July 21, 1943

Dear Dr. Dempster:

I have read your analysis of report A-55 dated July 9, 1943
and you and I have since that time gone jointly over report A-55. In the
following I am giving you a resume of the statements I made to you in
connection with report A-55:

1. A-55 gives a recipe which permits to determine for a lattice of
uranium spheres in graphite for what radius R of uranium spheres and for
what ratios of uranium to carbon for any given R the multiplication fac-
tor can be expected to be close to its highest possible value. By using
the formulae and the constants given in the paper one finds for room
temperature and density of 15 grams per cc for the best radius R, a value
slightly above 5 cm. Similarly, for 900°C one finds for the best radius
R, a value of about 8 cm. All examples given in the paper refer either
at 5 cm spheres at room temperatures or to 8 cm spheres at 900°C.

The corresponding ratios or uranium and carbon can be easily
calculated from the formulae given in A-55. No ratio of uranium to carbon
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is explicitly given for 8 cm spheres/but this could be obtained simply
by putting the set of values given on page 20 in A-55 into the formulae
given in the paper. For 5 cm spheres at room temperature a ratio of
uranium weight to graphite weight of 3 to 10 is given in A-55.

It is my contention that for all the R values, and all weight
ratios recommended in A-55, the multiplication factor is larger than 1
and that A-55 teaches how to obtain a lattice which is capable of sustain-
ing a chain reaction.

The theory given in the paper for the determination of the most favorable sizes and mixtures is an approximation which holds only for small uranium spheres, say spheres below 8 cm radius. The theory neglects various factors which counteract each other and thereby leads to very simple formulae.

a) The theory proven in A-55 permits to calculate for any given Radius R between 5 and 8 cm the most favorable ratio of uranium to carbon in the following manner:

The value of ε is calculated from R, the radius of the uranium spheres by means of equation (20a). The value of all physical constants occuring in equation (20a) are given in the paper. Having obtained the value of ε the value of gm can be calculated either from equation (26) or from equation (27). Having obtained gm the most favorable ratio of uranium to carbon corresponding to the chosen value of R can be obtained from equation (33a).

An alternative method which is mathematically identical gives the uranium-carbon ratio of calculating in addition to ε and gm also ψ from equation (19) and then substituting the values of ε, ψ, and gm into equation (33).

If the uranium graphite ratio has been calculated we may obtain the distance between neighboring uranium spheres in a cubic close-packed lattice from equation (34) or more directly from equation (35).

(Please note that the distance of 51 cm given on p.12 is not the distance corresponding to a physically real case, but is a maximum distance which can be gotten from equation (35) by means of a set of values

which are most pessimistically chosen, among all the values occurring on p. 20

for the purpose of demonstrating that there is no danger of this distance

becoming larger compared to the distance $\sqrt{\overline{r^2}}$ which is about 50 cm. See in

this connection the first paragraph on p.11.)

b) In order to get the most favorable multiplication factor we have

according to A-55 to choose R so as to make ε a maximum. This is obvious from

equation (26) which shows that gm increases with increasing ε. Accordingly

in order to make the multiplication factor a maximum we have to choose R so as

to make ε as given by experession (20a) a maximum. As stated before, the

maximum is at room temperature slightly above R=5 cm for uranium density of

15 gms per cc. If the temperature is increased to 900°C, the absorption and

capture cross sections are reduced by a factor 2 and the maximum for R shifts

by a factor of $\sqrt{2}$ into the neighborhood of R=8 cm.

2. A-55 as so far discussed in the present memorandum deals only with

the question of how to obtain the optimum size for the uranium spheres and the

optimum ratios of uranium to carbon. It is, however, also of interest to

know whether for the optimum case, the multiplication factor will be larger

than 1. In this respect, A-55 most emphatically asserts that this will be

the case and gives on p.21 for room temperature a rough guess of $(\mu q-1)$

being 1/8 which corresponds to a multiplication factor of μq = 1.125. This

corresponds to $gm/$(value given on p.21$^{.6}$) for spheres of 5 cm radius at room

temperature) and a value of μ= 1.87, which is in accordance with the value

of μ given on p.17 if we take into account the explicitly stated experimental

error given on p.17 for p.

It should be noted that only the product μq enters into the

determination of the critical radius μ and that it is indifferent from the

point of view of the chain reaction in what way this product is split into

the two factor

the two factors μ and q.

Similarly, Joliot's experiment which is discussed in the paper does not directly measure μ but measures a combination of constants something close to μ x (1-p). p is connected with the resonance absorption of uranium, and A-55 emphasizes that its value is very badly known. Although it is, therefore, somewhat uncertain in what way this product has to be split into μ and 1-p this uncertainty does not affect the results of Joliot's experiments inasmuch a it shows that uranium-water mixtures may have a multiplication factor which is, rather close to 1.

