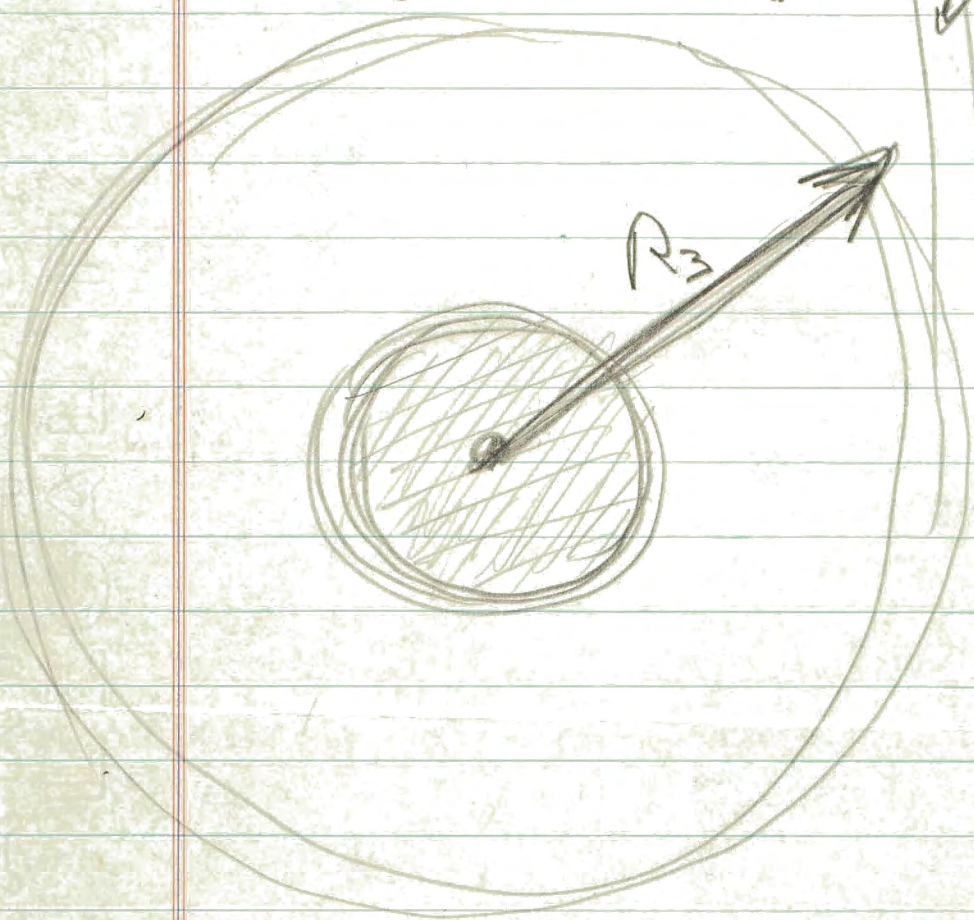
multiply these with $\left(1+\frac{R}{A}\right) =$

$A = 60$, $1+\frac{3}{60} \cong 1.05$

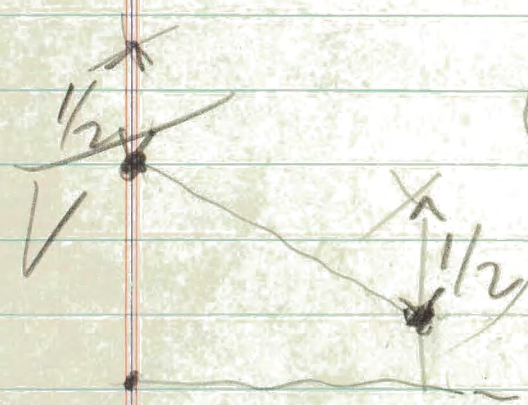
or $1+\frac{2}{60} \cong 1.03$

Cylinder

$$C = \frac{1}{\int_0^\infty 2\pi r e^{-a^2 r^2} dr} \qquad \int C e^{-a^2 x^2}$$



R_3 sei $1/2$ kurbl mes



$(n=n=0) \rightarrow \int_0^\infty x^n e^{-ax} dx = \frac{1}{a}$

$x = y^2$

$dx = 2y\,dy$

$y^{2n} 2y e^{-ay^2} dy$

$2\int y^{2n+1} e^{-ay^2} dy = \frac{1}{a}$

Brick Mesa Cylinder

$$\zeta^{res}_{cylinder} = 2\pi \frac{R}{B} \frac{K_1(R/B)}{K_0(R/B)} B^2 \not{R}$$

or putting for $R/B = 1$ $\quad f = \frac{1}{\frac{\pi}{3} - \frac{2}{\pi^2}}$

$$\zeta^{res}_{cylinder} = 2 \frac{B^2}{\frac{2}{3}\pi^2} \frac{R}{B} \frac{K_1(R/B)}{K_0(R/B)} = \frac{3B^2}{\pi^2} \frac{R}{B} \frac{K_1}{K_0}$$

for $R = B$

R/B . $\frac{K_1(1)}{K_0(1)} = \frac{6.0}{4.2}$

$\frac{K_1}{K_0}(0.4) = 1.92$

$\frac{K_1}{K_0}(0.6) = 1.67$

$1.8 \simeq \frac{K_1(0.5)}{K_0} \simeq \frac{3.59}{2} = 1.79$

$3 \times \frac{1}{16}\ 0.9 = \frac{2.7}{16} = 17\%$

for $R = \frac{B}{2}$ | $B = 6.5$ $\quad \pi^2 = (26)^2$

$\zeta^{res}_{cyl} = 3 \cdot \left(\frac{6.5}{26}\right)^2 \frac{1}{2} \cdot 1.8 =$

Cylinder (2)

$$\int_0^\infty y e^{-ay^2} dy = \frac{1}{2a}$$

$$\int 2\pi y e^{-ay^2} dy = \frac{\pi}{a}$$

$$C = \frac{a c_{4\ell}}{\pi}$$

$$a = \frac{1}{\frac{2}{3}\bar{r}^2}$$

$$\rho^* = \frac{1}{\pi} \frac{1}{\frac{2}{3}\bar{r}^2} e^{-\frac{x^2}{\frac{2}{3}\bar{r}^2}}$$

