Corresp - Nat. Def.

CONFERENCE WITH DR. LEO SZILARD
At One William Street
New York City
July 25, 1939

In pursuit of our recent discussions as to the need for bring to the attention of President Roosevelt the implications of atomic research for national defense and as background for the letter to be drafted by Dr. Szilard and the present writer for Dr. Einstein and the correlative documents from the present writer and Dr. Szilard to be submitted to the President, the present writer felt it necessary to have available evidence of the disinterest of the Services in the project, as otherwise the President might say that the matter ought to be taken up with the Army and the Navy on the technical level.

Accordingly, Dr. Szilard approached the outstanding adviser of the Services, who was sufficiently interest in theoretical scientific developments as to attend all important scientific gatherings.

In June this year a sequel to the spring experiments by Drs. Szilard and Fermi was completed and its results [redacted] enhanced the implications that were drawn by Dr. Szilard from the experiments recorded in the Physical Review of April 15, 1939 entitled "Instantaneous Emission of Fast Neutrons in the Interaction of Slow Neutrons with Uranium." This experiment was deemed particularly by Dr. Szilard as strengthening the hope that a chain reaction could be set up using ordinary uranium. Accordingly, at the meeting of the American Physical Society held at Princeton towards the end of June, Dr. Szilard approached Mr. Ross Gunn, technical adviser to the Naval Research Laboratory in Washington, with the proposition that the Navy itself should support work by Dr. Fermi and himself looking to effectuating such a chain reaction.

This representation to the Navy on the technical level was the sequal to representations that were made by Dean Pegram of Columbia in March 1939. The initial representations were made by Dr. Fermi. It was with mindfulness of these representations that Dr. Szilard talked at considerable length at Princeton to Mr. Ross Gunn.

The culminating result of all the representations was a formal notification by Mr. Gunn on July 10th to Dr. Szilard and the Department of Physics of Columbia that the Navy could not in any way be helpful to the experiments that were then in progress. While the letter of July 10th on the stationery of the Naval Research Laboratory concluded with an expression of a desire to be kept informed, the negative character of that communication was conclusive.

Accordingly, it was felt by us that an appeal to the President in his capacity as Commander-in-Chief was imperative. Moreover, the declination by the Navy of the invitation to be associated in any way with the project - for even moral support would have led to more positive and larger-scale than University support and called for ~~wkack~~ interrelated acts and measures to overcome what we deemed to be the probable hesitation on the part of the President to intervent in a highly abstruse scientific matter over the heads of scientifically trained and scientific-minded officials of the Navy and the Army.

Granting the long confidential relationship of the writer to the President on problems of international relations and defense, this formal declination by a Navy official, underlined by his statement that this was a situation from which he could "see no escape," calls for an adequately prepared and exceptionally impressive case when, as it is hoped, the matter can be presented to the President.

The discussions thus far have crystallized into the following positiv and negative ideas as to the way to proceed:

1. Since the direct approach by the Columbia scientists to the Navy has proved fruitless and a similar approach to the Army will likely produce the same negative result, the case before the President should be very definitely oriented. It should be in terms of assuring this country security against the terrible shock as well as the dangers from even reports of further progress by Nazi scientists to convert atomic ~~energy~~ research into an atomic weapon. Only if the refugee scientists who are best oriented on the research are enabled to proceed with speed and with the support of the Government will the Commander-in-Chief and the Defense Services be at least au courant of the portentous potential of danger that atomic research for war purposes might entail.

2. The evaluation of the research and its import for the defense of the United States and the threatened Western Civilization should at least be reinforced by a communication from a scientist of world fame to whose message the President would respons with a chord of recognition and confidence. The one scientist who meets all these qualifications is Dr. Albert Einstein. Hence, our first task is representations to Dr. Einstein and the submission of a draft for a communication from him to the President. Such communication should be reinforced by a more detailed technical summary by Dr. Szilard as a co-leader with Dr. Fermi in the atomic research being now pursued in this country and as one sensitive to the political implications.

3. Since the new orientation of the appeal to the President is the international crisis and the preview of the present writer of the imminent war, the covering letter by the writer should be supplemented by the analysis of the international situation made by the writer last spring entitled "Imminence World War in Perspective Accrued Errors and Cultural Crisis of the Inter-War Decades."

Corresp - Na - Dep.

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420 WEST 116TH STREET
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UNDER KNOTT MANAGEMENT
OPPOSITE COLUMBIA UNIVERSITY
TELEPHONE UNIVERSITY 4-2700

November 5th, 1939

Dr. Alexander Sachs
One South William Street
New York City

Dear Dr. Sachs:

I wish to confirm our appointment for Tuesday night, 7 p.m., at the Men's Faculty Club of Columbia, 400 West 117th Street (117th Street and Morningside Drive). I think you will find both Dr. Pegram and Dr. Fermi very enjoyable persons.

In addition to what I told you over the telephone I should like to make some observations for your personal information:

I expected Briggs to enlarge his committee by including men like you, K.T. Compton or G.B. Pegram. It was a surprise for me to hear that he wanted to include also a group of younger physicists who are themselves actively engaged in doing research on uranium, namely Fermi, Tuve and Beams.

To the inclusion of this second group I should like to make two observations:

1. Since it so happens that the proposed second group includes the name of Fermi we could be assured that the committee will always be well informed and conscientiously advised. The committee would not have to depend on information gathered

haphazardously. This may prove to be a very important point and may outweigh all other considerations.

2. The fact that such a second group is being included and that it does not contain my name will make it virtually impossible for me to do in the future what I tried to do in the past, i.e. concern myself beyond the scope of my own experiments with the broader aspects connected with the possibility of a chain reaction, and to act as a driving power in this connection. For me to go on in the future as I did in the past, with a status wholly undefined at a time when some other colleagues have a clearly defined status, would hardly be advisable and in the end probably physically impossible.

I came up against similar difficulties in England six years ago. When the German government started to dismiss German scholars I persuaded Sir William Beveredge to form a committee and create an organization for assisting and placing these scholars. After this was done I went on working for this cause for another six months without having any defined status. Though I finally succeeded in getting a number of things done by exerting myself up to the limit of my strength I learned a lesson, and now I am anxious to avoid a repetition of this experience.

This point may have little importance from a general point of view, but I feel that I have to state my case now so that after the proposed committee has been appointed you may not think that I am willfully abandoning a cause when in fact I shall have little choice left in the matter.

In addition to these observations I should like to repeat what I told you over the telephone:

It seemed to me that the omission of the name of G.B. Pegram, who is Head of the Physics Department at Columbia and also Dean of the Graduate School, might be an objective mistake and at the same time also be embarrassing to Fermi. I had a conversation on this subject with Fermi, and we thought that if the committee had the right to co-opt members you might find it perhaps possible to suggest the inclusion of Pegram at the first meeting of the committee.

On Monday I shall telephone your secretary in order to find out if there are any points in the memorandum which you are preparing, or anything else, which you care to discuss with me. I am looking forward to seeing you in any case Tuesday night.

Yours very sincerely,

Leo Szilard

(Leo Szilard)

In such a system the absorption of neutrons takes place in three different ways: The neutrons are absorbed at thermal energies, both by hydrogen and uranium, and they are also absorbed by uranium at resonance before they are slowed down to thermal energies. Our result is independent of the ratio of the concentrations of hydrogen and uranium, insofar as it shows that, for thermal neutrons, the ratio of the cross section for neutron production and neutron absorption in uranium is greater than one, and probably about 1.5. What fraction of the neutrons will reach thermal energies without being absorbed will, however, depend on the ratio of the average concentrations of hydrogen and uranium. Since there is an appreciable absorption even far from the center of the resonance band, it follows that the fraction of neutrons absorbed by uranium at resonance will increase with decreasing hydrogen concentration. This has to be taken into account in discussing the possibility of a nuclear chain reaction in a system composed essentially of uranium and hydrogen. A chain reaction would require that more neutrons be produced by uranium than absorbed by uranium and hydrogen together. In our experiment the ratio of the average concentration of hydrogen to uranium atoms was 17 to 1, and in the experiment of von Halban, Joliot and Kovarski this ratio was 70 to 1. At such concentrations the absorption of hydrogen in the thermal region will prevent a chain reaction. By reducing the concentration of hydrogen one would obtain the following effect: On the one hand a larger fraction of those neutrons which reach thermal energies will be absorbed by uranium on the other hand, fewer neutrons reach the thermal region, due to an increased absorption by uranium at resonance. Of these two counteracting factors the first is more important for high hydrogen concentrations, and the second is more important for low hydrogen concentrations. Starting with high hydrogen concentrations, the ratio of neutron production to total neutron absorption will thus first rise, pass through a maximum, and then decrease with decreasing hydrogen concentrations. We attempted to estimate the quantities involved from the information available about resonance absorption in uranium[5–7] and the observed net gain of 0.2 in the number of neutrons produced and absorbed by uranium in our experiment. The effect of the absorption at resonance turns out to be so large that even at the optimum concentration of hydrogen it is at present quite uncertain whether neutron production will exceed the total neutron absorption. More information concerning the resonance absorption of uranium as well as more accurate measurement of some of the values which enter into our calculation are required before we can conclude whether a chain reaction is possible in mixtures of uranium and water.

[5] Meitner, Hahn and Strassman, Zeits. f. Physik 106, 249 (1937).
[6] V. Halban, Kovarski and Savitch, Comptes rendus 208, 1396 (1939).
[7] H. L. Anderson and E. Fermi, Phys. Rev. 55, 1106 (1939).

We wish to thank Dr. D. W. Stewart, of the Department of Chemistry, and Mr. S. E. Krewer, for advice and assistance in carrying out some of these experiments. We are much indebted to the Eldorado Radium Corporation for enabling us to work with large quantities of uranium oxide in our experiments, and to the Association for Scientific Collaboration for the use of the photo-

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Neutron Production and Absorption in Uranium

H. L. Anderson, E. Fermi and Leo Szilard
Columbia University, New York, New York

(Received July 3, 1939)

IT has been found[1-3] that there is an abundant emission of neutrons from uranium under the action of slow neutrons, and it is of interest to ascertain whether and to what extent the number of neutrons emitted exceeds the number absorbed.

[1] V. Halban, Joliot and Kovarski, Nature **143**, 470 (1939).
[2] L. Szilard and W. H. Zinn, Phys. Rev. **55**, 799 (1939).
[3] Anderson, Fermi and Hanstein, Phys. Rev. **55**, 797 (1939).

This question can be investigated by placing a photo-neutron source in the center of a large water tank and comparing, with and without uranium in the water, the number of thermal neutrons present in the water. In the previous experiments of this type[1, 2] it was attempted to have as closely as possible a spherically symmetrical distribution of neutrons. The number of thermal neutrons present in the water was determined by measuring along one radius the neutron density ρ as a function of the distance r from the center, and then calculating $\int r^2 \rho dr$. A difference in favor of uranium of about five percent was reported by von Halban, Joliot and Kovarski.[4]

[4] V. Halban, Joliot and Kovarski, Nature **143**, 680 (1939).

Since one has to measure a small difference, slight deviations from a spherically symmetrical distribution might give misleading results. The present experiments which are based on the same general principle do not require such symmetry. In order to measnre the nnmber of thermal neutrons in the water we filled the tank with a ten-percent solution of $MnSO_4$. The activity induced in manganese is proportional to the number of thermal neutrons present. A physical integration was performed by stirring the solution before measuring the activity of a sample with an ionization chamber. To obtain an effect of sufficient magnitude, about 200 kg of U_3O_8 was used.

The experimental arrangement is shown in Fig. 1. A photo-neutron source, consisting of about 2 g of radium and 250 g of beryllium was placed in the center of the tank. The geometry was such that practically all neutrons emitted by the source and by the uranium oxide were slowed down and absorbed within the tank. Each irradiation extended over several half-life periods of radiomanganese, and the observed activity of the solution was about four times the background of the ionization chamber. Alternating measurements were taken with the cans filled with uranium oxide, and with empty cans of the same dimensions. The activity proved to be about ten percent higher with uranium oxide than without it. This result shows that in our arrangement more nentrons are emitted by uranium than are absorbed by uranium.

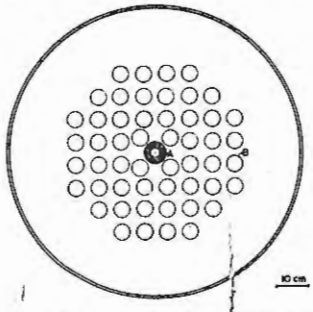

FIG. 1. Horizontal section through center of cylindrical tank which is filled with 540 liters of 10-percent $MnSO^4$ solution. A, Photo-neutron source composed of 2.3 grams of radium and 250 grams of beryllium. B, One of 52 cylindrical cans 5 cm in diameter and 60 cm in height, which are either empty or filled with uranium oxide.

In order to find the average number of fast neutrons emitted by uranium for each thermal neutron absorbed by uranium, we have to determine what fraction of the total number of neutrons emitted by the photo-neutron source is, in our experiment, absorbed in the thermal region by uranium. The number of photo-neutrons emitted by the source is indicated by the activity of the solution in the tank when the irradiation is carried out with empty cans surrounding the source. We obtained a measure of this number by taking into account that in our solution about 20 percent of the neutrons are captured by manganese and the rest by hydrogen. In order to obtain, in the same units, a measure of the number of neutrons absorbed by uranium we proceeded in the following way: A mixture of sand and manganese powder, having the same thermal neutron absorption as uranium oxide replaced the uranium oxide in $\frac{1}{4}$ of the cans which were distributed uniformly among the other uranium

Neutron Production and Absorption in Uranium

H. L. Anderson, E. Fermi and Leo Szilard
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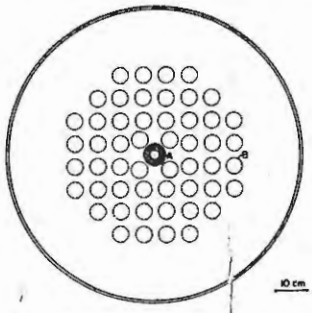

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In this way we found that about 50 percent of the neutrons emitted by the source are absorbed as thermal neutrons by uranium in our arrangement. It follows that, if uranium absorbed only thermal neutrons, the observed ten percent increase in activity obtained with uranium present would correspond to an average emission of about 1.2 neutrons per thermal neutron absorbed by uranium. This number should be increased, to perhaps 1.5, by taking into account the fraction of neutrons which, in our particular arrangement, is absorbed at resonance in the nonthermal region by uranium without causing neutron emission.

From this result we may conclude that a nuclear chain reaction could be maintained in a system in which neutrons are slowed dowa without much absorption until they reach thermal energies and are then mostly absorbed by uranium rather than by another element. It remains an open question, however, whether this holds for a system in which hydrogen is used for slowing down the neutrons.

Letters to the Editor

The Editor does not hold himself responsible for opinions expressed by his correspondents. He cannot undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*

NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR ON P. 247.