At the time when the paper was written I knew from experiments made on heterogenous and homogenous systems of uranium and water that such systems have a multiplication factor which in the most favorable cases comes xxxxxx rather close to 1 but most xxx probably stays below 1. If one now replaces as a slowing agent, one hydrogen atom with 25 to 30 carbon atoms one should expect a system in which the loss of neutrons due to uranium resonance is about equal to loss of resonance in the hydrogen system. Since the absorption of 25 to 30 carbon atoms of a cross section of .005 each is only about .13 to .15 whereas the thermal absorption cross section of a hydrogen atom is about .3. So it may be seen that the uranium-graphite system is more favorable and can be expected to have a multiplication factor above 1, but cannot be expected to have a multiplication factor very much above 1 unless the carbon cross section should be considerably lower than .005.#

#(Please note that if the carbon absorption cross section were .01, then the above consideration would show that carbon and hydrogen are just about equal. I stated this fact in my letter to Fermi, July 3, 1939 and my statement was based on the consideration given above. Even so, carbon would have an advantage over hydrogen is used in a Heterogeneous system, for reasons which were given in my

At the time when A-55 was written the best value for μ appeared to be about 2. This would have led to a multiplication factor which was obviously far too high above 1 had I not made a very pessimistic assumption about the resonance absorption of small spheres of uranium and assumed that much rich uranium spheres absorb at resonance considerably more than any other known element which has an even atomic number. Only in this way could I obtain for $\sigma_c(C) = 0.005$ sufficiently small value for qm to give a product μ q not too far above 1.

The quantity of resonance absorption enters into A-55 by means of a length B which A-55 took to be 6.5 cm. The assumption of such a large resonance absorption leads to very low values of qm but if we multiply q with μ in order to obtain the multiplication factor we come out with the approximately correct result. Had I at the time when A-55 was written used the correct value for the resonance absorption of uranium in conjunction with the value of $\mu=2$ I would have come out with far too high values for the multiplication factor which would not have fitted with my knowledge that the graphite-uranium system can only be slightly better than the water-uranium system and, therefore, cannot have a multiplication factor very much above 1, unless the use of carbon were much smaller than 0.005.

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letter to Fermi, July 5, 1939, which reads as follows: "carbon would also have an advantage over hydrogen in so far as there is no change in the scattering cross section, in the transition from the resonance region to the thermal region. Consequently, if layers of uranium oxide of finite thickness are used, the diffusion of the thermal neutrons produced in the carbon to the uranium layer is not adversely affected as in the case of hydrogen by such a change." It is due to this fact that we can use fairly large spheres of uranium and fairly large distances between these spheres in the graphite and that is the reason why A-55 was justified in stating

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3. On page 21, a formula is given for the critical radius ℓ. Using the value of 50 cm given on p.21 have $\sqrt{\overline{r^2}}$ one may write,

$$\ell = \sqrt{\frac{7500}{\mu q - 1}}$$

This formula is correct in terms of μq and gives for μ q=1.125, ℓ = 250 cm corresponding to about 100 tons of graphite.

In view of the general statements made in the paper concerning the inaccuracy of some of the constants involved, and in view of the great sensitivity of the formula for ℓ to changes in the multiplication factor, it must be obvious size of the proposed chain reacting system. It is my contention, which I do not propose to discuss here, that in February, 1940, there were methods available and known to men skilled in the art of inducing radioactivity by neutrons which permitted to determine the accurate size by making measurements on structures which were smaller than the accurate size.

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fav

from Szilard

July 21, 1943

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Dr. Dempster
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This formula is correct in terms of μq and gives for $\mu q = 1.125$, l = 250 cm corresponding to about 100 tons of graphite.

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to the distance $\sqrt{r^{-2}}$ which is about 50 cm. See in this connection the first
paragraph on p.11.)

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according to A-55 to choose R so as to make ε a maximum. This is obvious from
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At the time when A-55 was written, there was a consensus of opinion based on various measurements on homogeneous and heterogeneous water-uranium systems that such systems may have at room temperature a multiplication factor which comes very close to 1, but will not exceed 1 by more than a minute fraction if at all. This opinion was based on much better evidence than there was available for any individual constant, for example, μ. If we now replaced, as the slowing agent, hydrogen by carbon by substituting 25 to 30 carbon atoms for one hydrogen atom, one should expect a system in which the loss of neutrons due to uranium resonance is about equal to the loss due to resonance in the hydrogeneous system. Taking at room temperature the absorption cross section of carbon at .005, 25 to 30 carbon atoms would, at room temperature, have an absorption of only about .13 to .15, whereas, the thermal absorption cross section of hydrogen atom is about .3. Accordingly, one has to expect that the uranium-graphite system is more favorable and will have, in the most favorable case, a multiplication favo

cation factor above 1, but cannot have, at room temperature, a multiplication factor very much above 1, unless the carbon cross section should be considerably lower than .005.

The set of constants used in A-55 was so adjusted that the formulae given in A-55 yield for room temperature in the most favorable case for $\sigma_c(C) = 0.005$ a multiplication factor of <u>slightly</u> above one, for $\mu = 1.87$; q = 0.6; $\mu_q = 1.125$.

The same set of constants give then if put into the formulae for room temperature as the most favorable size for 15 gm/cc density a radius slightly above 5 cm and for the most favorable weight ratio of uranium to carbon a value which is close to the optimum and for which <u>in the light of our present knowledge</u> the multiplication factor would be in fact above 1.