Plane case

Q B captured at

resonance

Q B captured for

infinite distance $B = 6.5$

$$\frac{B}{\sqrt{\frac{2}{3}\bar{r}^2}} \frac{1}{\sqrt{\pi}} \simeq \frac{6.5}{r_0 \sqrt{\frac{2}{3}} \sqrt{\pi}} \simeq \frac{6.5}{38}$$

$$= 0.17$$

26 cm

Answer

10 values

n	$\frac{20}{60}$	$\frac{19}{60}$	$\frac{18}{60}$	$\frac{17}{60}$	$\frac{16}{60}$	$\frac{15}{60}$	$\frac{14}{60}$	$\frac{13}{60}$	
X	1.1375	1.1308	1.1264	1.1229	1.1201	1.1172	1.1149	1.1129	
C_n	~~1.1375~~								
Equilibrium	.22253	.18597	.15903	.13648	.11738	.09642	.08008	.06466	

COPIED FROM GROUND IN THIS SQUEEZER

Abernethy

Jan. 25

$$\left\{\frac{\lambda+G}{\rho}M_x - \frac{G}{\rho}N_y\right\} \quad \left\{\frac{G}{\rho}M_x - \frac{\lambda+G}{\rho}N_y\right\}$$

$M'_x \; N' \qquad M = Ae^{-\gamma y}\cos(\omega t - ax)$

$N = Be^{-\gamma y}\sin(\omega t - ax)$

$$\left\{\gamma^2 - a^2\right\}\left\{A\frac{2G+\lambda}{\rho}a + B\frac{G-S_2}{\rho}\gamma\right\} = -\left\{A\alpha + \beta\right\}\omega^2 \qquad <\gamma$$

$$\left\{\gamma^2 - a^2\right\}\left\{-A\frac{2G+\lambda}{\rho}\gamma + B\frac{G+S_2}{\rho}a\right\} = -\left\{-A\gamma + Ba\right\}\omega^2 \qquad <\alpha$$

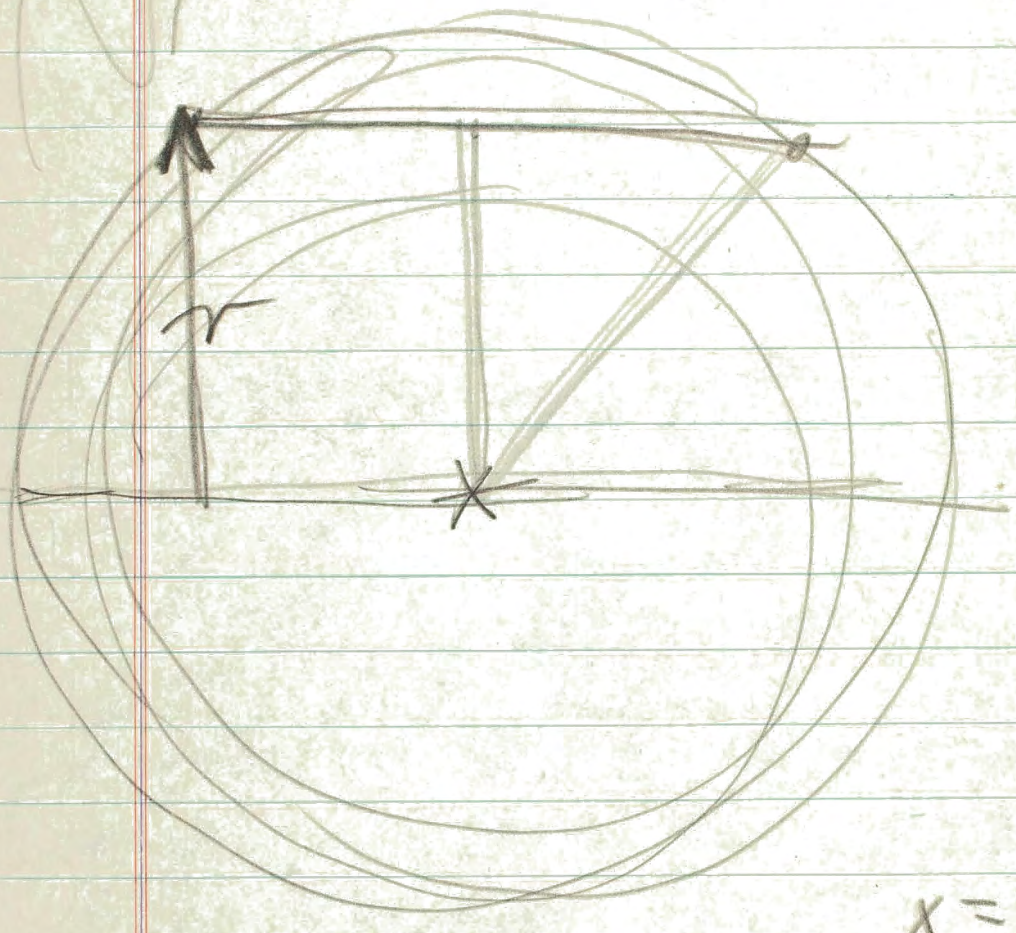
$$\left\{\gamma^2 - a^2\right\}\left\{\frac{G-S_2}{\rho}\gamma^2 + \frac{G+S_2}{\rho}a^2\right\} = -\left\{\gamma^2 - a^2\right\}\omega^2$$

$$\left\{\frac{G-S_2}{\rho}\gamma^2 + \frac{G+S_2}{\rho}a^2\right\} = -\omega^2$$

dec

Small sphere

(1)



Placed
Wapus
Dellen

$$x = 2\sqrt{R^2 - r^2}$$

$$\text{Abs} = \int_0^{r=R} \left(1 - e^{-\frac{ax}{R}}\right) 2\pi r\, dr \quad 2\sqrt{R^2 - r^2}$$

$$\text{Durchgelt} = \int_0^{} e^{-a\,2\sqrt{R^2 - r^2}} \times 2\pi r\, dr$$

Einheitskugel

Small sphere ②

$$\int_0^R 2\pi \left[r - 2a\sqrt{\cdot}\, r + \frac{4a^2}{2}(R^2 - r^2)\, r - \frac{1}{8!} \right] dr$$

$$2\pi R^2 - a\,\text{Volume} + \frac{4\pi a^2}{6} R^4$$

$$a \cdot \text{Vol} \times \frac{aR}{2}$$

$$\frac{d}{dr}\left[\frac{R^2 r^2}{2} - \frac{r^4}{4} \right] = \frac{1}{4} R^4$$