CORRESPONDENTS ARE INVITED TO ATTACH SIMILAR SUMMARIES TO THEIR COMMUNICATIONS.

Disintegration of Uranium by Neutrons: a New Type of Nuclear Reaction

ON bombarding uranium with neutrons, Fermi and collaborators[1] found that at least four radioactive substances were produced, to two of which atomic numbers larger than 92 were ascribed. Further investigations[2] demonstrated the existence of at least nine radioactive periods, six of which were assigned to elements beyond uranium, and nuclear isomerism had to be assumed in order to account for their chemical behaviour together with their genetic relations.

In making chemical assignments, it was always assumed that these radioactive bodies had atomic numbers near that of the element bombarded, since only particles with one or two charges were known to be emitted from nuclei. A body, for example, with similar properties to those of osmium was assumed to be eka-osmium ($Z = 94$) rather than osmium ($Z = 76$) or ruthenium ($Z = 44$).

Following up an observation of Curie and Savitch[3], Hahn and Strassmann[4] found that a group of at least three radioactive bodies, formed from uranium under neutron bombardment, were chemically similar to barium and, therefore, presumably isotopic with radium. Further investigation[5], however, showed that it was impossible to separate these bodies from barium (although mesothorium, an isotope of radium, was readily separated in the same experiment), so that Hahn and Strassmann were forced to conclude that *isotopes of barium* ($Z = 56$) *are formed as a consequence of the bombardment of uranium* ($Z = 92$) *with neutrons.*

At first sight, this result seems very hard to understand. The formation of elements much below uranium has been considered before, but was always rejected for physical reasons, so long as the chemical evidence was not entirely clear cut. The emission, within a short time, of a large number of charged particles may be regarded as excluded by the small penetrability of the 'Coulomb barrier', indicated by Gamov's theory of alpha decay.

On the basis, however, of present ideas about the behaviour of heavy nuclei[6], an entirely different and essentially classical picture of these new disintegration processes suggests itself. On account of their close packing and strong energy exchange, the particles in a heavy nucleus would be expected to move in a collective way which has some resemblance to the movement of a liquid drop. If the movement is made sufficiently violent by adding energy, such a drop may divide itself into two smaller drops.

In the discussion of the energies involved in the deformation of nuclei, the concept of surface tension of nuclear matter has been used[7] and its value has been estimated from simple considerations regarding nuclear forces. It must be remembered, however, that the surface tension of a charged droplet is diminished by its charge, and a rough estimate shows that the surface tension of nuclei, decreasing with increasing nuclear charge, may become zero for atomic numbers of the order of 100.

It seems therefore possible that the uranium nucleus has only small stability of form, and may, after neutron capture, divide itself into two nuclei of roughly equal size (the precise ratio of sizes depending on finer structural features and perhaps partly on chance). These two nuclei will repel each other and should gain a total kinetic energy of c. 200 Mev., as calculated from nuclear radius and charge. This amount of energy may actually be expected to be available from the difference in packing fraction between uranium and the elements in the middle of the periodic system. The whole 'fission' process can thus be described in an essentially classical way, without having to consider quantum-mechanical 'tunnel effects', which would actually be extremely small, on account of the large masses involved.

After division, the high neutron/proton ratio of uranium will tend to readjust itself by beta decay to the lower value suitable for lighter elements. Probably each part will thus give rise to a chain of disintegrations. If one of the parts is an isotope of barium[5], the other will be krypton ($Z = 92 - 56$), which might decay through rubidium, strontium and yttrium to zirconium. Perhaps one or two of the supposed barium-lanthanum-cerium chains are then actually strontium-yttrium-zirconium chains.

It is possible[5], and seems to us rather probable, that the periods which have been ascribed to elements beyond uranium are also due to light elements. From the chemical evidence, the two short periods (10 sec. and 40 sec.) so far ascribed to ^{239}U might be masurium isotopes ($Z = 43$) decaying through ruthenium, rhodium, palladium and silver into cadmium.

In all these cases it might not be necessary to assume nuclear isomerism; but the different radioactive periods belonging to the same chemical element may then be attributed to different isotopes of this element, since varying proportions of neutrons may be given to the two parts of the uranium nucleus.

By bombarding thorium with neutrons, activities are obtained which have been ascribed to radium and actinium isotopes[8]. Some of these periods are approximately equal to periods of barium and lanthanum isotopes[5] resulting from the bombardment of uranium. We should therefore like to suggest that these periods are due to a 'fission' of thorium which is like that of uranium and results partly in the same products. Of course, it would be especially interesting if one could obtain one of these products from a light element, for example, by means of neutron capture.

It might be mentioned that the body with half-life 24 min.[2] which was chemically identified with uranium is probably really ^{239}U, and goes over into an eka-rhenium which appears inactive but may decay slowly, probably with emission of alpha particles. (From inspection of the natural radioactive elements, ^{239}U cannot be expected to give more than one or two beta decays; the long chain of observed decays has always puzzled us.) The formation of this body is a typical resonance process[9]; the compound state must have a life-time a million times longer than the time it would take the nucleus to divide itself. Perhaps this state corresponds to some highly symmetrical type of motion of nuclear matter which does not favour 'fission' of the nucleus.

LISE MEITNER.

Physical Institute,
Academy of Sciences,
Stockholm.

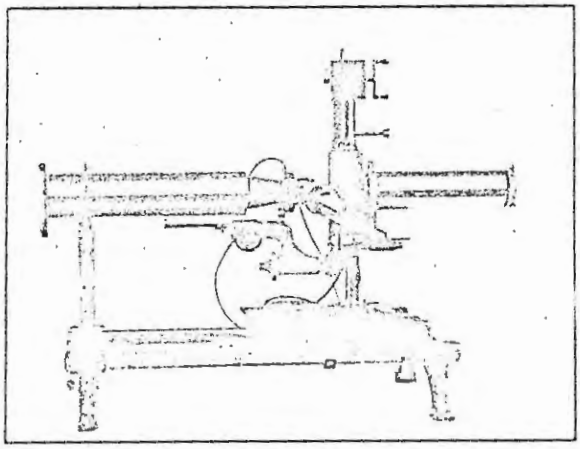
O. R. FRISCH.

Institute of Theoretical Physics,
University,
Copenhagen.
Jan. 16.

[1] Fermi, E., Amaldi, F., d'Agostino, O., Rasetti, F., and Segrè, E. *Proc. Roy. Soc.*, A, 146, 483 (1934).
[2] See Meitner, L., Hahn, O., and Strassmann, F., *Z. Phys.*, 106, 249 (1937).
[3] Curie, I., and Savitch, P., *C.R.*, 208, 906, 1643 (1938).
[4] Hahn, O., and Strassmann, F., *Naturwiss.*, 26, 756 (1938).
[5] Hahn, O., and Strassmann, F., *Naturwiss.*, 27, 11 (1939).
[6] Bohr, N., NATURE, 137, 344, 351 (1936).
[7] Bohr, N., and Kalckar, F., *Kgl. Danske Vid. Selskab, Math. Phys. Medd.*, 14, Nr. 10 (1937).
[8] See Meitner, L., Strassmann, F., and Hahn, O., *Z. Phys.*, 109, 538 (1938).
[9] Bethe, A. H., and Placzek, G., *Phys. Rev.*, 51, 450 (1937).

A Novel Thermostat

IT is often necessary to maintain an apparatus at a constant temperature. This may be done by immersing it in a circulating liquid maintained at a constant temperature by a thermostat, or by jacketing



TEMPERATURE-CONTROLLED APPARATUS.

it with alternate shells of thermally conducting and insulating materials heated to the selected temperature by means of an internal electric heater. These methods have the disadvantages that the thermostatic system makes the apparatus less accessible, the control of the temperature to within a narrow range requires some complication in the whole system, and it is difficult to prevent 'hunting'.

In a measurement which we are making of the electronic charge, it is necessary to maintain the temperature of the air, in which an oil drop moves, uniform and constant so that it has no motion due to convection. As a convenient solution of this problem has been found which seems capable of many applications, it is described here.

A resistance thermometer is formed by winding a single layer coil of copper wire around and in good thermal contact with the microscope condenser which forms part of the apparatus the temperature of which is under control. (In the accompanying illustration the condenser tube is on the right.) This coil forms one arm of a Wheatstone bridge, the other arms being of manganin resistances. Any change in temperature of the apparatus deflects the light spot of the galvanometer connected to this bridge, and for one direction of deflection the spot falls on a photo-electric cell, which operates a polarized relay, which in turn puts off two 30-watt lamps placed on opposite sides of the apparatus. The amplification of the galvanometer current by the photo-electric cell is 10^6, and including the relay about 10^7.

The bridge is adjusted to be balanced at a temperature a few degrees above the maximum temperature to which the room rises during a day. The lamps flash on and off every few seconds and maintain the temperature of the external surface of the apparatus constant to about 0·002° C. After the thermostat has been in operation for an hour, we have not been able to detect, by means of a thermo-couple, any change of temperature inside the apparatus.

T. H. LABY.
V. D. HOPPER.

Natural Philosophy Laboratory,
University of Melbourne.
Dec. 9.

Limitations on the Modern Tensor Scheme of Relativity

IT does not appear to have been noticed by anybody that the tensor scheme of relativity is incompetent by itself to include relations of chirality, to use Lord Kelvin's term. For it is developed from a pure Riemannian geometry, as based solely on the use of an ideal mobile a-chiral linear measuring rule. The meaning of relativity has, of course, always been that knowledge consists of the relations of one system to another, especially when one type of system of high simplicity, such as the linear measuring rule, is taken as the standard of comparison for all others. This significance of the chiral property, which is the difference between a chiral system and its mirror-image, for example, between a right-hand glove and a left-hand, goes back to Kant's early writings, and remained fundamental in his trains of thought in relation to space and time; later, in the more amateur hands of Pasteur, it created a fundamental science. Chiral systems can be compared completely only with chiral systems. The frame of reference for a chiral system must itself have chiral property; for example, to be effective, the mobile measuring rod of Einstein would require to possess a screw structure essential to it. When Newton explained how he could tell by experiment

Isotopes of the Alkaline Earth Metals from Uranium

DURING the last two years, the bombardment of uranium by neutrons has led to some striking results, due primarily to the combined efforts of Prof. O. Hahn, L. Meitner and F. Strassmann, working in the Kaiser Wilhelm Institute of Chemistry in Berlin-Dahlem. In addition to *three* artificial isotopes of uranium, they have succeeded in establishing the existence of *six* trans-uranium elements of atomic numbers 93–96, and the existence of a seventh trans-uranium element (half-value period = 60 days) is indicated.

In similar experiments, I. Curie and P. Savitch obtained evidence of the production of a substance with a half-value period of 3½ hours. The identity of this substance is uncertain, and although the authors have considered several possibilities, none of them is very satisfactory. Recent experiments on the chemical properties of the trans-uranium elements have led O. Hahn and F. Strassmann (*Naturwissenschaften*, 26, 755, Nov. 18, 1938; and 27, 11, Jan. 6, 1939) to look for this substance (half-value period = 3½ hours), and they have been successful in obtaining it by the same methods as those used by the French workers. Their investigations have led to remarkable results which are summarized in what follows.

The bombardment of uranium with neutrons apparently resulted in the production of *four isomeric isotopes of 'radium'*, which must have arisen by two successive α-ray transformations via thorium. These four 'radium' isotopes omit β-rays and yield four isomeric isotopes of 'actinium', which, in their turn, by further emission of β-rays, presumably give rise to four isotopes of 'thorium', but details of these last products are still incompletely known. The results so far obtained may be represented provisionally by the following scheme, in which the nature of the radiations emitted and the revised half-value periods of the new products are indicated.

$$^{238}_{92}U + {}^{1}_{0}n \longrightarrow {}^{239}_{92}U \xrightarrow{\alpha} {}^{235}_{90}Th \xrightarrow{\alpha} \begin{cases} \text{'Ra}_{\text{i}}\text{'} ? \xrightarrow[<1\text{ m.}]{\beta} \text{'Ac}_{\text{i}}\text{'} \xrightarrow[<30\text{ m.}]{\beta} \text{'Th'} ? \\ \text{'Ra}_{\text{ii}}\text{'} \xrightarrow[14\pm2\text{ m.}]{\beta} \text{'Ac}_{\text{ii}}\text{'} \xrightarrow[\text{c. 2·5h.}]{\beta} \text{'Th'} ? \\ \text{'Ra}_{\text{iii}}\text{'} \xrightarrow[80\pm6\text{m.}]{\beta} \text{'Ac}_{\text{iii}}\text{'} \xrightarrow[\text{sev. days?}]{\beta} \text{'Th'} ? \\ \text{'Ra}_{\text{iv}}\text{'} \xrightarrow[250\text{-}300\text{h.}]{\beta} \text{'Ac}_{\text{iv}}\text{'} \xrightarrow[<40\text{ h.}]{\beta} \text{'Th'} ? \end{cases}$$

Should this scheme adequately represent the facts, these twelve new substances should have atomic weights of value 231, and since a natural thorium isotope (UY) of atomic weight 231 and half-value period 25 hours is already known, it will be of interest to investigate whether one of the four 'thorium' isotopes is identical with UY.

Hahn and Strassmann believe that the substance found by Curie and Savitch is a mixture of the above-mentioned isotopes, each of which has been individually detected and chemically examined. Moreover, the properties of such a mixture would agree with those of the substance detected by the latter authors, who suspect that their substance also contains others of longer life, but of unknown genesis.

An interesting feature of this work is that the production of the new isotopes is enhanced by the use of *slow* neutrons, and the α-particle disintegrations indicated in the earlier stages of the process are believed to be the first instances of α-particle emission effected by *slow* neutrons. It is, of course, possible that the quadruplicity of isotopes resulting from the bombardment of uranium by neutrons takes place in $^{239}_{92}U$, as in the case of the trans-uranium elements, and the intermediate 'thorium' isotope $^{235}_{90}Th$ may also possess four half-value periods.

In the second of their publications mentioned above, Hahn and Strassmann give particulars of the chemical methods employed in establishing their results, as well as a number of activity curves from which some of the half-value periods have been derived. In order to establish beyond question the chemical nature of the products they have designated 'radium' isotopes, which were separated with barium, the authors carried out fractional crystallizations and precipitations with the active barium salts, by the method used for altering the concentration of radium in salts of barium containing radium.

As is well known, crystals richer in radium can be obtained by fractional crystallization of the chloride, bromide or chromate of barium, when it contains radium. In experiments on these lines, carried out by Hahn and Strassmann with their active barium preparations from which disintegration products had been removed, negative results were always obtained, that is, the activity was uniformly distributed amongst the various barium fractions, whereas control experiments with barium containing the radium isotopes ThX and $MsTh_1$ yielded results conforming to those obtained when the barium contains ordinary radium. In a further experiment, in which 'Ra$_{\text{iv}}$' and $MsTh_1$ were mixed with barium and then subjected to fractional crystallization, only the $MsTh_1$ was concentrated in the process.