Please note that from the consideration given above it would follow that of the carbon cross section were 0.01, carbon would just about be equivalent to hydrogen. I wrote in my letter to Fermi dated July 3, 1939(which dealt with heterogeneous mixtures but did not yet envisage, the use of thick layers of graphite) "If the cross section were 0.01 carbon would be no better than hydrogen." On July 5, 1939, envisaging the use of thick layers of carbon between uranium-containing elements I wrote to Fermi: "carbon would also have an advantage over hydrogen insofar as there is no change in the scattering cross section, in the transition from the resonance region to the thermal region. Consequently, if layers of uranium oxide of finite thickness are used, the diffusion of the thermal neutrons produced in the carbon to the uranium layer is not adversely affected as in the case of hydrogen by such a change."

This consideration gives carbon a sufficient advantage going sufficiently beyond the equivalent considerations stated in the letter of July 3rd to enable me to state in A-55 that the chain reacting in the uranium-carbon lattice would go

even if the carbon absorption were 0.01, but the advantage was not sufficient
to make this statement hold for room temperature. For high temperature where
the optimum is shifted towards larger uranium spheres and thicker layers of
carbon between the uranium spheres this advantage of carbon becomes greater
and A-55 says that it becomes sufficiently great to raise the multiplication
factor above one for a carbon cross section of 0.01.

c. I submit that it is not justified to criticize A-55 on the grounds
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corresponding to about 100 tons of graphite.

In view of the general statements made in the paper concerning the inaccuracy
of some of the constants involved, and in view of the great sensitivity of
the formula for l to changes in the multiplication factor, it must be obvious to
anyone who reads A-55 that the statement $l = 250$ cm indicates only the approximate
size of the proposed chain reacting system. It is my contention, which I do not
propose to ~~mention~~ discuss here, that in February 1940, there were methods available and
known to men skilled in the art of inducing radioactivity by neutrons which permitted
to determine the ~~accurate~~ size by making measurements on structures which were smaller
than the ~~accurate~~ critical size.

December 14, 1959

Dr. Hardin Jones
Donner Laboratory
Berkeley Campus

Dear Hardin:

Thanks for the correspondence with Szilard. On the sex ratio business, the U.S. data for many years certainly gave a highly significant partial regression with age of father and a small non-significant one with age of mother, although later less extensive data showed that the regression was most importantly with parity! (The latter data were classified simultaneously for age of mother, of father, and order of birth). I was quite interested in these data, which were obtained by Novitski and worked over by him and by Alan Kimball. I personally came to the rather unsatisfactory belief that there probably exist in the U.S. subpopulations with different sex ratios, and that classification by the three variables mentioned above cuts across these subpopulations in such a way that only a small portion of the variability showed up between the age and parity groups. I would not like to have to prove this belief but it seems to me reasonable in view of the difficulty of making much sense out of the parity correlation, and of the very small variation in these data compared with some reports showing much larger differences in special cases.

Dr. Szilard's ageing theories are certainly interesting and I would think, have a good chance of being essentially valid to a degree—i.e. I doubt if there is any one simple explanation but this could certainly be a partial one. I am not too happy about the particular way he applies the theory to sex ratio. For example, I doubt very much whether a mutation in the Y chromosome would kill or particularly incapacitate a spermatagonium or prevent it from producing functional sperm. However we do, now, have reason to suppose that a functional sperm lacking both a Y and X chromosome would probably produce a Turner-syndrome sterile female; but it seems to me that the frequency of production of a completely incapacitated Y in spermatagonia is not likely to be very frequent. I also agree with you that the postulated frequency of a lethal mutation in the sex chromosomes of spermatagonia is rather high, so much so that I think it might be difficult to explain the survival of human populations. In addition it is quite possible that many of the recessive lethals that would kill a zygote or embryo when homozygous or heterozygous might not kill a spermatagonium. Kaplan as I remember was able to induce lethals in germ-line X chromosomes of males (although I think there was some reason to suppose that maybe half of them were cell lethal and therefore not detected) by feeding larvae on formaldehyde.

I wonder why Dr. Szilard does not consider the usual theory that mutation of an X chromosome in an oogonium would lead to lethality in male zygotes and would, if recessive, have no effect (usually) in female zygotes? Even if the X were completely incapacited one might obtain an X-0 Turner syndrome female but certainly no male.

Perhaps above I did not make clear the improbability of lethal effects in a Y chromosome—since XO individuals survive and seem to be fairly healthy except for being sterile, it seems to me unlikely that lethal mutations would occur frequently, although of course two hit lethals (say dicentrics) or possibly some other kind of chromosome aberration resulting in possible mechanical difficulties is not at all out of the question.

P.S. (over)

With regards,

Everett

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ERD:yu

P.S.

I — should have mentioned the information available on offspring of irrad. mice mated after the production of new spermatocytes from irradiated spermatogonia. Henry Kohn has fairly extensive data for a considerable range of dosages with no unilateral trend with dosage & very slight effect, considering the dosage.