Einheitskugel

$$2\pi R^2 - \left(1 - \frac{aR}{2}\right) a\,\text{Volume}$$

Absorbent mixed:

$$\left(1 - \frac{aR}{2}\right) a\,\text{Volume}$$

$$a = \frac{1}{\lambda(4)} \frac{\sigma_a(4)}{\sigma_{sc}(4)}$$

a is Absorptionswahrscheinlichkeit

Sunsphere (4)

$R = 4$

$\alpha = \frac{x}{4}$

$\varphi = \left(1 - \frac{1}{2}\right)\frac{4}{3}$ $\qquad \frac{1}{1+R/B}$

$\frac{2}{3}\left(\frac{1}{1+\frac{4}{6.5}}\right) = \frac{1}{1.62} \times \frac{2}{3}$

$\varphi \frac{1}{1+R/B} = \frac{1}{2.4}$

$R = B$

$\varphi = \left(1 - \frac{1}{2}\frac{3}{4}\right) = \frac{4}{3}\frac{3}{4} = \frac{5}{8}$

$\frac{1}{1+\frac{3}{6.5}} = \frac{1}{1.46} =$

$\varphi \frac{1}{1+\frac{R}{B}} = \frac{1}{2.35}$

die Achse

$\frac{18}{\lambda(u)} = 2$

$R = 2$ cm

$\varphi = \left(1 - \frac{1}{4}\right)\frac{2}{3} = \frac{1}{2}$

$\frac{1}{1+\frac{2}{6.5}} = \frac{1}{1.31} =$

$\varphi \frac{1}{1+\frac{R}{B}} = \frac{1}{2.62}$

Al; paper on Be

$$\xi = \left(\frac{53}{6.5}\right)^2 \frac{1}{1+5/6.5} \frac{1}{\frac{2.44}{5\sqrt{\frac{3}{2}}\ 7 - N14} + \frac{1}{1+5/53}}$$

$$\frac{1}{1.78} \qquad \frac{1}{1.54}$$

$$\frac{1}{\lambda} = \frac{16 \times 6 \times 10^{+23} \quad 11 \times 10^{-24}}{238} =$$

$$= \frac{16 \times 6 \times 1.1}{238} =$$

$$= 2.25 \text{ cm}$$

$$\frac{6.1}{2.25} \qquad \frac{1}{0.63 + 0.91}$$

$$\frac{}{3.85} = \frac{}{1.54}$$

$$\frac{1}{1.78} \quad \frac{1}{1.54} = \frac{1}{2.75}$$

$$\left(\frac{53}{6.5}\right) = 66.5 \qquad \frac{66.5}{2.75} =$$

$$\xi \cong 24$$

1000

334

666

$$\frac{0.334}{6} \quad \frac{5^2}{(6.5)^2} \quad \frac{1}{(1+R/13)} - \frac{1}{1.78}$$

$$\frac{1}{18} \times 0.8 \times \frac{1}{1.78}$$

$$\frac{1}{} \quad 0.033 \times 0.8 =$$

Check from (33)

$$\frac{V}{\frac{4\pi}{3}R^3} = 3\varphi \times \frac{0.334}{1.33}\left(\frac{53}{50}\right)^2 1.1$$

$$\varphi = \frac{\text{Wt} \pm \sqrt{\frac{300}{0.32}\,\text{Wt} - \frac{4(M)}{R}}}{\frac{N(1)}{R}\left(1+\frac{\gamma}{A}\right) +}$$

$$\varphi \cong \frac{1}{1.54} \cong 0.6$$

1.55

2 1.1

$$\frac{(53)}{50}^2 \times 1.1 \times 0.6 \times 0.75 \times 0.6$$

Johnson

use of for $\sigma_d(4) \approx$

$\sigma_{sd}(4)$

$\lambda(4)$ can be

improved in Formula

(20)