The authors conclude from these results that their 'radium' isotopes have the properties of barium, and that, from the chemical point of view, other elements being excluded, these active substances must be regarded as consisting not of radium but of *barium*. This conclusion has been confirmed by further indicator experiments, according to a personal communication from one of the authors (O.H.).

Hahn and Strassmann have also subjected a mixture of their 'Ac$_{\text{ii}}$' and the actinium isotope $MsTh_2$ with lanthanum oxalate to fractionation from a nitric acid solution. At the end of the process a gain in concentration of $MsTh_2$ was achieved, but no increase in the concentration of 'Ac$_{\text{ii}}$' was noted. From this they conclude that the substances generated by β-ray emission from their alkaline-earth products are not isotopes of actinium, a conclusion which agrees with that of Curie and Savitch for their composite product of half-value period 3½ hours. In all probability they will prove to be isotopes of *lanthanum*. The products designated 'Th' in the above scheme of disintegration, which constitute the final members of the series

derived from the 'Ac-La' preparations, have not yet been tested for their identity with *cerium*.

On the strength of the chemical evidence, it is difficult to avoid the conclusion that the elements denoted by the symbol 'Ra' in the above-mentioned disintegration scheme should in reality be designated 'Ba', and so far as the evidence has gone it seems probable that the succeeding elements 'Ac' and 'Th' will prove to be 'La' and 'Ce' respectively. It is as though, at some stage in the process resulting from the bombardment of uranium by neutrons, a 'collapse' of the nucleus occurs, giving rise to the four active isomeric alkaline-earth elements and the succeeding 'lanthanum' and 'cerium' products. The investigations are being continued. R. W. L.

Subspecies and Varieties

A DISCUSSION dealing with subspecies and varieties was held at the meeting of the Linnean Society on February 2. This discussion had been arranged at the request of the Association for the Study of Systematics in Relation to General Biology in order to obtain information as to the views and principles governing the practice of systematists in various groups of animals and plants. It was apparent from the discussion that systematists are generally dissatisfied with the existing state of affairs. As was to be expected, little or no attention is paid to infra-specific categories in the less-known groups of both plant and animal kingdoms; but where the broader taxonomic outlines are well understood there is considerable divergence in systematic practices according to the amount of genetical, cytological and ecological work that has been done. In vertebrates, only a single infra-specific category, the 'subspecies', is generally recognized, but in entomology and botany there are others. How many there may be and their status in the taxonomic scheme are matters of dispute, and there is a disturbing confusion in the terminology applied to them. In this connexion, it may be mentioned that the Association is compiling a list of the various terms which have been used. Almost all the speakers stressed the need for more experimental work from the genetical, cytological, physiological and ecological aspects.

In opening the discussion, Mr. M. A. C. Hinton described the practice of mammalogists, who, in recent years, have used the term 'subspecies' to express stages or trends in geographical variation; they are sections of what Dr. Julian Huxley has called 'clines' (NATURE, 142, 219; 1938). One principle generally accepted is that no two races of the same species can ever be found on the same ground, though exceptionally two such forms may meet after very different histories and journeys and continue to exist side by side without fusing. The principle that, unless intergradation can be demonstrated, differences, however trivial, indicate specific separation, has some theoretical support. In practice, however, it has disadvantages, especially in dealing with insular forms, and tends to mask that most interesting and instructive phenomenon, discontinuous distribution.

The definition of subspecies is an essential part of any intensive analysis of the facts of variation and distribution, and is particularly valuable in palæontology, by preventing loose identifications and consequent faulty geological deductions. It is hoped soon to commence large-scale experiments to test the values and permanence of subspecific characters and to obtain more definite information on subjects concerning which surmise alone is at present possible.

Dr. W. B. Turrill emphasized that all taxonomic categories are matters of scientific convenience based on abstractions from a continuous evolution. He recognizes that for little-known floras it is undesirable to analyse below the species level; but urged that in relatively well-studied floras the need is for intensive studies of infra-specific variation from every angle. His own researches have led him to the conclusion that taxonomic categories intergrade completely. He suggested that the term 'variety' should be used for every phenotype which is the expression of a different genotype, and that the term subspecies should be used only when a species is in process of breaking up into new ones. Names should be given to infra-specific groups only to serve some definite purpose, and in highly polymorphic species symbols might be used instead. Apomicts should receive distinct treatment.

Mr. A. J. Wilmott pointed out that confusion results from using the words species, subspecies and variety both for categories of variation in Nature and groups of different rank in the nomenclatural system. He suggested that the terms 'binome' and 'trinome' might be substituted for species and subspecies in the nomenclatural sense, leaving the latter free for application to categories of variation. True subspecies are parts of the present time section of a lineage which has become branched by isolation, but the branches of which are not yet completely separated. Varieties, on the other hand, are in a different category, being merely the observed phenotypic effects of separate genes, and not different *kinds* of organisms; they should not be given names under the same system as species and subspecies, but might be designated by symbols.

Mr. J. S. L. Gilmour dealt with the philosophical aspect of the subject and drew a distinction between taxonomic and non-taxonomic infra-specific categories. The former, which might be limited to subspecies, variety, and form, should be based so far as possible on the total attributes of the individuals concerned, while the latter should be based on a selection of attributes chosen for special purposes. Examples of such non-taxonomic categories are Dancer's 'commisceum', 'comparium', and 'convivium', based on interfertility data, and useful for investigating the relationship between such data and other attributes, such as morphological differentiation. He urged that this distinction is essential for the proper classification of infra-specific variability.

Mr. H. W. Parker showed that the practice of herpetologists is essentially similar to that of mammalogists. He believes, however, that owing to lack of other data, systematists have laid undue emphasis on geographical considerations, with invidious results. Recognition by trinomials of certain forms to the

Letters to the Editor

The Editor does not hold himself responsible for opinions expressed by his correspondents. He cannot undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*

NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR ON P. 337.

CORRESPONDENTS ARE INVITED TO ATTACH SIMILAR SUMMARIES TO THEIR COMMUNICATIONS.

Disintegration of Heavy Nuclei

THROUGH the kindness of the authors I have been informed of the content of the letters[1] recently sent to the Editor of NATURE by Prof. Meitner and Dr. Frisch. In the first letter, these authors propose an interpretation of the remarkable findings of Hahn and Strassmann as indication for a new type of disintegration of heavy nuclei, consisting in a fission of the nucleus into two parts of approximately equal masses and charges with release of enormous energy. In the second letter, Dr. Frisch describes experiments in which these parts are directly detected by the very large ionization they produce. Due to the extreme importance of this discovery, I should be glad to add a few comments on the mechanism of the fission process from the point of view of the general ideas, developed in recent years, to account for the main features of the nuclear reactions hitherto observed.

According to these ideas, any nuclear reaction initiated by collisions or radiation involves as an intermediate stage the formation of a compound nucleus in which the excitation energy is distributed among the various degrees of freedom in a way resembling the thermal agitation of a solid or liquid body. The relative probabilities of the different possible courses of the reaction will therefore depend on the facility with which this energy is either released as radiation or converted into a form suited to produce the disintegration of the compound nucleus. In the case of ordinary reactions, in which the disintegration consists in the escape of a single particle, this conversion means the concentration of a large part of the energy on some particle at the surface of the nucleus, and resembles therefore the evaporation of a molecule from a liquid drop. In the case of disintegrations comparable to the division of such a drop into two droplets, it is evidently necessary, however, that the quasi-thermal distribution of energy be largely converted into some special mode of vibration of the compound nucleus involving a considerable deformation of the nuclear surface.

In both cases, the course of the disintegration may thus be said to result from a fluctuation in the statistical distribution of the energy between the various degrees of freedom of the system, the probability of occurrence of which is essentially determined by the amount of energy to be concentrated on the particular type of motion considered and by the 'temperature' corresponding to the nuclear excitation. Since the effective cross-sections for the fission phenomena for neutrons of different velocities seem to be of about the same order of magnitude as the cross-sections for ordinary nuclear reactions, we may therefore conclude that for the heaviest nuclei the deformation energy sufficient for the fission is of the same order of magnitude as the energy necessary for the escape of a single nuclear particle. For somewhat lighter nuclei, however, where only evaporation-like disintegrations have so far been observed, the former energy should be considerably larger than the binding energy of a particle.

These circumstances find their straightforward explanation in the fact, stressed by Meitner and Frisch, that the mutual repulsion between the electric charges in a nucleus will for highly charged nuclei counteract to a large extent the effect of the short-range forces between the nuclear particles in opposing a deformation of the nucleus. The nuclear problem concerned reminds us indeed in several ways of the question of the stability of a charged liquid drop, and in particular, any deformation of a nucleus, sufficiently large for its fission, may be treated approximately as a classical mechanical problem, since the corresponding amplitude must evidently be large compared with the quantum mechanical zero-point oscillations. Just this condition would in fact seem to provide an understanding of the remarkable stability of heavy nuclei in their normal state or in the states of low excitation, in spite of the large amount of energy which would be liberated by an imaginable division of such nuclei.

The continuation of the experiments on the new type of nuclear disintegrations, and above all the closer examination of the conditions for their occurrence, should certainly yield most valuable information as regards the mechanism of nuclear excitation.

N. BOHR.

At the Institute for Advanced Study,
Princeton, N.J. Jan. 20.

[1] [NATURE, 143, 239 and 275 (1939)].

Photoactivation of Solids and its Effect on Adsorption

CONSIDERABLE attention has been given recently to chemical processes involving an activating influence of a crystal excited by irradiation[1]. The mechanism of such photosensitized reactions, although unknown in detail, is generally believed to be a more or less complete transfer of the energy absorbed by the crystal to the reacting components, physically or chemically. Accordingly, the essential difference between photosensitized processes and real photochemical ones is the distance between the place of absorption and the place of reaction. But there must also be another, more general, effect of irradiation on the activity of crystals. Due to the change in the electronic state of the particles in the lattice by the absorption of light (change in charge and degree of polarization, formation of space charges, etc.), the forces between the particles are changed and, consequently, there is also a change in potential of the

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REPRINT SERIES

INSTANTANEOUS EMISSION OF FAST NEUTRONS IN THE INTERACTION OF SLOW NEUTRONS WITH URANIUM

By
LEO SZILARD AND WALTER H. ZINN

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Instantaneous Emission of Fast Neutrons in the Interaction of Slow Neutrons with Uranium*

Recently it became known[1] that uranium can be split by neutrons into two elements of about equal atomic weight. In this fission of uranium the two elements produced have a large neutron excess; moreover they are probably produced in an excited nuclear state. One might therefore expect that these excited fragments instantaneously emit neutrons and that perhaps the number emitted is even larger than one per fission.

One might also expect a delayed emission of neutrons—as was first pointed out by Fermi—if some of the fragments go through one or more beta-transformations before they emit a neutron. Delayed emission of neutrons caused by the action of both slow and fast neutrons on uranium has recently been reported by Roberts, Meyer, and Wang,[2] who find a period of about 12 seconds.

In order to see if there is an instantaneous emission of neutrons from the fission of uranium we have performed the following experiment. We exposed uranium oxide to neutrons which were slowed down by paraffin wax, using as a source of neutrons a block of beryllium from which photoneutrons were liberated by the gamma-rays of radium. A helium-filled ionization chamber connected to a linear amplifier served as a detector for fast neutrons. The ionization pulses of the chamber were observed visually by means of a cathode-ray oscillograph and were recorded by the usual counting arrangement.

Figure 1 shows a diagram of the experimental arrangement. The ionization chamber is covered by a cadmium sheet cap G which prevents the thermal neutrons from penetrating to the helium ionization chamber. A cadmium sheet shield H, 0.5 mm thick, is used to cover the cylindrical box E which contains 2300 g of uranium oxide. The uranium oxide is screened from the thermal neutrons by this shield and can be exposed to them simply by removing the shield.

We observed about 50 pulses per minute from the helium chamber when we exposed the uranium oxide to the thermal neutrons in the absence of the cadmium shield H, but obtained only 5 pulses per minute when the uranium was screened from the thermal neutrons by the cadmium shield. The difference of about 45 pulses per minute we have to attribute to fast neutrons emitted from uranium under the action of thermal neutrons. It is reasonable to assume that this emission of fast neutrons is connected with the fission of uranium.

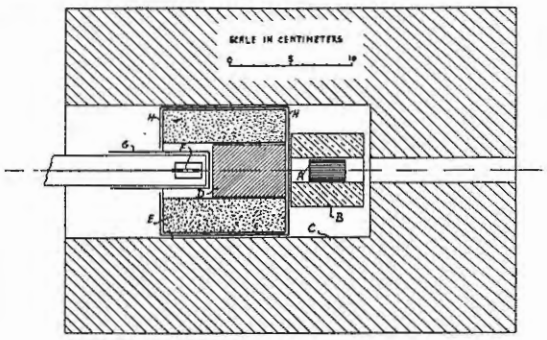

FIG. 1. Arrangement for the observation of the emission of fast neutrons from uranium. A, Radium. B, Beryllium block. C, Paraffin wax. D, Lead block. E, Box filled with uranium oxide. F, Ionization chamber. G, Cadmium sheet cap. H, Cadmium sheet shield.

Control experiments were carried out in which uranium was replaced by lead. The effect of the presence and absence of the cadmium shield H and the cadmium cap G was tested.

In order to estimate the number of fast neutrons emitted per fission under the action of thermal neutrons we used an ionization chamber lined with a thick layer of uranium oxide having an area of 25 cm². This uranium chamber was put in place of the helium chamber without otherwise materially changing the experimental arrangement. Under these conditions the uranium chamber gave about 45 fissions per minute. Assuming the range of the fission fragments to be about 0.005 g per cm² in uranium oxide, the observed 45 fissions per minute should occur in a surface layer, weighing 0.13 g, of the thick uranium oxide lining. Accordingly, about 800,000 fissions per minute should occur in the 2300 g of uranium oxide which was used in our experiment. By taking into account the solid angle, the size of the helium chamber and the pressure used, and by assuming that the "fission neutrons" have an average collision cross section in helium of 3.5×10^{-24} cm² we find the number of neutrons emitted per fission to be about two.

This number is of course only a rough estimate; the main cause of uncertainty is the considerable variation of the cross section of helium with the neutron energy in the region around one million volts.[3] A hydrogen-filled

ionization chamber is now being used in order to obtain a more accurate estimate. It seems to be established, however, that the order of magnitude is one neutron per fission.

Anderson, Fermi and Hanstein have independently, and by a different method, carried out experiments on the neutron emission connected with the fission of uranium. Our observations are consistent with their results, and we wish to thank them for communicating their results to us before publication.

While from our observations we can only say that the time delay involved in this "instantaneous" neutron emission appears to be less than one second, we should expect, for theoretical reasons, this emission to take place within less than 10^{-14} second.