+ 0 0 0

3 3 4

6.6 6

0. 334

6

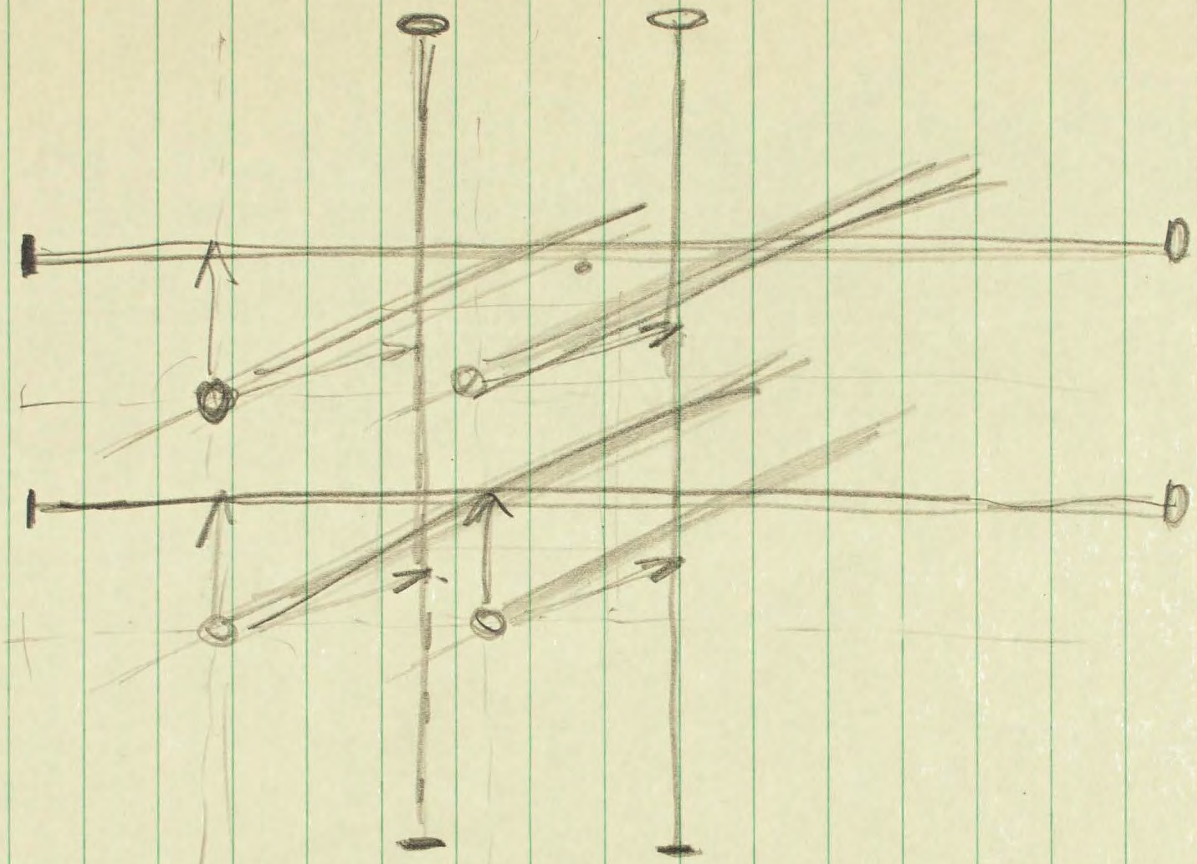
0.666 ×2

1.332

Also must observe

treated with pure

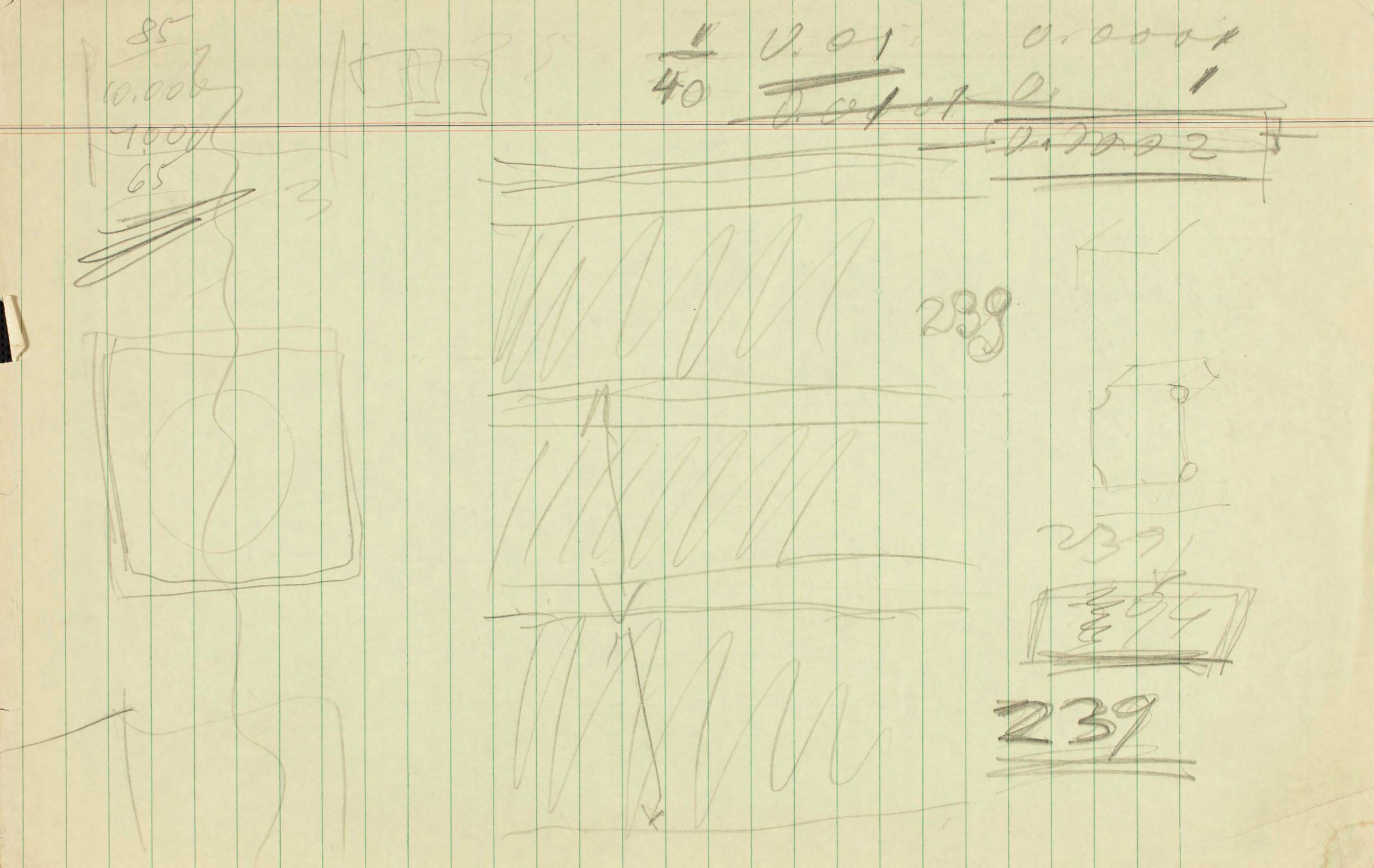
assumption



Cubic symmetry
4 cylinder

Brent

Marco


239
239

Small sphere (3)

$\lambda = 2$ cm Quelques

$a = \frac{1}{4}$ $R = 4$

$\left(1 - \frac{aR}{2}\right) \times Vol = \frac{1}{2} \times Volume$

$\frac{1}{2}$ Volume $\frac{1}{\lambda(4)} \frac{\sigma_a(4)}{\sigma_{sc}(4)}$

$\left(1 - \frac{aR}{2}\right) a\,Vol$ $\lambda(4) = 2$ cm

mais $a = \frac{1}{\lambda(4)} \frac{\sigma_a(4)}{\sigma_{sc}(4)} \simeq \frac{1}{4}$

$\varphi = \frac{\left(1 - \frac{aR}{2}\right) a\,Vol}{\pi R^2} = \left(1 - \frac{aR}{2}\right) \times \frac{4R^3}{3} \frac{1}{\lambda(4)} \frac{\sigma_a}{\sigma_{sc}}$