We have also looked for a delayed emission of fast neutrons by performing the following experiment. The uranium oxide was irradiated for some length of time in the arrangement shown in Fig. 1. Then the radium was quickly removed from the beryllium block and the cathode-ray oscillograph screen was watched for a period of 15 seconds for an indication of a delayed emission of fast neutrons. After the radium is removed there is no gamma-ray background to set a lower limit for the observable helium recoil energy; the only slight background remaining is due to electrical fluctuations of the amplifier. In 50 experiments, corresponding to a total observation time of more than 12 minutes, we observed only two pulses which may or may not have been due to a delayed emission of fast neutrons. This is to be compared with the emission of 45 fast neutrons per minute, the number observed while the radium is inside the beryllium block. We conclude that, if slow neutrons falling on uranium cause a delayed emission of neutrons which are sufficiently fast for us to observe, their number must be very much smaller than the number of neutrons which we have observed in the instantaneous emission.

We are indebted to Dr. S. Seely for bis assistance in carrying out some of these experiments. We wish to thank the Department of Physics of Columbia University for the hospitality and the facilities extended to us, and also wish to thank the Association for Scientific Collaboration for enabling us to use one gram of radium in these experiments.

LEO SZILARD
WALTER H. ZINN

Pupin Physics Laboratories,
Columbia University,
New York, New York,
March 16, 1939.

* Publication assisted by the Ernest Kempton Adams Fund for Physical Research of Columbia University.

[1] O. Hahn and F. Strassmann, Naturwiss. 27, 11 (1939); L. Meituer and R. Frisch, Nature (February, 1939).
[2] R. B. Roberts, R. C. Meyer, and P. Wang, Phys. Rev. 55, 510 (1939).
[3] H. Staub and W. E. Stephens, Phys. Rev. 55, 131 (1939).

Comm-Nat. Def.

Ex 2b

MEMORANDUM

August 15th, 1939

Much experimentation on atomic desintegration was done during the past five years, but up to this year the problem of liberating nuclear energy could not be attacked with any reasonable hope for success. Early this year it became known that the element uranium can be split by neutrons. It appeared conceivable that in this nuclear process uranium itself may emit neutrons, and a few of us envisaged the possibility of liberating nuclear energy by means of a chain reaction of neutrons in uranium.

Experiments were thereupon performed, which led to striking results. One has to conclude that a nuclear chain reaction could be maintained under certain well defined conditions in a large mass of uranium. It still remains to prove this conclusion by actually setting up such a chain reaction in a large-scale experiment.

This new development in physics means that a new source of power is now being created. Large amounts of energy would be liberated, and large quantities of new radioactive elements would be produced in such a chain reaction.

In medical applications of radium we have to deal with quantities of grams; the new radioactive elements could be produced in the chain reaction in quantities corresponding to tons of radium equivalents. While the practical application would include the medical field, it would not be limited to it.

A radioactive element gives a continuous release of energy for a certain period of time. The amount of energy which is released per unit weight of material may be very large, and therefore such elements might

be used - if available in large quantities - as fuel for driving boats or airplanes. It should be pointed out however that the physiological action of the radiations emitted by these new radioactive elements makes it necessary to protect those who have to stay close to a large quantity of such an element, for instance the driver of the airplane, It may therefore be necessary to carry large quantities of lead, and this necessity might impede a development along this line, or at least limit the field of application.

Large quantities of energy would be liberated in a chain reaction, which might be utilized for purposes of power production in the form of a stationary power plant.

In view of this development it may be a question of national importance to secure an adequate supply of uranium. The United States has only very poor ores of uranium in moderate quantities; there is a good ore of uranium in Canada where the total deposit is estimated to be about 3000 tons; there may be about 1500 tons of uranium in Czechoslovaki which is now controlled by Germany; there is an unknown amount of uranium in Russia, but the most important source of uranium, consisting of an unknown but probably very large amount of good ore, is Belgian Congo.

It is suggested therefore to explore the possibility of bringing over from Belgium or Belgian Congo a large stock of pitchblend, which is the ore of both radium and uranium, and to keep this stock here for possible future use. Perhaps a large quantity of this ore might be obtained as a token reparation payment from the Belgian Government. In taking action along this line it would not be necessary officially to disclose that the uranium content of the ore is the point of interest; action might be taken on the ground that it is of value to secure a stock of the ore on account of its radium content for possible future

extraction of the radium for medical purposes.

Since it is unlikely that an earnest attempt to secure a supply of uranium will be made before the possibility of a chain reaction has been visibly demonstrated, it appears necessary to do this as quickly as possible by performing a large-scale experiment. The previous experiments have prepared the ground to the extent that it is now possible clearly to define the conditions under which such a large-scale experiment would have to be carried out. Still two or three different set-ups may have to be tried out, or alternatively preliminary experiments have to be carried out with several tons of material if we want to decide in advance in favor of one set-up or another. These experiments cannot be carried out within the limited budget which was provided for laboratory experiments in the past, and it has now become necessary either to strengthen - financially and otherwise - the organizations which concerned themselves with this work up to now, or to create some new organization for the purpose. Public-spirited private persons who are likely to be interested in supporting this enterprise should be approached without delay, or alternatively the collaboration of the chemical or the electrical industry should be sought.

The investigations were hitherto limited to chain reactions based on the action of <u>slow</u> neutrons. The neutrons emitted from the splitting uranium are fast, but they are slowed down in a mixture of uranium and a light element. Fast neutrons lose their energy in colliding with atoms of a light element in much the same way as a billard ball loses velocity in a collision with another ball. At present it is an open question whether such a chain reaction can also be made to work with <u>fast</u> neutrons which are not slowed down.

There is reason to believe that, if fast neutrons could be used, it would be easy to construct extremely dangerous bombs. The destructive power of these bombs can only be roughly estimated, but there is no doubt that it would go far beyond all military conceptions. It appears likely that such bombs would be too heavy to be transported by airplane, but still they could be transported by boat and exploded in port with disastrous results.

Although at present it is uncertain whether a fast neutron reaction can be made to work, from now on this possibility will have to be constantly kept in mind in view of its far-reaching military consequences. Experiments have been devised for settling this important point, and it is solely a question of organization to ensure that such experiments should be actually carried out.

Should the experiments show that a chain reaction will work with fast neutrons, it would then be highly advisable to arrange among scientists for withholding publications on this subject. An attempt to arrange for withholding publications on chain reactions has already been made early in March but was abandoned in spite of favorable response in this country and in England on account of the negative attitude of certain French laboratories. The experience gained in March would make it possible to revive this attempt whenever it should be necessary.

Leo Szilard

(Leo Szilard)

Corresp - Nat Bur.

Ex. 4

October 17, 1939

My dear Professor Teller:

In the wake of numerous conferences with Dr. Szilard since late summer and a more recent meeting with Professor Wigner of Princeton, I had the honor to submit last week to the President and to an informal committee from the Army, the Navy and the Bureau of Standards called together at his instance, information regarding the experimental work conducted by Dr. Szilard, Professor Fermi and others on atomic disintegration and certain proposals for aiding that work in the light of its potential significance for national defense.

Following that conference there was formed a committee including Colonel Adamson, Commander Hoover and Dr. Lyman J. Briggs. A conference has been scheduled by this committee with Professor Wigner, Dr. Szilard and myself for Saturday morning, October 21st, at 9:30 in Dr. Briggs' office in the Bureau of Standards of the Department of Commerce. In a telephone conversation with Dr. Briggs this afternoon, I submitted to him a suggestion which he cordially accepted, namely that you be included in this conference as one of the cognoscenti of this subject and as a common friend of the scientists from this end and the scientists from the Government end.

While I take it that you will hear direct from Dr. Briggs, this letter and the supplementary memorandum of Dr. Szilard will, I trust, serve to reinforce the invitation and to provide orientation on the purpose of the conference. As a matter of convenience, would you be good enough to call for us at the Carlton Hotel at 9:15 Saturday morning, when we will be arranging to proceed to the Department of Commerce.

Yours sincerely,

Professor E. Teller,
George Washington University,
Washington, D. C.

Albert Einstein
Old Grove Road
Peconic, Long Island
August 2nd, 1939

F. D. Roosevelt
President of the United States
White House
Washington, D.C.

Sir:

Some recent work by E. Fermi and L. Szilard, which has been communicated to me in manuscript, leads me to expect that the element uranium may be turned into a new and important source of energy in the immediate future. Certain aspects of the situation which has arisen seem to call for watchfulness and, if necessary, quick action on the part of the Administration. I believe therefore that it is my duty to bring to your attention the following facts and recommendation.

In the course of the last four months it has been made probable through the work of Joliot in France as well as Fermi and Szilard in America - that it may become possible to set up a nuclear chain reaction in a large mass of uranium, by which vast amounts of power and large quantities of new radium-like elements would be generated. Now it appears almost certain that this could be achieved in the immediate future.

This new phenomenon would also lead to the construction of bombs, and it is conceivable - though much less certain - that extremely powerful bombs of a new type may thus be constructed. A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with some of the surrounding territory. However, such bombs might very well prove to be too heavy for transportation by air.

The United States has only very poor ores of uranium in moderate quantities. There is some good ore in Canada and the former Czechoslovakia, while the most important source of uranium is Belgian Congo.

In view of this situation you may think it desirable to have some permanent contact maintained between the Administration and the group of physicists working on chain reactions in America. One possible way of achieving this might be for you to entrust with this task a person who has your confidence and who could perhaps serve in an unofficial capacity. His task might comprise the following:

a) to approach Government Departments, keep informed of the further developments, and put forward recommendations for Government action, giving particular attention to the problem of securing a supply of uranium ore for the United States.

b) to speed up the experimental work, which is at present being carried on within the limits of the budgets of University laboratories, by providing funds, if such funds be required, through his contacts with private persons who are willing to make contributions for this cause, and perhaps also by obtaining the co-operation of industrial laboratories which have the necessary equipment.

I understand that Germany has actually stopped the sale of uranium from the Czechoslovakian mines which she has taken over. That she should have taken such early action might perhaps be understood on the ground that the son of the German Under-Secretary of State, von Weizsacker, is attached to the Kaiser-Wilhelm-Institute in Berlin where some of the American work on uranium is now being repeated.

Yours very truly,

/s/ Albert Einstein

From: Alexander Sachs July 26, 1939

[Chron. file July 1939

DRAFT LETTER FOR DR. ALBERT EINSTEIN

(Acc. Dr. Aydelotte at Institute, at Dr. Moore's Cottage, Peconic Bay, Long Island)

Dear Mr. President:

It is as one who has come to cherish the freedom quest of the original Pilgrims and shares a similar quest of this decade's new pilgrims from persecution to the "life and liberty" of this country, that I beg your consideration by this and through our common friend, of an epochal discovery, which bears on U.S. security. At the turn 1938-9 researches, - in which the democracies inclusive scientists-refugees from Nazism and Fascism have led - climaxed in an experiment in Germany which effected the split or fission of the atom, entailing in the process unprecedentedly tremendous energy. This has been raised the grave question of possible discovery of ways to harness that power, in that ambivalence for evil as for good ((for)) the fruits of the Tree of Knowledge.

In the course of four months' work here by E. Fermi, Nobel Laureate in atomic physics and L. Szilard, from Germany via England, along with work by F. Joliot and colleagues in France Great Britain and Scandinavia, new vistas have been opening on setting up nuclear reactions towards the producibility of "chain reactions." In the favorable event, it might lead to the construction of atom bombs, by plane or boat.

Granting the not yet advanced stage of the research, the accelerated tempo of the burgeoning experiments and the onrushing war-dangers in Europe, the human and material resources involved seem to me to clamor for being concentrated here, lest the opportunities be foreclosed. As more advanced weapons have been exploited consistently against the democracies, taking time by the forelock in and across the new frontiers of science would give byproducts of military strength, and give the U.S. the lead in the specific atomic field. On the source-material, I am informed our uranium ore is moderate in quantity and inferior in quality Canada has larger and good supplies. The largest and most important source is the Belgian Congo, - a storehouse of uranium.

Without presuming to deal with matters of organization, permit me to stress the need for a confidant of yours as liaison with the scientists and Government. Such liaison would ascertain the progress and trends in researches in the universities, which are still handicapped in their budgets by the declines of income and capital funds from the slow economic recovery, as I am told by our friend. Financial resources have doubtless been readier and freer in Germany for research that might aid the military.

My more recent followers of these phases of atomic physics in touch with the European developments advise me that Germany has taken over the uranium mines in Czechoslovakia and stopped sales and exports. Arrangements for Canadian and Belgian sources should be made. And as crucial research-issues loom large, governmental and cognate industry and institutes should be interested.

Respectfully yours,

Corresp - Nat. Def.

1

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

LJB:DEK

October 14, 1939

D

Dr. Alexander Sachs,
Care of Lehman Brothers,
One William Street,
New York, N. Y.

Dear Dr. Sachs:

Confirming our conversation over the telephone last evening, General Watson has asked Colonel Adamson, Commander Hoover and myself to study the proposal which you submitted to the President. As I stated to you last evening, the Committee would appreciate the opportunity to meet with you and the gentleman you named at my office in the National Bureau of Standards on Wednesday morning, October 18th, at ten o'clock. I hope this date may be acceptable to you.

Sincerely yours,

L. J. Briggs

Lyman J. Briggs, Director.

Corresp - Nat. Def

King's Crown Hotel

420 WEST 116TH STREET
NEW YORK
UNDER KNOTT MANAGEMENT
OPPOSITE COLUMBIA UNIVERSITY

PRIVATE OFFICE

TELEPHONE UNIVERSITY 4-270

August 15th, 1939

Dr. Alexander Sachs
One William Street
New York City

Dear Dr. Sachs:

Enclosed I am sending you a letter from Prof. Albert Einstein, which is addressed to President Roosevelt and which he sent to me with the request of forwarding it through such channels as might appear appropriate. If you see your way to bring this letter to the attention of the President, I am certain Prof. Einstein would appreciate your doing so; otherwise would you be good enough to return the letter to me?

If a man, having courage and imagination, could be found and if such a man were put - in accordance with Dr. Einstein's suggestion - in the position to act with some measure of authority in this matter, this would certainly be an important step forward. In order that you may be able to see of what assistance such a man could be in our work, allow me please to give you a short account of the past history of the case.

In January this year, when I realized that there was a remote possibility of setting up a chain reaction in a large mass of uranium, I communicated with Prof. E.P. Wigner of Princeton University and Prof. E. Teller of George Washington University, Washington, D.C., and the three of us remained in constant consultation ever since. First of all it appeared necessary to perform certain fundamental experiments for which the use of about one gram of radium was required. Since at that time we had no certainty and had to act on a remote possibility, we could hardly hope to succeed in persuading a university laboratory to take charge of these experiments, or even to acquire the radium needed. Attempts to obtain the necessary funds from other sources appeared to be equally hopeless. In these circumstances a few of us physicists formed an association, called "Association for Scientific Collaboration", collected some funds among ourselves, rented about one gram of radium, and I arranged with the Physics Department of Columbia University for their permission to carry out the proposed experiments at Columbia. These experiments led early in March to rather striking results.

At about the same time Prof. E. Fermi, also at Columbia, made experiments of his own, independently of ours, and came to identical

conclusions.

A close collaboration arose out of this coincidence, and recently Dr. Fermi and I jointly performed experiments which make it appear probable that a chain reaction in uranium can be achieved in the immediate future.

The path along which we have to move is now clearly defined, but it takes some courage to embark on the journey. The experiments will be costly since we will now have to work with tons of material rather than - as hitherto - with kilograms. Two or possibly three different alternatives will have to be tried; failures, set-backs and some unavoidable danger to human life will have to be faced. We have so far made use of the Association for Scientific Collaboration to overcome the difficulty of persuading other organisations to take financial risks, and also to overcome the general reluctance to take action on the basis of probabilities in the absence of certainty. Now, in the face of greater certainty, but also greater risks, it will become necessary either to strengthen this association both morally and financially, or to find new ways which would serve the same purpose. We have to approach as quickly as possible public-spirited private persons and try to enlist their financial co-operation, or, failing in this, we would have to try to enlist the collaboration of the leading firms of the electrical or chemical industry.

Other aspects of the situation have to be kept in mind. Dr. Wigner is taking the stand that it is our duty to enlist the co-operation of the Administration. A few weeks ago he came to New York in order to discuss this point with Dr. Teller and me, and on his initiative conversations took place between Dr. Einstein and the three of us. This led to Dr. Einstein's decision to write to the President.

I am enclosing memorandum which will give you some of the views and opinions which were expressed in these conversations.

I wish to make it clear that, in approaching you, I am acting in the capacity of a trustee of the Association for Scientific Collaboration, and that I have no authority to speak in the name of the Physics Department of Columbia University, of which I am a guest.

Yours sincerely,

Leo Szilard

(Leo Szilard)

[Corresp. - Szilard]

July 26 1939

Dear Dr Szilard:

From my friend(s) in the Institute I have secured the indicated address - The main thing to realize is that the President is overwhelmed with the effort to amend the 1937 Neutrality Act. The odds appear against him [illegible] and its predecessors in our affliction of appeasement. The second and coeval thing to realize is that the [illegible] tendency from Roosevelt [illegible] is definite - Even [illegible] the latter [illegible] would [illegible] decision something back.

Pressing the President [illegible] Einstein [illegible] [illegible] [illegible]

[Chron file: July 1939]

GUIDANCE FOR CARRIER OF DRAFT TO DR. EINSTEIN

(Note to Dr. Leo Szilard)

July 26, 1939

Dear Dr. Szilard:

From my friends at the Institute, I have secured the indicated address. -- The main thing to realize is that the President is overwhelmed with the effort to amend the 1937 Neutrality Act. The odds appear against, for now. That Act and its predecessors is our affliction of "appeasement". The second and coeval thing to realize is that the Navy turndown from Ross Gunn of the Research Laboratory is definite. Even from mere reading of the letter, an experienced person would infer that it harks back to a decision sometime back.

Pressing the President now would only reinstate and seal that rejection by the only Service with any interest in science. -- A new approach is sine qua non.

A.S.

[Corresp. Teall]

Extract from

"EARLY HISTORY ATOMIC PROJECT IN RELATION TO PRESIDENT ROOSEVELT, 1939-1940"

By ALEXANDER SACHS

I. PRELUDE TO PRESENTATION OF PROJECT TO PRESIDENT ROOSEVELT

By Spring 1939, there was completed in the Physics Laboratories at Columbia University an advance upon the uranium researches in Europe of Drs. O. Hahn and L. Strassman and the supplementary researches of Drs. L. Meitner and R. Frisch, which, in turn, had corresponded to independent work in this country by Prof. Enrico Fermi at Columbia and others (the European work was reported, respectively, in "Naturwiss," No. 27, 11 (1939), and "Nature" (Feb. 1939)).

The advances made by the collaborative work of Drs. Fermi and Leo Szilard were summarized in a communication entitled, "Instantaneous Emission of Fast Neutrons in the Interaction of Slow Neutrons with Uranium," dated March 16, 1939, from the Pupin Physics Laboratories of Columbia University, which was published in "The Physical Review" of April 15, 1939.

The receipt and study by Dr. Einstein at the Institute for Advanced Study in Princeton of reports of these researches of Drs. Fermi and Szilard led him to confer, first, with Dr. Szilard, and then, directly and indirectly, with the writer, on the implications of atomic disintegration for the world situation in the then and emerging phases. It so happens that the last mentioned, as an economist and political scientist, had already been known for his "Cassandra forebodings" on the significance of the European developments dating back to the Great Depression; and just prior to the Nazi seizure of Prague, he had prepared a memorandum entitled "Notes on Imminence World War in Perspective Accrued Errors and Cultural Crisis of the Inter-War Decades" (included herein as Exhibit 1). The concluding note of the memorandum emphasized that "preparedness has and will become more and more urgent for all members of Western civilization

as a result of the past errors committed and in the course of the prospective unfolding aggressions of Nazi Germany." Sharing these views, Dr. Einstein connected the experimental work on uranium carried on throughout the world with certain actions of the German Government since the seizure of Prague on the 15th of March, 1939. For one thing, Germany had stopped the sale of uranium from the Czechoslovakian mines, and for another, there was feverish activity at the "Kaiser Wilhelm Institut" in Berlin in the direction of repeating and trying to surpass the work on uranium that was proceeding and under our system being published by a number of refugee scientists largely from Germany who had found haven and scope for work in democratic countries.

Congress - Nat. Def.
Comm 8/10/45

October 11, 1939

Dear Mr. President:

With approaching fulfillment of your plans in connection with revision of the Neutrality Act, I trust that you may now be able to accord me the opportunity to present a communication from Dr. Albert Einstein to you and other relevant material bearing on experimental work by physicists with far-reaching significance for National Defense.

Briefly, the experimentation that has been going on for half a dozen years on atomic disintegration has culminated this year (a) in the discovery by Dr. Leo Szilard and Professor Fermi that the element, uranium, could be split by neutrons and (b) in the opening up of the probability of chain reactions, - that is, that in this nuclear process uranium itself may emit neutrons. This new development in physics holds out the following prospects:

1. The creation of a new source of energy which might be utilized for purposes of power production;

2. The liberation from such chain reaction of new radio-active elements, so that tons rather than grams of radium could be made available in the medical field;

3. The construction, as an eventual probability, of bombs of hitherto unenvisaged potency and scope: As Dr. Einstein observes, in the letter which I will leave with you, "a single bomb of this type carried by boat and exploded in a port might well destroy the whole port together with some of the surrounding territory!"

In connection, then, with the practical importance of this work - for power, healing and national defense purposes - it needs to be borne in mind that our supplies of uranium are limited and poor in quality as compared with the large sources of excellent uranium in the Belgian Congo and, next in line, Canada and former Czechoslovakia. It has come to the attention of Dr. Einstein and the rest of the group concerned with this problem that Germany has actually stopped the sale of uranium from the Czechoslovakian mines it seized. This action must be related to the fact that the son of the German Under-Secretary of State, Karl von Weizsaecker, had been an assistant at the Kaiser Wilhelm Institute in Berlin to some of the great physicists now resident in this country who are carrying forward these experiments on uranium.

Mindful of the implications of all this for democracy and civilization in the historic struggle against the totalitarianism that has exploited the inventions of the free human spirit, Dr. Szilard, in consultation with Professor E. P. Wigner, head of the physics department of Princeton, and Professor E. Teller of George Washington University, sought to aid this work in the United States through the formation of an association for scientific collaboration, to intensify the cooperation of physicists in the democratic countries - such as Professor Joliot in Paris, Professor Lindemann of Oxford and Dr. Dirac of Cambridge - and to withhold publication of the progress in the work on chain reactions. As the international crisis developed this summer, these refugee scholars and the rest of us in consultation with them unanimously agreed that it was their duty, as well as desire, to apprise you at the earliest opportunity of their work and to enlist your cooperation.

In view of the danger of German invasion of Belgium, it becomes urgent to make arrangements - preferably through diplomatic channels - with the Union Miniere du Haut-Katonga, whose head office is at Brussels, to make available abundant supplies of uranium to the United States. In addition, it is necessary to enlarge and accelerate the experimental work, which can no longer be carried out within the limited budgets of the departments of theoretical physics in our universities. It is believed that public-spirited executives in our leading chemical and electrical companies could be persuaded to make available certain amounts of uranium oxide and quantities of graphite, and to bear the considerable expense of the newer phases of the experimentation. An alternative plan would be the enlistment of one of the foundations to supply the necessary materials and funds. For either plan and for all the purposes, it would seem advisable to adopt the suggestion of Dr. Einstein that you designate an individual and a committee to serve as a liaison between the scientists and the Executive Departments.

In the light of the foregoing, I desire to be able to convey in person, in behalf of these refugee scholars, a sense of their eagerness to serve the nation that has afforded them hospitality, and to present Dr. Einstein's letter together with a memorandum which Dr. Szilard prepared after some discussion with me and copies of some of the articles that have appeared in scientific journals. In addition, I would request in their behalf a conference with you in order to lay down the lines of policy with respect to the Belgian source of supply and to arrange for a continuous liaison with the Administration and the Army and Navy Departments, as well as to solve the immediate problems of necessary materials and funds.

With high regard,

Yours sincerely,

ALEXANDER SACHS

The President,
The White House,
Washington, D. C.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. They cannot undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*

NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR ON P. 208.

CORRESPONDENTS ARE INVITED TO ATTACH SIMILAR SUMMARIES TO THEIR COMMUNICATIONS.

Nuclear Reactions in the Continuous Energy Region

It is typical for nuclear reactions initiated by collisions or radiation that they may, to a large extent, be considered as taking place in two steps: the formation of a highly excited compound system and its subsequent disintegration or radiative transition to a less excited state. We denote by A, B, . . . the possible alternative products of the reaction, specified by the nature, internal quantum state, and spin direction both of the emitted particle or photon and of the residual nucleus and the orbital momentum. Further, we call P_A, P_B . . . the probabilities, per unit time, of transitions to A, B, . . . respectively, from the compound state.

The cross-section for the reaction $A \rightarrow B$ is then evidently

$$\sigma_B^A = \sigma^A \frac{P_B}{P_A + P_B + \cdots}, \qquad (1)$$

where σ^A is the cross-section for a collision in which, starting from the state A, a compound nucleus is produced. This formula implies, of course, that we are dealing with energies for which the compound nucleus can actually exist, that is, that we are either in a region of continuous energy values or, if the levels are discrete, that we are at optimum resonance. Moreover, it is assumed that all possible reactions, including scattering, proceed by way of the compound state, neglecting, in particular, the influence of the so-called 'potential scattering', where the particle is deflected without actually getting into close interaction with the individual constituents of the original nucleus.

On these assumptions a very general conservation theorem of wave mechanics[1] yields the relation

$$\sigma^A = \frac{\lambda^2}{\pi}(2l+1)\frac{P_A}{P_A + P_B + \cdots}, \qquad (2)$$

where λ is the wave-length of the incident particle and l is the angular momentum.

In the case of discrete levels, (1) and (2) give the same cross-section as the usual dispersion formula, if one applies it to the centre of a resonance level and neglects the influence of all other levels. In this case we have for each resonance level a well-defined quantum state of the compound nucleus, and its properties, in particular the probabilities P_A, P_B, . . . then cannot depend on the kind of collision by which it has been formed, that is, they would be the same if we had started from the fragments B, or C, . . . instead of A.

In the case of the continuum, however, where there are many quantum states with energies that are indistinguishable within the life-time of the compound nucleus, the actual state of the system is a superposition of several quantum states and its properties depend on their phase relations, and hence on the process by which the compound nucleus has been produced.

This dependence is made particularly obvious if we consider the formula

$$\overline{\sigma^A} = \frac{\hbar}{2}\rho\lambda^2 (2l+1) P_A^a, \qquad (3)$$

for the mean value of the cross-section over an interval containing many levels, which follows from the well-known considerations of detailed balancing. Here ρ is the density per unit energy of levels (of suitable angular momentum and symmetry) of the compound nucleus. P_A^a is the probability for process A in statistical equilibrium and thus refers to a microcanonical ensemble of compound states built up from the fragments A, B . . . respectively, with proper statistical weights.

In the case of discrete levels, where formula (3) can also be derived directly from the dispersion formula, P_A^a is simply an average over the individual levels of the probability P_A, which in this case is well defined.

In the continuum, (3) must be identical with (2) since the cross-section does not vary appreciably over an energy interval containing many levels, and hence, comparing (2) and (3)

$$\frac{P_A^{(A)}}{P_A^a} = \frac{\pi}{2}\hbar\rho\,(P_A^{(A)} + P_B^{(A)} + \ldots) = \frac{\pi}{2}\frac{\Gamma^{(A)}}{d}, \qquad (4)$$

where the superscript A has been added to the probabilities occurring in (1) in order to show explicitly the dependence on the mode of formation, and where $\Gamma^{(A)}$ is the total energy width of the compound state concerned and $d = \frac{1}{\rho}$ the average level distance. In the continuum, where $\Gamma^{(A)} \gg d$, the probability $P^{(A)}$ of re-emitting the incident particle without change of state of the nucleus will thus be much larger than the probability of the same process in a compound nucleus produced in other ways.

While the arguments used so far are of a very general character, more detailed considerations on the mechanism of nuclear excitation are required for a discussion of the dependence $P_B^{(A)}$ of the mode A of the compound nucleus provided $A = B$.

One can think of cases in which such a dependence must obviously be expected; in fact, if a large system be hit by a fast particle, the energy of excitation might be localized in the neighbourhood of the point of impact, and the escape of fast particles from this neighbourhood may be more probable than in statistical equilibrium. Further, if the system has modes of vibration very loosely coupled, the excitation of one of them, for example by radiation, would be unlikely to lead to the excitation of a state of vibration made up of very different normal modes, even though the state may be quite strongly represented in statistical equilibrium.

In actual nuclei, however, the motion cannot be described in terms of loosely coupled vibrations, no

would one expect localization of the excitation energy to be of importance in nuclear reactions of moderate energy. If we suppose that there are no other special circumstances which would lead to a dependence of $P_B^{(A)}$ on A, it is thus a reasonable idealization to assume that, even in the continuum, all $P_A^{(A)}$ are equal to P_B^0, except, of course, for $A = B$, where we have seen in (4) that the phases are necessarily such as to favour the re-emission of the incident particle.

A typical case of a reaction in the continuum is the nuclear photo-effect in heavy elements, produced by γ-rays of about 17 mv. In the first experiments of Bothe and Gentner, there seemed to be marked differences between the cross-sections of different elements, but the continuation of their investigations[2] indicated that these differences can be accounted for by the different radioactive properties of the residual nuclei, and that the cross-sections of all heavy nuclei for photo-effect are of the order of 5×10^{-26} cm.^{2}.