$= \left(1 - \frac{aR}{2}\right) \times \frac{4}{3} \frac{R}{\lambda(4)} \left(\frac{\sigma_a}{\sigma_{sc}}\right)^{1/2}$

$\varphi = \left(1 - \frac{1}{2} \frac{R}{\lambda(4)} \frac{\sigma_a}{\sigma_{sc}}\right) \frac{4}{3} \frac{R}{\lambda(4)} \left(\frac{\sigma_a}{\sigma_{sc}}\right)^{1/2}$

Ok Nucleo Inert Lec 27

$$V_{res} = \frac{4\pi}{3} Q^* R B^2 \left(1 + \frac{R}{B}\right)$$

$$= \frac{1 - 4\pi}{2} QV$$

$$\frac{V}{\frac{4\pi}{3} R^3} = \frac{6}{1 - 4\pi} \frac{B^2}{R^2} \left(1 + \frac{R}{B}\right) \frac{Q^*}{Q}$$

Let us put $Q = \frac{1}{V}$

$$\frac{1}{\frac{4\pi}{3} R^3} = \frac{6}{1 - 4\pi} \frac{B^2}{R^2} \left(1 + \frac{R}{B}\right) Q^*$$

$$(1 - 4\pi) = \frac{6 \times 4\pi}{3} R B^2 \left(1 + \frac{R}{B}\right) Q^*$$

NAVAL ORDNANCE LABORATORY
NAVY YARD, WASHINGTON, D.C.

December 29, 1940

Professor Leo Szilard
Department of Physics
Columbia University
New York City

Dear Szilard:

I am very glad that you are willing to participate in the conference. The plan is to have it Monday, January 6th, from 9:30 to 5 P.M. at the Carnegie Institution Building at 16th and P Sts.,N.W., Washington, D.C. I should suggest coming to Washington on an afternoon train on Sunday, January 5th and stopping overnight at the Mayflower Hotel where rooms will be reserved for this purpose. Another possibility is to use a night train and a sleeper.

If this arrangement does not suit you, please let me know by wire at once.

Sincerely yours,

Gregory Breit

G. Breit

CORRECTION

The hotel mentioned in my letter of December 29th should have been WARDMAN PARK instead of Mayflower

G. Breit

420 West 116th Street
New York, N. Y.
December 30, 1940

Professor G. Breit
Naval Ordnance Laboratory
Navy Yard
Washington, D. C.

Dear Breit:

Many thanks for your letter of December 29. After having talked the matter over with Fermi, I sent you today the following telegram:

> "Fermi and I would prefer meeting January 9 to 12, that is, Thursday, Friday, Saturday or Sunday of next week if convenient to you"

One of the reasons why we would prefer to meet on one of the dates indicated, rather than on January 6, is the following: Fermi has to be in New York on Tuesday and we want to make use of the opportunity and go through some other business while we are in Washington, either the day preceding the meeting or the day following the meeting. We also feel that it would be a good thing to reach a concensus of opinion on a number of questions during the projected meeting, and this is an additional reason for choosing a date which would make it possible to continue the conversations if necessary, at least with some of those who attended the conference, for another day or two. However, if you should have some very compelling reasons for retaining the date of January 6, I suppose matters could be arranged so as to make it possible to meet on that date.

I hope, though, that the proposed change of date will not cause you inconvenience.

Sincerely yours,

(Leo Szilard)

TELEPHONE
UNIVERSITY 4-2700

KING'S CROWN HOTEL

OPPOSITE
COLUMBIA UNIVERSITY

UNDER KNOTT MANAGEMENT

420 WEST 116TH STREET, NEW YORK N.Y.

January 2, 1941

Professor G. Breit
Naval Ordinance Laboratory
Navy Yard
Washington, D.C.

Dear Breit;

After we talked over the telephone I sent you a telegram to let you know that January 13th would be convenient to us. I am writing this letter in order to confirm that message.

Fermi has to be back in New York on the 14th so the possibility of a two day conference which we have tentatively discussed does not arise.

I intend to get to Washington Sunday afternoon (January 12), and if you should happen to be free in the afternoon or evening it would be nice to see you unofficially.

Sorry that you had to go to the trouble to telephone. The next time I shall send you a more complete list of the days on which we are free so that you can pick a suitable day.

Sincerely yours,

Leo Szilard

NAVAL ORDNANCE LABORATORY
NAVY YARD, WASHINGTON, D.C.

January 5, 1941

Professor Leo Szilard
Department of Physics
Columbia University
New York City

Dear Szilard:

Many thanks for your letter. I am glad that it turned out to be possible to arrange a date for the conference which is at least approximately satisfactory to all concerned. I am sorry not to have realized that the suggestion for Thursday was more than just a preference. One of the men attending the conference had an engagement for Thursday, Friday and Staurday of the coming week. The dates were also inconvenient for me.

I have an engagement for most of the day for Sunday, January 12th with Wigner and Teller. It would be nice if we could all have dinner together Sunday night. We live at 49 Eye St.,N.W., ← } (I)
Apartment 502 and Wigner and Teller will be at our apartment. The place is very close to Union Station. I should suggest that you come here rather than try to meet at the Wardman because at the Wardman the group will be increased to a larger number. Our home telephone is REpublic 3169. If you wish to talk privately, Wigner and Teller would doubtless excuse us either for dinner or right after it. I will have to report at the Navy Yard before 8:00 A.M. Monday morning before the conference and will have to be in bed by 10:30.

With best wishes and regards to Fermi,

Sincerely yours,

Gregory Breit

G. Breit

January 23, 1941

Professor G. Breit
Washington, D. C.

Dear Breit:

I was very glad to hear that you intend to come here on Monday next. I believe it is perfectly convenient to all those concerned.

It so happens that Monday is the one day in the week which D. P. Mitchell spends in New York, and, therefore, part of the time is usually spent in conferences concerning purchases, desired appropriations and some such matters. These are quite important things for us just now and perhaps you too would find some of the questions which are arising quite interesting. It would be desirable, however, to arrange matters so that you should be free to stay over on Tuesday and possibly Wednesday, in case you should wish to continue any discussions which may be started on Monday. Perhaps we could get Wigner to come up from Princeton on Tuesday or Wednesday, if this seems desirable.