In previous discussions, based on formulæ (1) and (2), where the distinction between $P_A^{(A)}$ and P_A^0 was not clearly recognized, it was found difficult, however, to account for photo-effect cross-sections of this magnitude. In fact, if one estimates the probability of neutron escape P_B at about 10^{17} sec.$^{-1}$, one should have for P_A 10^{15} sec.$^{-1}$, and as long as this was taken as P_A^0 it seemed much too large, since it evidently must be much smaller than the total radiation probability, estimated at about 10^{15}, which included transitions to many more final levels besides the ground state.

We see now, however, that $P^{(A)}$ is here considerably larger than P_A^0, since the level distance at the high excitations concerned is probably of the order of 1 volt, whereas the level width corresponding to the above value of P_B is about 100 volts. From (4), or more directly from (3), P_A^0 is thus seen to be only about 10^{13} sec.$^{-1}$, which would appear quite reasonable.

N. BOHR.
R. PEIERLS.
G. PLACZEK.

Institute of Theoretical Physics,
Copenhagen.
July 4.

[1] The details of this and of the other arguments of this note will be published in the *Proceedings of the Copenhagen Academy*.
[2] Bothe, W., and Gentner, W., *Z. Phys.*, **106**, 236 (1937); **112**, 45 (1939).

The Scattering by Uranium Nuclei of Fast Neutrons and the Possible Neutron Emission Resulting from Fission

THE work to be described concerns only fast neutrons, and its object is the study of their scattering by uranium and the possible neutron emission which accompanies the fission of the nucleus.

The experiments were performed with a polonium plus beryllium source equivalent to 3mC. of radon plus beryllium. An ionization chamber surrounded with 2·5 cm. lead, filled with hydrogen at a pressure of 35 atm., was used as a neutron detector. The insulated electrode was connected to a compensated electrometer valve[1], the grid leak being 10^{11} ohms and the sensitivity $1{\cdot}2 \times 10^{-15}$ amp./div. on the scale.

We have employed two experimental arrangements in which the source was placed (1) between the chamber and the substance used as scatterer, the nature and the thickness of which were variable; (2) in the centre of a cube of 16 cm. side, alternately filled with uranium oxide (specific gravity, $d = 4{\cdot}0$) and lead oxide (compressed to $d = 3{\cdot}8$).

The first type of experiment gave us the total scattering cross-section, which is, as can be shown, $\sigma_t = \sigma_e + k_i\sigma_i$; for uranium oxide $\sigma_t = \sigma_e + k_i\sigma_i + k_r\nu_r\sigma_r$, where σ_e, σ_i, σ_r are respectively the average cross-sections of elastic and inelastic scattering and of fission; ν_r is the average number of neutrons produced per fission; k_i and k_r are the average efficiency factors of the chamber for the neutrons having undergone an inelastic collision or for the neutrons resulting from fission. The efficiency for the direct neutrons was taken to be unity, $k = 1$. For neutrons elastically scattered by nuclei of sufficiently high mass, $k_e = k = 1$. We have calculated k, taking into account the size of the chamber, the cross-section for proton projection, etc. The spectrum of polonium plus beryllium neutrons has been considered[2] to contain 50 per cent of neutrons of W_n less than 10^6 ev. We thus obtain:

$10^{-6}\,W_n$	0·1	0·5	3	5	10	ev.
k	0·3	1	1·9	1·7	1·2	

In view of a possible extrapolation that would give $\sigma_e + k_i\sigma_i$ for uranium, we have in the same way experimented with scattering by lead oxide, lead, copper and zinc.

The results of the first experiment were as follows:

Substance	Cu	Zn	Pb	PbO_2	UO_2	(O)calc.	(U)calc.
$\sigma_t \times 10^{24}$ cm.^{2} (± 10%)	2·2	2·3	5·4	9·5	14·4	2	10·3

The values for uranium and oxygen are calculated on the assumption of the additivity of the cross-sections in lead oxide and uranium oxide.

The second experiment gives us, in the first approximation, the absorption coefficient $(1-k_i)\sigma_i + (1-k_r\nu_r)\sigma_r$, the value of σ_e being only as a correction term in the determination of the mean free path λ and the average distance L travelled by the neutrons before they escape from the whole mass, which is supposed spherical, the radius being r and $L = r\left(1 + \frac{1}{2}\frac{r}{\lambda}\right)$. This experiment, taking into account the results of the previous experiments, gives for lead, $(1-k_i)\sigma_i \simeq 2 \times 10^{-24}$ cm.^{2}. Assuming that σ_i can reach 30 per cent of σ_e[3], this gives $k_i\ (\simeq)\ 0$.

With the exception of uranium, for which one must consider not only σ_i, but also $\nu_r\sigma_r$, it is probable that σ_t is not very different from σ_e because of the small value of k_i.

In the case of uranium, however, we have,

$$(1-k_i)\sigma_i + (1-k_r\nu_r)\sigma_r \simeq 0{\cdot}9 \times 10^{-24}\ \text{cm.}^2 \qquad (1),$$

or, by adding to σ_t, thus eliminating k_i and k_r,

$$\sigma_e + \sigma_i + \sigma_r \simeq 11{\cdot}2 \times 10^{-24}\ \text{cm.}^2. \qquad (2).$$

If it is supposed that each fission produces radio-elements, the cross-section measured by Joliot, and by Anderson *et al.*[4] would be identical with σ_r, which they found to be $\sigma_r \simeq 10^{-25}$ cm.^{2}. In this case we see that $(\sigma_e + \sigma_i)$ is much greater ($\simeq 11{\cdot}1 \times 10^{-24}$ cm.^{2}) than that given by an extrapolation ($\simeq 6 \times 10^{-24}$ cm.^{2}).

On the other hand, it results from (1) that, if the value of σ_i is comparable to that of the next elements (1 to 2×10^{-24} cm.^{2}), ν_r can, with plausible assumptions as to the coefficients k_i and k_r, take variable values, for example, from 1 to 5, or even more.

One can see that, so long as σ_i is not determined separately, the experiments of the type described do not allow us to determine ν_r and σ_r (characteristics of the fission), or to conclude that neutrons are

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Emission of Neutrons by Uranium*

W. H. ZINN, *City College, The College of the City of New York, New York*

LEO SZILARD, *Pupin Physics Laboratories, Columbia University, New York, New York*

(Received August 14, 1939)

Fast neutrons emitted by uranium under the action of thermal neutrons were studied by using a radium-beryllium photoneutron source. The background due to the primary neutrons can be neglected since only a few of the photoneutrons are sufficiently fast to be counted. Data are obtained concerning the energy spectrum of the uranium fission neutrons by recording photographically by means of a linear amplifier and cathode-ray oscillograph the pulses due to helium atoms projected in an ionization chamber. Visual inspection of the record gives an upper limit of the spectrum of 3.5 Mev. The number of neutrons emitted is estimated by analyzing the pulse distribution of hydrogen atoms projected by uranium neutrons in an ionization chamber filled with hydrogen and argon. The number found is brought into relationship with the number of fissions, observed under comparable conditions, in an ionization chamber lined with a thin film of uranium oxide containing a known amount of uranium. In this way it is found that about 2.3 neutrons are emitted per fission. The method used would permit a greater accuracy in the determination of this number than the actual accuracy obtained in the present experiments. This number, together with the fission cross section and the cross section for radiative capture gives the number of neutrons produced for each thermal neutron absorbed in uranium.

WE reported[1] some time ago that fast neutrons are emitted—apparently instantaneously—from uranium under the action of thermal neutrons and we found, as a rough estimate, an average of two neutrons per fission process. This result was obtained by counting the helium recoil nuclei which the fission neutrons project in a helium-filled ionization chamber. The emission of neutrons in the fission of uranium was independently discovered by von Halban, Joliot and Kowarski[2] as well as by Anderson, Fermi and Hanstein,[3] who observed an increase of the thermal neutron density in water in the presence of uranium. Others[4] have investigated the same phenomenon.

Before this "instantaneous" emission had been observed, Roberts, Meyer and Wang[5] discovered a delayed emission of neutrons from uranium which apparently follows a beta-transformation of a half-life period of twelve seconds. We had found that the instantaneous emission was very much stronger than the delayed emission and we assumed that it corresponds to a direct ejection of neutrons from the uranium fragments, without being preceded by a beta-transformation, and that accordingly the time delay involved is far too small to be measured by the usual

* Publication assisted by the Ernest Kempton Adams Fund for Physical Research of Columbia University.

[1] L. Szilard and W. H. Zinn, Phys. Rev. **55**, 799 (1939).

[2] H. von Halban, F. Joliot and L. Kowarski, Nature **143**, 470 (1939).

[3] H. L. Anderson, E. Fermi and H. B. Hanstein, Phys. Rev. **55**, 797 (1939).

[4] G. P. Thomson, J. L. Michiels and G. Parry, Nature **143**, 760 (1939); G. von Droste and H. Reddeman, Nat. Wiss. **20/21**, 371 (1939).

[5] R. B. Roberts, R. C. Meyer and P. Wang, Phys. Rev. **55**, 510 (1939).

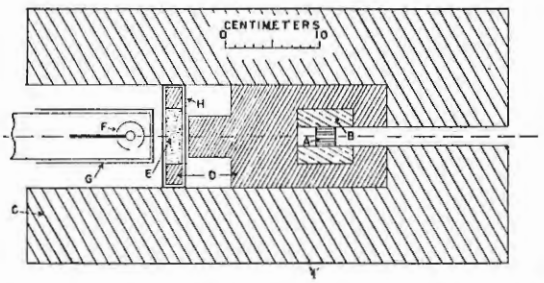


FIG. 1. Arrangement for the observation of the emission of fast neutrons from uranium. *A*—radium; *B*—beryllium block; *C*—paraffin wax; *D*—lead; *E*—uranium cell; *F*—spherical ionization chamber; *G*—cadmium sheet cap; *H*—cadmium sheet shield.

methods. This assumption was based on the arguments that it would be very difficult to explain the great abundance of the instantaneous neutron emission without assuming direct ejection and that no hard beta-rays were observed which should be expected to be present if the neutron emission followed a very short-lived beta-transformation. From direct experimental evidence, however, we could not exclude a delay smaller than one-tenth of a second. Gibbs and Thomson[6] have now shown by direct experiments that the delay is smaller than one-thousandth of a second and this appears to leave little doubt as to a direct ejection of neutrons.

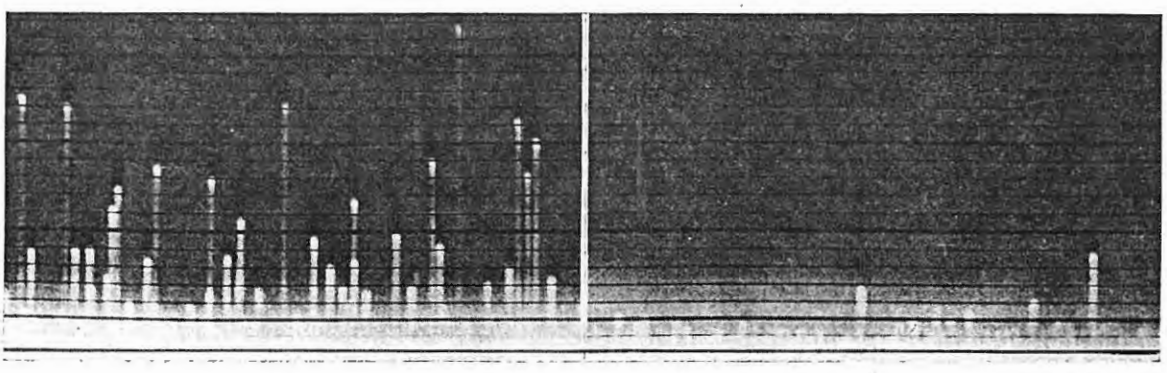
In the present experiments helium recoils were used for investigating the energy distribution of the fission neutrons, but hydrogen recoils were used for estimating the number emitted per fission.

The experimental arrangement is shown in Fig. 1. The source of thermal neutrons was about one gram of radium, *A*, placed in the center of a beryllium block, *B*, and surrounded by a paraffin cylinder, *C*. Fast neutrons emitted under the action of the thermal neutrons by about 430 grams of uranium metal enclosed in the cell *E*, were detected by the spherical ionization chamber, *F*. The pulses from the chamber were fed into a linear amplifier and were made visible by means of a cathode-ray oscillograph. A camera with a moving film was used to obtain a photographic record of the pulses appearing on the oscillograph screen.

Two such records are shown in Fig. 2; one was obtained in the absence and the other in the presence of the cadmium sheet shield, *H*. The shield *H* completely surrounds the uranium and shuts it off from most of the thermal neutrons, leaving a background of pulses which is partly due to particularly fast photoneutrons from the source, and partly due to fission neutrons from the uranium emitted under the action of the few thermal neutrons which pass through the cadmium shield. This background amounts to less than one pulse per minute.

The ionization chamber, which was filled with 10 atmospheres of hydrogen and 8 atmospheres of argon, contained a small amount of nitrogen. By removing the cadmium cap, *G*, from the chamber the nitrogen atoms in the chamber can be exposed to the action of thermal neutrons and

[6] D. F. Gibbs and G. P. Thomson, Nature **144**, 202 (1939).



(*a*) (*b*)

FIG. 2. (*a*) Oscillograph record of the fast neutrons from uranium. Cadmium sheet shield, *H* of Fig. 1, absent. Thermal neutrons falling on the uranium in the cell *E*. Ionization chamber filled with 10 atmospheres of hydrogen and 8 atmospheres of argon. (*b*) Record obtained with the cadmium shield *H* shutting off the thermal neutrons from the cell *E*.

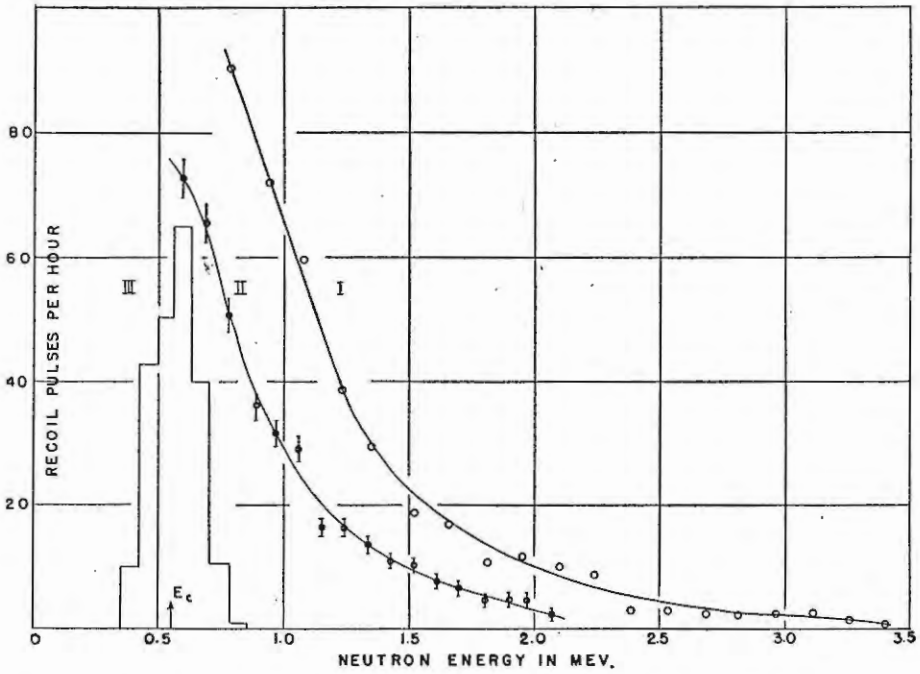


FIG. 3. Curve I: Pulse distribution due to helium recoils. Ionization chamber filled with 10 atmospheres of helium and 10 atmospheres of argon. Curve II: Pulse distribution $P(E)$ due to hydrogen recoils. Ionization chamber filled with 10 atmospheres of hydrogen and 8 atmospheres of argon. Curve III: Pulse distribution due to protons emitted, under the action of thermal neutrons, by a small amount of nitrogen in the chamber filled with 10 atmospheres of helium and 10 atmospheres of argon.

will then emit protons of about 0.6 Mev energy.[7] The pulses due to these protons were recorded and their distribution is shown in curve III of Fig. 3. This curve shows a sharp maximum which should correspond to an energy of about 0.6 Mev, and therefore this curve was used for calibrating the ionization chamber.