With kind regards,

Yours

L. Szilard

LZ/eh

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS
Room 117, East Building

IN YOUR REPLY
REFER TO FILE

March 28, 1941

Confidential

Professor L. Szilard
Department of Physics
Columbia University
New York City

Dear Szilard:

I have shown your letter to Dr. Briggs who has authorized me to tell you that an order for the graphite for the intermediate experiment has been placed with the U.S.Graphite Comapny, Saginaw, Michigan. I understand that Dean Pegram is conversant with the situation.

It is a good plan to write to me about the uranium matters only in a confidential envelope sealed and enclosed in an ordinary envelope. This is especially advisable at the Bureau because occasionally my mail may be opened by mistake by a mail clerk.

I am sorry to learn that you have not been well and I hope that you are recovering rapidly. When you are in Washington, please look me up.

Sincerely yours,

Gregory Breit

G. Breit

WOodley 1720
Extension 282

P. S. Since writing this letter it became necessary for me to be at Princeton Tuesday and I shall try to go to New York on Wednesday.

CONFIDENTIAL

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

GB:DEK July 16, 1941 D

Professor L. Szilard,
Department of Physics,
Columbia University,
New York, N. Y.

Dear Szilard:

The provisional plan is to have a meeting of the Subcommittee on Theoretical Aspects on August 8th and 9th. I am very anxious to hear you talk about your paper and I should like to devote the afternoon of the first day to its presentation and discussion.

I am asking Fermi to give a general introduction on the morning of the first day and to continue his talk on the morning of the second day. I hope that the two of you can arrange to avoid too much overlapping. A moderate amount of overlapping will do no harm of course and will be useful. There will perhaps be other talks as well and there will be a business session on the afternoon of the second day.

I mentioned to Dr. Briggs that you are preparing a stencil of your paper. He sees no objection to the arrangement we discussed. On mentioning to him that you could turn the stencil in to him he did not feel like asking you to do so. I think, however, that it would be a good idea to send the stencil to him because the ultimate responsibility for the handling of confidential matter rests on him. Your paper will be circulated in the group and since it will be mimeographed others may wish to mimeograph their papers as well. The temptation of running off a dozen or so of extra copies and distributing them among friends will be too large for the average physicist and on purely statistical grounds you could be sure of a large number of leaks. I am very anxious, therefore, to have a general policy against mimeographing unless the stencil is used only at the Bureau of Standards. A special marking system can then be devised also to identify official copies and make the production of unauthorized copies detectable. I feel

that in your case, on account of your special connection with the work, an exception could be made regarding the place where the mimeographing is done but it is probably better not to have such an exception in order to have a general policy.

Sincerely yours,

Gregory Breit

Gregory Breit.

CONFIDENTIAL

July 22, 1941

Dr. Gregory Breit
National Bureau of Standards
Washington, D. C.

Dear Breit:

Many thanks for your kind letter of July 16. If you will let me know how many copies you want of my paper of February 1940, I shall send them to you as soon as they are available.

This paper contains essentially calculations made in July 1939 and no nuclear value obtained after we started measurements of carbon are included in the paper. If you think that we should adopt the historical presentation and give a general picture of how our ideas gradually developed, then I shall be very glad to talk about this paper on the afternoon of August 8. There would be little point in doing so, however, if the present state of the theory were discussed on the morning of August 8. I understand from Fermi that he intends to report in the morning of August 8 on various measurements which were made at Columbia on carbon and uranium, and accordingly it seems that there will be no overlapping.

I am wondering somewhat whether the early part of September would not really be a better time for this meeting than the early part of August, since many of the proposed participants of the meeting may have planned short vacations for the early part of August. However, you may have some important reasons for having the meeting earlier of which I am not aware.

With kind regards,

Yours sincerely,

L. S.

(Leo Szilard)

LS:H

cc: Professor Pegram
Professor Fermi

CONFIDENTIAL

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

GB:KSV July 28, 1941. D

Dr. L. Szilard,
Department of Physics,
Columbia University,
New York, N. Y.

Dear Szilard:

I am very happy that you are willing to give a report on your paper. The plan of arranging the subject matter which you and Fermi have worked out seems to me to be a very good one.

The time of the meeting is still uncertain. Some wished to have the meeting immediately, and some want it later. I would advise having the talk ready by August 8.

I have written to Dean Pegram asking his opinion regarding what should be done with the stencil.

Sincerely yours,

Gregory Breit

Gregory Breit.

July 30, 1941

Dr. Gregory Breit
National Bureau of Standards
Washington, D. C.

Dear Breit:

I have now a photostat copy made of the paper which was sent to the PHYSICAL REVIEW on February 14, 1940, and am enclosing it for your personal use. This photostat is a positive made from the negative which was made from the manuscript before it was sent. You will find two pages marked "21". The one which is marked "21 repl" was written to replace the original page 21 after the manuscript was sent off, and this replacement page bears the postmark of February 21, 1940. The same holds for the summary.

Please note that I am sending you this photostat copy for your personal use only. It seems to me desirable that you should have such an original document at your disposal.

At the request of Tate I have attempted to shorten this paper and the mimeographed copies which I am going to send you represent such a shortened version. Otherwise there is no change either in the content or in the emphasis, as you will see if you compare it with the original. The somewhat long introduction will be dropped at the suggestion of Tate. On the other hand, I am anxious to include a rather full statement of the background of the paper, i.e. a full discussion of all unpublished information which was available at the time the paper was written, so that if credit should be due to others the reader should be in the position to pass judgment on this point for himself.

I am quite anxious that no version of the paper should be circulated which does not have attached to it a full statement of the background. It seems to me that since the normal channels of publication are blocked, we

should be even more careful than ordinarily to give credit for all unpublished statements which might possibly have contributed to or accelerated the development of our own ideas.

Yours sincerely,

LS:H (Leo Szilard)

cc: 1 - Pegram
1 - Fermi
1 - Szilard
2 - Mitchell

July 30, 1941

Dr. Gregory Breit
National Bureau of Standards
Washington, D. C.

Dear Breit:

I have now a photostat copy made of the paper which was sent to the PHYSICAL REVIEW on February 14, 1940, and am enclosing it for your personal use. This photostat is a positive made from the negative which was made from the manuscript before it was sent. You will find two pages marked "21". The one which is marked "21 repl" was written to replace the original page 21 after the manuscript was sent off, and this replacement page bears the postmark of February 21, 1940. The same holds for the summary.

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should be even more careful than ordinarily to give credit for all unpublished statements which might possibly have contributed to or accelerated the development of our own ideas.

Yours sincerely,

LS:H (Leo Szilard)

cc: 1 - Pegram
1 - Fermi
1 - Szilard
2 - Mitchell

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SYMBOLS
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WA 980 37 GOVT NT=WASHINGTON DC 12

1941 AUG 12 PM 5 53

DR L SZILARD= QR

DEPT OF PHYSICS COLUMBIA UNIVERSITY NYK=

CONFERENCE ON THEORETICAL ASPECTS PLANNED FOR AUGUST 22 AND 23 NATIONAL BUREAU OF STANDARDS PLEASE INFORM WHETHER YOU CAN ATTEND AND WHETHER YOU WISH YOUR TRANSPORTATION REQUESTS FROM PRESENT ADDRESS TO WASHINGTON AND RETURN TO SAME ADDRESS=

GREGORY BREIT.

22 23.

THE COMPANY WILL APPRECIATE SUGGESTIONS FROM ITS PATRONS CONCERNING ITS SERVICE

CONFIDENTIAL

MRD

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

GB:KSV August 2, 1941. D

On account of unforeseen delays connected with reorganization it has proved advisable to postpone the meeting of the Subcommittee on Theoretical Aspects by one week from August 8th and 9th to August 15th and 16th.

General instructions have been issued to me to arrange for attendance only by those who have satisfied official clearance requirements. This is part of the reason for the delay.

Some changes in personnel of the Subcommittee have been made in connection with the present reorganization. The present letter is, therefore, not a definite notice of the meeting and of its attendance.

A notice of the meeting will be sent out at a later time.

Sincerely,

Gregory Breit

Gregory Breit.

Copies to:
Messrs. Condon, Eckart, Fermi, Szilard, Teller, Wheeler, Wigner, Beams, Pegram, Allison, Smyth, and Lawrence.

1527

GB:DEK

NATIONAL BUREAU OF STANDARDS

1320602 Operation and Administration,
National Bureau of Standards, 1942

NIGHT LETTER

August 12, 1941

Dr. L. Szilard,
Department of Physics,
Columbia University,
New York, N. Y.

Conference on Theoretical Aspects planned for August twenty-second and twenty-third National Bureau of Standards. Please inform whether you can attend and whether you wish your transportation requests from present address to Washington and return to same address.

Gregory Breit.

CONFIRMATION COPY

THIS MESSAGE WAS TELEPHONED,
AT THE TIME SHOWN ON THE
REVERSE HEREOF, AS CHECKED.

.... Western Union ..... Postal
..... Navy Dept. ..... War Dept.
.....Radio Corp. of America

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

GB:DEK August 19, 1941 D

Messrs. S. K. Allison, E. U. Condon, G. B. Pegram, H. C. Urey, J. W. Beams, C. Eckart, J. A. Wheeler, H. D. Smyth, L. Szilard.

The conference on theoretical aspects will begin at 9:30 o'clock Friday morning, Room 300 South Building.

G. Breit

Gregory Breit.

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

GB:DEK

August 20, 1941

D

Dr. L. Szilard,
Department of Physics,
Columbia University,
New York, N. Y.

Dear Szilard:

I wish to acknowledge receipt of a copy of your report to Dr. Briggs enclosed with your letter of August 19th.

Sincerely yours,

Gregory Breit

Gregory Breit.

October 4, 1941

Professor G. Breit
National Bureau of Standards
Washington, D. C.

Dear Breit:

Enclosed I am sending you two copies of a memorandum which I have submitted to Professor G. B. Pegram. It will give you information of the plans which I am pursuing in connection with the uranium oxide purification. I shall probably be in Port Hope some time next week, and after that I shall know better just how great the technical difficulties are which will have to be overcome if a fluoride precipitation is to be carried out on an industrial scale.

Yours sincerely,

(Leo Szilard)

LS:MEB

Enclosure

Note ref. to

Report A-45

bbs-39

Final Report

October 17, 1941

Dr. Gregory Breit
National Bureau of Standards
Washington, D. C.

Dear Breit:

Enclosed you will find an appendix to my memorandum of September 26th.

Please note that that memorandum was written for the purpose of keeping the members of our project informed, and that there was no intention of having it recorded as a Uranium Section report. I wonder whether you wouldn't think it better if this report were withdrawn. However, if, for any reason, this should not be feasible, then I would appreciate it if the enclosed appendix to the memorandum of September 26th were attached to Report A-45.

Sincerely yours,

L. S.

(Leo Szilard)

LS:MEB

Enclosure

APPENDIX TO MEMORANDUM OF SEPTEMBER 26, 1941

In writing the above memorandum I assumed that element 94 has a long lifetime. This assumption was based on the published statement that this element has no appreciable alpha activity. Abelson and MacMillan reported (Physical Review, 1940) that if 94 is an alpha emitter its lifetime will exceed one million years.

After writing the memorandum of September 26th I learned that, according to some unpublished work of Segre and Seaborg, and contrary to Abelson and MacMillan's publication, there is an observable emission of alpha rays from 239-94, and that the half lifetime of this element is about 30,000 years. If this is correct, the chance of finding element 239-94 in pitchblende is considerably smaller than it would appear from my memorandum of September 26, 1941. In the light of this additional information the memorandum of September 26th appears to be rather misleading in its emphasis. Whether a search for longer lived transuranic elements should be undertaken at this time is a question of a rather different character.

L. S.

(Leo Szilard)

October 17, 1941

CONFIDENTIAL

NPD

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

GB:KSV December 2, 1941. D

Dr. L. Szilard,
Department of Physics,
Columbia University,
New York, N. Y.

Dear Szilard:

In your letter of December 1, you suggest that I date your report A-55. According to the Physical Review records, your paper has been submitted on February 16, 1940, which corresponds to your date of February 14, 1940, if account is taken of the time it takes for a manuscript to reach Minneapolis from New York. Report A-55 does not appear to be identical with the manuscript submitted to the Physical Review. The summary in A-55 is longer and speaks of 30 tons of uranium instead of 10 tons in the Phys. Rev. paper. Also p.21 of A-55 does not correspond to the analogous page in the Phys. Rev. manuscript.