In order to find from the observed number of hydrogen recoils the number of neutrons which pass through the chamber it is necessary to know something about the energy distribution of the fission neutrons. This knowledge is required for two reasons. First, the scattering cross section of hydrogen is a function of the neutron energy; secondly, the observed pulse distribution of the hydrogen recoils is cut off at a certain energy E_c, which in this case was 0.55 Mev, in order to avoid the counting of pulses in the region which is affected by the gamma-ray background. Neutrons which have an energy below this cut-off energy, E_c, do not contribute to the recorded pulse distribution and their number has to be determined from the shape of the neutron spectrum, provided this spectrum is known.

If the time required for the collection of ions in the chamber were short compared with the time constant of the amplifier, the size of the pulses recorded by the oscillograph might be considered a fair measure of the energy which the recoil proton loses in the chamber. Even so, the size of the pulses cannot be considered a measure of the initial energy of the recoil protons if these lose only part of their energy in the chamber and are stopped by the walls. Therefore, if $R(E)dE$ is the number of recoil protons having an initial energy between E and $E+dE$, and if $P(E)dE$ is the number of recoil protons which lose in the chamber an amount of energy between E and $E+dE$, these two functions will be rather different in the high energy region where the range of the recoil protons cannot be neglected in comparison with the linear dimensions of the chamber. For the hydrogen-argon filled chamber

[7] J. Chadwick and M. Goldhaber, Proc. Camb. Phil. Soc. **31**, 612 (1935); T. W. Bonner and W. M. Brubaker, Phys. Rev. **49**, 778 (1936); M. S. Livingston and H. A. Bethe, Rev. Mod. Phys. **9**, 344 (1937).

which was used the two functions can be expected to coincide very nearly in the region of the cut-off energy E_c, and can be expected to differ widely for energies above 1.5 Mev.

For this reason helium recoils (which have about $\frac{1}{10}$ the range of recoil protons for equal neutron energies) had to be used instead of hydrogen recoils in order to find the upper end of the energy spectrum of the fission neutrons. Curve I, in Fig. 3, shows the pulse distribution of helium recoils obtained with 10 atmospheres of helium and 10 atmospheres of argon in the chamber. This curve shows that the spectrum of the fission neutrons extends to about 3.5 Mev. Though the existence of a small number of high energy neutrons such as reported by von Halban, Joliot and Kowarski,[8] is not inconsistent with our result, the number of neutrons having energies above 4 Mev appears to be too small to have much bearing on our estimate of the total number of fission neutrons.

Since the calibration of the chamber, which we performed by means of protons, is not entirely satisfactory for correlating the size of the pulses due to helium recoils witb the energy of the helium recoils, the helium-argon filled chamber was also calibrated by means of D+D neutrons of 2.5 Mev energy. The two calibrations coincided within the limits of the experimental error.

An estimate of the number of fission neutrons should be based on a count of hydrogen recoils rather than helium recoils since the scattering cross section of helium has a sharp maximum[9] for neutrons of about 1.0 Mev energy, and helium is therefore not suitable for the purposes of a quantitative estimate. It can be shown that, if the neutron-proton scattering is spherically symmetrical in the system of the center of gravity, the number of neutrons $N(E)dE$ which pass through the chamber, and which have an energy between E and $E+dE$, is given by:

$$N(E)=-\frac{E}{H\sigma(E)}\frac{dR(E)}{dE},$$

where $\sigma(E)$ is the scattering cross section of the proton and H is the number of hydrogen atoms in the ionization chamber. From this we derive, for the total number of neutrons, N, passing through the chamber:

$$N=\int_0^\infty N(E)dE=\frac{\alpha}{\sigma_{Av}H}\left[E_cR(E_c)+\int_{E_c}^\infty R(E)dE\right],$$

where α is the ratio of the total number of neutrons to the number of neutrons which have an energy in excess of E_c, and σ_{Av} is an average scattering cross section of the proton, the value of which has to be determined from the energy spectrum of the neutrons. Since we have:

$$\int_0^\infty R(E)dE=\int_0^\infty P(E)dE$$

and since, for the reasons stated above, we have with good approximation:

$$R(E)\backsimeq P(E) \quad \text{for} \quad E\leq E_c,$$

we can express N in terms of $P(E)$ instead of $R(E)$. We then have:

$$N=\frac{\alpha}{\sigma_{Av}H}\left[E_cP(E_c)+\int_{E_c}^\infty P(E)dE\right].$$

Let it now be assumed for the sake of argument that all the neutrons are emitted from a moving uranium fragment which has a mass number of about 120 and a kinetic energy of about 100 Mev. If all the neutrons were emitted from such a moving fragment with a single energy E_0, the energy distribution of the neutrons in the

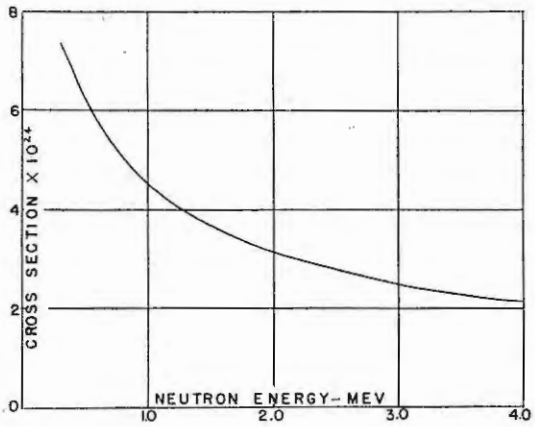


FIG. 4. Neutron-proton cross section as a function of neutron energy according to current theory.

[8] H. von Halban, F. Joliot and L. Kowarski, Nature **143**, 939 (1939).
[9] H. Staub and W. E. Stephens, Phys. Rev. **55**, 131 (1939).

laboratory reference system would stretch from:

$$E_{\min}=(0.9-E_0^{\frac{1}{2}})^2 \text{ Mev}$$

to:

$$E_{\max}=(0.9+E_0^{\frac{1}{2}})^2 \text{ Mev}.$$

It is easy to see that the neutrons should be uniformly distributed in this energy interval if their distribution is spherically symmetrical in the center of mass system. One obtains accordingly:

$$\alpha=(E_{\max}-E_{\min})/(E_{\max}-E_c)$$

and

$$\frac{1}{\sigma_{Av}}=\frac{1}{E_{\max}-E_c}\int_{E_c}^{E_{\max}}\frac{dE}{\sigma(E)}.$$

Using for $\sigma(E)$ the curve shown in Fig. 4 which has been theoretically derived,[10] the following values of α/σ_{Av} are obtained for various values of $E_{\max}$.

$E_{\max}$	2.0 Mev	3.0 Mev	4.0 Mev
α/σ_{Av}	$0.316\times10^{+24}$	$0.353\times10^{+24}$	$0.388\times10^{+24}$

The variation of α/σ_{Av} with $E_{\max}$ is so slight because of the manner in which both α and σ_{Av} decrease with rising $E_{\max}$.

The value of the expression:

$$E_cP(E_c)+\int_{E_c}^{\infty}P(E)dE$$

was found from the observed pulse distribution (curve II of Fig. 3) to be 13.7 pulses per minute. Since the number H of hydrogen atoms in the chamber was $H=6.9\times10^{+21}$ the number of neutrons passing through the chamber is

$$N=1.98\times10^{-21}(\alpha/\sigma_{Av}) \text{ per minute.}$$

If the cadmium cap G is removed the number of thermal neutrons reaching the uranium cell is increased and the number of fast neutrons passing through the ionization chamber is increased by the same factor. This factor was found to be 1.22 by filling the ionization chamber with pure hydrogen and then counting the hydrogen recoils giving rise to pulses above a certain arbitrarily set level, both in the presence and absence of the cadmium cap. Thus the number of neutrons N^* which pass through the chamber in the absence of the cadmium cap, G, is

$$N^*=2.415\times10^{-21}(\alpha/\sigma_{Av}) \text{ per minute.}$$

From N^* the number, K, of neutrons emitted per minute by the uranium was calculated by taking into account the geometrical factors, including the variation of the thermal neutron density within the uranium cell. K is thus found to be:

$$K=8.25\times10^{-19}(\alpha/\sigma_{Av}) \text{ per minute.}$$

In order to obtain the number of neutrons emitted per fission it is necessary to compare K with the number of fissions, L, which occur in the uranium under the conditions of this experiment. For this purpose both the ionization chamber and the uranium cell were removed and a parallel plate ionization chamber lined with a thick layer of uranium oxide was placed in the position previously occupied by the uranium cell. The number of fissions produced in this chamber was observed and found to be 19 per minute. The chamber was then calibrated by comparing the number of fissions obtained from the thick uranium oxide layer with the number of fissions obtained in the same chamber at the same thermal neutron intensity from a thin layer of uranium oxide containing 1.4 mg of uranium. The calibration was carried out by using a particularly strong neutron source, so as to obtain a sufficiently large number of counts from the thin layer. The ratio of the fission counts from the thick layer and from the thin layer was found to be 29.2, from which it is concluded that 196,000 fissions per minute should take place in the uranium cell containing 427.7 grams of uranium. This would be the number of fissions if the density of the thermal neutrons were not reduced in the uranium cell due to the absorption of such neutrons in uranium. We estimate that the average density of thermal neutrons within the cell is reduced by a factor of 0.715. The number of fissions L actually taking place within the cell is therefore

$$L=140{,}000 \text{ per minute.}$$

In order to estimate the reduction of the average thermal neutron density within the uranium cell leading to the factor of 0.715, we first explored the anisotropy of the thermal neutron radiation near the uranium cell by means

[10] J. Schwinger and E. Teller, Phys. Rev. 52, 286 (1937).

of a rhodium indicator, and then calculated the thermal neutron density within the uranium by assuming the distribution of thermal neutrons to be the same as would result from the superposition of two parallel thermal neutron beams, one directed away from the source and the other towards it, and having an intensity ratio of 3 to 1. We assume exponential absorption for these two beams within the uranium and an exponent corresponding to a half-value thickness in uranium of 14 g per cm^2.

The number of neutrons emitted per fission is

$$K/L=5.9\times10^{-24}(\alpha/\sigma_{Av}).$$

This number should be increased by perhaps 10 percent in order to correct for the fact that $P(E)$ does not exactly coincide with $R(E)$ even for $E\leq E_c$. The magnitude of this correction was estimated by comparing for D+D neutrons of 2.5 Mev energy the observed pulse distribution $P(E)$ with the calculated distribution $R(E)$ in the region of the low recoil energies. Making this correction one finds for ρ, the number of neutrons per fission

$$\rho=6.5\times10^{-24}(\alpha/\sigma_{Av}).$$

Using for α/σ_{Av} the value 0.353×10^{-24} which corresponds to $E_{max}=3$ Mev rather than to the actually observed upper limit of the fission neutron spectrum, one finds

$$\rho=2.3.$$

Since the fission neutrons hardly will be emitted with a single energy E_0, too high a value for α/σ_{Av} would be obtained if the observed value of the upper limit of the energy spectrum were used for E_{max}. In any case the error introduced by the uncertainty of the actual energy distribution of the fission neutrons should be small since one finds for

$$E_{max}=2 \text{ Mev} \quad \rho=2.0$$

and for

$$E_{max}=4 \text{ Mev} \quad \rho=2.5.$$

More serious, however, may be a number of experimental inaccuracies which might conceivably add up to give a considerable error.

The interest in the number of neutrons emitted per fission arose out of its obvious importance from the point of view of the possibility of nuclear chain reactions. At present we have the following set of values: number of neutrons per fission, 2.3; fission cross section,[11] 2.0×10^{-24} cm^2; cross section for radiative capture[12] 1.3 or 1.2×10^{-24} cm^2. According to these values, the number of neutrons emitted by uranium per thermal neutron absorbed should be 1.4, which agrees with the value of 1.5 recently obtained by another method by Anderson, Fermi and Szilard.[13] Too much significance should not be attributed to this agreement, since the values given above are subject to fairly wide experimental errors.

If required the present experiments could be repeated with greater accuracy since the method used is quite capable of being applied with greater precision. Moreover, it gives the number of neutrons per fission independently of the value of the fission cross section which enters into the method used by von Halban, Joliot and Kowarski. These authors report[14] a value of 3.5 ± 0.7 neutrons per fission.

It should be mentioned that it appears to be essential for the method presented here to work with a low background count. The background is due to the primary neutrons and can be kept small by using a photoneutron source. We did not find it possible to obtain quantitative results by using neutrons from radon-beryllium sources or from the D+D reaction on account of the high background count due to the primary neutrons.

We are indebted to Dr. G. N. Glasoe for suggesting the method of obtaining the photographic records and for much valuable advice in this connection, and to Dr. E. T. Booth for determining by means of an alpha-particle count the uranium content of the thin uranium sheet which we used for purposes of calibration. Also, we wish to thank the Department of Physics of Columbia University for the laboratory facilities placed at our disposal and the Association for Scientific Collaboration for enabling us to obtain the radium used in this experiment.

[11] H. Anderson, E. Booth, J. Dunning, E. Fermi, G. Glasoe and F. Slack, Phys. Rev. **55**, 511 (1939).
[12] H. v. Halban, L. Kowarski and P. Savitch, Comptes rendus **208**, 1396 (1939); H. L. Anderson and E. Fermi, Phys. Rev. **55**, 1106 (1939).
[13] H. L. Anderson, E. Fermi and L. Szilard, Phys. Rev. **56**, 284 (1939).
[14] H. von Halban, F. Joliot and L. Kowarski, Nature **143**, 680 (1939).