In view of these differences I wonder whether it would be satisfactory to you if I were to send out the following statement to be attached to the report:

> "Report A-55 is substantially the same and in most parts identical with a paper submitted for publication in the Physical Review on February 16, 1940, ~~and~~ which was generously withheld from publication at the request of the author in the interests of national defense."

Sincerely yours,

Gregory Breit

Gregory Breit.

December 5, 1941

Dr. Gregory Breit
National Bureau of Standards
Washington, D. C.

Dear Breit:

Many thanks for your letter of December 2. I am sorry to take your time with this correspondence. Do you think it would be possible to make the statement which you propose to attach to the report more precise, for instance by saying the following:

> "Report A-55 is, with the exception of the summary and page 21, a copy of a paper submitted for publication in the PHYSICAL REVIEW on February 16, 1940. The paper has been withheld from publication at the request of the author in the interests of national defense."

If you should find it convenient to do so, you might attach to this statement a copy of the original summary and a copy of the original page 21, but I personally have no opinion as to whether this is desirable or not.

Many thanks again.

Sincerely yours,

LS:H

Leo Szilard

CONFIDENTIAL

COPY

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

GB:KSV July 16, 1941. D

Professor H. C. Urey,
Department of Chemistry,
Columbia University,
New York, N. Y.

Dear Harold:

I was much interested to receive your letter of July 11 regarding the operation of the Reference Committee. The Subcommittee on Theoretical Aspects can be made use of to meet the points brought out by Wigner and seconded by Condon. Dr. Briggs tells me that during the next year the restrictions need not be quite so severe on account of a difference in the source of funds. The Subcommittee on Theoretical Aspects includes at present Messrs. Condon, Eckart, Fermi, Szilard, Teller, Wheeler, and Wigner. Messrs. Smyth and Turner are being invited to join the group in the immediate future. Also Messrs. Goldhaber and Placzek may be called on to attend meetings as "special advisers".

Dr. Briggs authorized me to invite any member of the Uranium Committee to the meetings of the Subcommittee. It is, therefore, planned to have notices of the Subcommittee meetings sent to Messrs. Beams, Pegram, and Urey, and it is hoped that they will attend the meetings.

It is planned to circulate among members of the Subcommittee some confidential reports as well as papers that have been withheld from publications.

It is recommended that the process of multiplying (reproducing) manuscript copies be centralized at the Bureau of Standards. This is very desirable because Dr. Briggs, as Chairman of the Uranium Committee, has the ultimate responsibility for the observance of secrecy regulations for this work. It would hardly be fair to ask him to be responsible for details of handling confidential material in bulk in a dozen places throughout the country.

There will be no trouble in multiplying copies of non-mathematical manuscripts. For mathematical manuscripts it is suggested that one of the following procedures be followed.

(a) Supply your report in duplicate: Original + carbon. Have the carbon copy made on a good grade of thin paper. The more transparent and uniform the thin paper the better will be the results. Have the thin paper backed by a sheet of black typewriter carbon paper so as to have a black impression of type on both sides of the thin paper.

Fill in the formulas in the carbon copy with soft black pencil preferably backing the copy with carbon paper so as to have a black impression of the mathematical symbols on the face and on the back of the copy.

The carbon copy so prepared can be readily reproduced by a kind of blueprinting process. Blue impressions cannot be reproduced by this process. Black or green ink will reproduce satisfactorily but blue ink will not show.

(b) Have the original manuscript (heavy paper) in shape for photostating. Black ink is preferred to blue ink. Blue ink requires the use of a blue filter.

Process (a) gives somewhat less bulky results than process (b) and is, therefore, recommended.

It is intended to have the first meeting of the Subcommittee at the National Bureau of Standards in Washington on August 8th and 9th. Arrangements for a program of talks are being made. Time will be allowed for discussion of ways in which the Subcommittee can be useful in the general program. The present plan is to have one or two-day meetings at intervals of about 6 weeks. I should appreciate greatly receiving any ideas or suggestions regarding the conduct of the meetings either by letter or verbally during the conference.

Sincerely yours,

Gregory Breit.

Copies to:
Messrs. Condon, Eckart, Fermi, Teller, Wheeler, Wigner, Beams, Pegram, Allison, Smyth, Turner, Lawrence, and Briggs.

SLOANE PHYSICS LABORATORY
YALE UNIVERSITY
NEW HAVEN 11, CONNECTICUT
2014 Yale Station

April 7, 1960

Professor Leo Szilard
Memorial Center of Cancer & Allied Diseases
444 East 68th Street
New York, New York

Dear Szilard:

It is with great pleasure that I have learned through the New York Times that the Atoms for Peace Award will be made to you. Allow me to express my sincere good wishes and heartiest congratulations in this connection.

I do hope that you will recover from your present illness rapidly.

With best wishes,

Sincerely yours,

Gregory Breit

Gregory Breit

gb:y

abstain from participation

PHYSICS DEPARTMENT
YALE UNIVERSITY

Sloane Laboratory
217 Prospect Street
New Haven 11, Connecticut
2014 Yale Station

nuclear

May 19, 1961

Dr. Leo Szilard
Hotel dupont Plaza
1500 New Hampshire Avenue, N.W.
Washington 6, D.C.

Dear Szilard:

I was glad to receive your memorandum of May 10 with the enclosed copy of the petition to the President of the United States and the enclosed material. It is especially nice to learn that you are well enough to be so active and I sincerely hope that your health will continue to improve. As to the petition, I do not feel myself to be competent to express an opinion on the actions of the President especially because the executive branch of the Government may have information with which I am unfamiliar and which it may be harmful to divulge.

At all events it will be surprising if the USSR are not planning to use Cuba as a military base. To what extent action should be contingent on agreements with the United Nations should depend, I believe, on the degree to which the situation can be classified as an emergency and the political complications in the meetings of the United Nations. It is conceivable that tying the hands of the Administration may have disasterous consequences because our opponents interpret their obligations to the United Nations in a very loose manner. In view of all of this I do not find it possible to sign the petition.

With best wishes,

Sincerely,

Gregory Breit

Gregory Breit

gb:y