[Chron File July 1939]
Corresp - Nat Def
Corresp - GLL
Corresp - Szilard

REPLY IN DUPLICATE
AND REFERENCE TO

WILL BE APPRECIATED

NAVAL RESEARCH LABORATORY

ANACOSTIA STATION
WASHINGTON, D. C.

RG/ejh

10 July 1939

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

Dear Dr. Szilard:

The matter which we discussed at the Princeton meeting of the Physical Society has been carefully considered. As I indicated to you at that time, it seems almost impossible, in light of the restrictions which are imposed on Government contracts for services, to carry through any sort of an agreement that would be really helpful to you. I regret this situation but see no escape. We are anxious, however, to cooperate with you in every respect and appreciate your assistance on this important problem.

Very truly yours,

Ross Gunn

Ross Gunn,
Technical Adviser.

[Chron File
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(sgd) ROSS GUNN
Technical Adviser

Memorandum. Part I. to meeting of Oct. 21st 1939
from L. Szilard

To Dr. A. Sachs.

SUMMARY

Recent experimental work and calculations based on its results make it appear possible that in the immediate future a nuclear chain reaction might be set up under certain well specified conditions in a system composed of uranium oxide and graphite. In view of this and other possibilities it seems desirable

1. that it should be made the responsability of some person or persons to watch on behalf of the government the further development of this branch of research, so that the government should be at any time in the position of taking such action as it deems appropriate;

2. that some person or persons who have the confidence of the government should take upon themselves the task of furthering this branch of research, of insuring that it should not suffer from lack of facilities, and of preparing the ground for experiments on a large scale, which might become necessary.

Observations to the above.

The fairly large quantities of material, which might be required for performing large-scale experiments, might perhaps be secured, without drawing on existing funds, by enlisting the assistance of certain industrial firms in the U.S.A. and of the Union Minière du Haut Katanga. Most of the materialsrrequired are produced by large corporations who own uranium mines and would therefore directly benefit if the present development created a market for uranium. Some of these firms could be approached now with a view of obtaining the promise of their assistance.

THE POSSIBILITY OF A LARGE-SCALE EXPERIMENT IN THE IMMEDIATE FUTURE.

At present it appears quite possible that a nuclear chain reaction could be set up in a system composed of uranium oxide (or uranium metal) and graphite. The graphite would have to be piled up in a space of perhaps 4 x 4 x 4 metres and might weigh about 100 metric tons. Perhaps 10 to 20 tons of uranium oxide would have to be used, embedded in some such pile of graphite.

The probable success or failure of such a large-scale experiment can not be forecast at present with any degree of assurance. The properties of a system composed of uranium and graphite have been calculated independtently, for a homogeneous mixture by Fermi, and,for a lattice of spheres of uranium oxide, or uranium metal, embedded in graphite, by myself. The results of these two independent calculations are in reasonable agreement and show that the two arrangements have different properties. For instance, in the case of using a lattice of spheres a great advantage could be obtained by using uranium metal instead of uranium oxide, whereas in the case of the homogeneous mixture the use of uranium metal would be of no great advantage. In spite of these calculations, we cannot foretell with certainty whether or not a nuclear chain reaction can be maintained in such a system because the absorption cross-section of carbon for slow neutrons is not sufficient ly known.

In order to remove this incertainty Fermi and I have devised two different experiments by means of which the absorption cross-section of carbon, which is very small, could be measured. It is assumed that one of these experiments, or both of them, will be started at Columbia University as soon as the facilities required can be obtained.

If the absorption of carbon should turn out to be comparatively large we could conclude that the large-scale experiment is bound to fail, and in this case it need not be started. If the absorption of carbon should prove to be exceedingly small the large-scale experiment would appear to be very promising, and it can be assumed that everybody will then be in favor of starting it without delay.

Unfortunately, we must be also prepared to find an intermediate value for the carbon absorption. In this case a large-scale experiment will have to be performed in order to find out whether or not a nuclear chain reaction can be achieved with a combination of uranium and graphite. So we may have to make the experiment and risk its possible failure.

It should be borne in mind that a negative result of the large-scale experiment could also be of value by showing with certainty that a chain reaction cannot be achieved with simple means in the near future. Otherwise there remains an ever-present potential threat arising out of experiments on uranium, which are carried out in certain other countries. Therefore, in my personal opinion, a large-scale experiment ought to be performed unless the possibility of its succes can be excluded with reasonable assurance on the basis of experiments which are designed to determine the absorption of carbon, or other similar experiments which can be carried out on a moderately small scale.

RECOMMENDATIONS CONCERNING LARGE-SCALE EXPERIMENTS.

No expenses need be incurred in connection with large-scale experiments until the absorption of carbon has been measured. On the other hand, steps ought to be taken now in order to prepare the ground for a large-scale experiment, so that this can be started without delay

at the proper time. For instance, the possibility of converting uranium oxide into urenium metal ought to be explored. An attempt ought to be made to obtain a promise on the part of certain industrial corporations to supply at the proper time the quantities of the materials, which are required. If possible, these materials ought to be loaned without any financial consideration. Barring an accident in the case of a successful large-scale experiment, most of the materials used would remain unaffected and could be returned after the experiment is completed.

100 metric tons of graphite represent a value of about $ 33.000.- at the rate of 15 ¢ per pound. If a purer brand of graphite has to be used, which rates at 24 ¢ per lb. the value involved would be $ 53.000.-

20 metric tons of uranium oxide represent a value of $ 100.000.- at the rate of $ 2.50 per lb. If it need not be converted into uranium metal but can be used in the form of oxide in the large-scale experiment, this material could be kept pure and could be returned undamaged. It would be desirable to have up to 50 tons of uranium oxide readily available for experiments in the United States.

STATEMENT CONCERNING THE POTENTIAL ASSISTANCE OF THE UNION MINIERE DU HAUT KATANGA.

It would be of particular value to enlist the assistance of this Belgian corporation which is to some extent controlled by the Belgian Government. It appears to be the only corporation which could supply at short notice 20 metric tons of uranium oxide, and probably even 50 tons. I understand that the Managing Director, Mr. E. Sengier, is

on a short visit in American.

From conversations which Professor G.B. Pegram of Columbia University had with a representative of the Eldorado Gold Mines, Ltd. it appears that this Canadian corporation might be able to supply uranium oxide for our purposes at the rate of 1 ton per week. If the uranium oxide were to be bought rather than obtained as a gift or a loan, it might be secured from Canada probably just as easily as from Belgium. On the other hand, the Canadian corporation is rather small and can hardly be asked to give away large quantities of material without financial compensation.

So far, radium up to about 2.5 gms was used in our experiments, and we had to pay a high rent to a subsidiary of the Union Minière, the only corporation from which large quantities of radium can be readily rented in this country. An attempt ought to be made to obtain radium for the purposes of such experiments rent-free from the Union Minière in the future.

Carnotites containing uranium are mined in the U.S.A. by the U.S. Vanadium Corporation which is owned by the Union Carbon and Carbide Company. A conversation which I recently had with William F. Barrett, Vice-President of this corporation, did not encourage the hope of obtaining large quantities of uranium oxide from this firm, but the issue could perhaps be reopened.

STATEMENT ABOUT URANIUM ORE.

As far as I was able to find out, pitchblend, which is an ore rich in uranium, is mined in Czechoslovakia, Canada and Belgian Congo. The total content of uranium in the deposit in Czechoslovakia is estimated

to be between 1000 and 1500 tons. The Canadian deposit visibly contains a total of 3000 tons. The amount of pitchblend in the Belgian Congo is not known, but it is believed to be very much larger. In the United States uranium occurs chiefly in the form of carnotites, which is an ore poor in uranium, and is mined for the sake of its vanadium content. The total deposit is estimated to contain 3000 tons of uranium oxide. (Perhaps there are in the United States larger quantities of ore containing a very small amount of uranium which are not included in the above estimate).

RECOMMENDATION CONCERNING URANIUM ORE.

Steps to secure a stock of uranium ores for the government can hardly be recommended at the present time if such steps would involve financial commitments on the part of the government. It might, however, be advisable to begin to study the question in what manner the government could secure such a stock at a later date if required.

For instance, the question has been raised whether it might not be possible to obtain for the government a large quantity of pitchblend from Belgium as a token reparation payment. Such a transaction would not cause alarm abroad if it were arranged before the world learns of the results of some successful large-scale experiment. The transaction could be justified without reference to the uranium content of the ore. Pitchblend is also the ore of radium, and action could be taken on the ground of securing the ore for the sake of its radium content, with a view of extracting the radium at some future date for medical purposes. Action taken on this ground alone might in fact be entirely justified.

Comes - Nat Sci.

On print.

EMISSION OF NEUTRONS BY URANIUM

W.H. Zinn, City College, The College of the
City of New York, New York.

Leo Szilard, Columbia University, New York, New York.

We reported[1] some time ago that fast neutrons are emitted -- apparently instantaneously -- from uranium under the action of thermal neutrons and we found, as a rough estimate, an average of two neutrons per fission process. This result was obtained by counting the helium recoil nuclei which the fission neutrons project in a helium-filled ionization chamber. The emission of neutrons in the fission of uranium was independently discovered by von Halban, Joliot and Kowarski[2] as well as by Anderson, Fermi and Hanstein[3], who observed an increase of the thermal neutron density in water in the presence of uranium. Others[4] have investigated the same phenomenon.

Before this "instantaneous" emission had been observed, Roberts, Meyer and Wang[5] discovered a delayed emission of neutrons from uranium which apparently follows a beta transformation of a half-life period of twelve seconds. We had found that the instantaneous emission was very much stronger than the delayed emission and we assumed that it corresponds to a direct evaporation of neutrons from the uranium fragments, without being preceded by a beta transformation, and that

accordingly the time delay involved is far too small to be measured by the usual methods.This assumption was based on the arguments that it would be very difficult to explainthe reat abundance of the instantaneous neutron emission without assuming direct evaporation and that no hard beta rays were observed which should be expected if the neutron emission followed a very short - lived beta transformation.From direct experimental evidence,however,we could not exclude a delay smaller than one-tenth of a second.Gibbs and Thomson (6) have now shown by direct experiments that the delay is smaller than one- thousandth of a second and this appears to leave little doubt as to a direct evaporation of neutrons.

In the present experiments helium recoils were used for investigating the energy distribution of the fission neutrons,but hydrogen recoils were used for estimating the number emitted per fission.

The experimental arrangement is shown in Fig.1. The source of thermal neutrons was about one gram of radium, A, placed in the center of a beryllium,B,and surrounded by a block paraffin cylinder,C. Fast neutrons emitted under the action of the thermal neutrons by about 430 grams of uranium metal endlosed in the cell E,were detected by the sphercal ionization chamber,F.The pulses from the chamber were ~~found~~ fed into a linear amplifier and were made visible by means of a

cathode-ray oscillograph.A camera with a moving film was used to obtain a photographic record of the pulses appearing on the oscillograph screen.

Two such records are shown in Fig.2;one was obtained in the absence and the other in the presence of the cadmium sheet shield,H. The shield H,if present,shuts off the uranium from most of the thermal neutrons,leaving a background of pulses which is partly due to particularly fast photo-neutrons from the source,and partly due to fission neutrons from the uranium emitted under the action of the few thermal neutrons which pass through the cadmium shield.This background amounts to less than one pulse per minute.

The ionization chamber which was filled with 10 atmospheres of hydrogen and 8 atmospheres of argon contained a small amount of nitrogen.By removing the cadmium cap,G,from the chamber the nitrogen atoms in the chamber can be exposed to the action of thermal neutrons and will then emit protons of about .6 Mev energy[7]. The pulses due to these protons were recorded and their distribution is shown in curve III of Fig.3. This curve shows a sharp maximumwhich should correspond to an energy of about 0.6 Mev,and therefore this curve was used for calibrating the ionization chamber.

In order to find from the observed number of hydrogen recoils the number of neutrons which pass through the chamber it is necessary to know something about the energy distribution of the fission neutrons.This knowledge is required for two reasons.First,the scattering cross- section of hydrogen is a function of the neutron energy;secondly,the observed pulse

distribution of hte hydrogen recoils is cut off at a certain energy E_c, which in this case was 0.55 Mev, in order to avoid the counting of pulses in the region which is affected by the gamma -ray background. Neutrons which have an energy below this cut-off energy, E_c, do not contribute to the recorded pulse distribution and their number has to be determined from the shape of the neutron spectrum, provided this spectrum is known.

If the time required for the collectiom of ions in the chamber were short compared with the time constant of the amplifier, the size of the pulses recorded by the oscillograph might be considered a fair measure of the energy which the recoil proton loses in the chamber. Even so, the sizes of the pulses they cannot be considered a measure of the initial energy of the recoil protons if these lose only part of their energy in the chamber and are stopped by the walls. Therefore, if $R(E)dE$ is the number of recoil protons having an initial energy between E and E dE, and if $P(E)dE$ is the number of recoil protons which lose in the chamber an amount of energy between E and E dE, these two functionswill be rather different in the high energy region where the range of the recoil protons cannot be neglectedin comparison with the linear dimensions of the chamber. For the hydrogen-argon filled chamber which was used the two functions can be expected to coincide very nearly in the region of the cut-off energy Ec, and can be expected to differ widely for energies above 1.5 Mev.

For this reason helium recoils (which have about 1/10 the range of recoil protons for equal neutron energies) had to be used instead of hydrogen recoils in order to find the upper end of the energy spectrum of the fission neutrons. Curve I, in Fig.3, shows the pulse distribution of helium recoils obtained with 10 atmospheres of argon in the chamber. This curve shows

that the spectrum of the fission neutrons extends to about 3.5 Mev.Though the existence of a small number of high energy neutrons such as reported by von Halban ,Joliot and Kowarski[8], is not inconsistent with our result,the number of neutrons having energies above 4 Mev appears to be too small to have any bearing on our estimate of the total number of fission neutrons.

Since the calibration of the chamber,which we performed by means of protons,is not entirely satisfactory for correlating the size of the pulses due to helium recoils with the energy of the helium recoils,the helium-argon filled chamber was also calibrated by means of D + D neutrons of 2.5 Mev energy.The two calibrations coincided within the limits of the experimental error.

An estimate of the number of fissions neutrons should be based on a count of hydrogen recoils rather than helium recoils since the scattering cross-section of helium has a sharp maximum[9] for neutrons of about 1.0 Mev energy,and helium is therefore not suitable for the purposes of a quantitative estimate.It can be shown that,if the neutron proton scattering is spherically symmetrical in the system of the center of gravity,the number of neutrons N(E)dE which pass through the chamber,and which have an energy between E and E dE,is given by:

$$N(E) = -\frac{E}{H\sigma(E)}\,\frac{dR(E)}{dE}$$

where ~~$\sigma(E)$~~ $\sigma(E)$ is the scattering cross-section of the proton and H is the number of hydrogen atomes in the ionization chamber. From this we derive, for the total number of neutrons, N, passing through the chamber:

$$N = \int_0^\infty N(E)\,dE = \frac{\alpha}{\bar{\sigma}} \cdot \frac{E_c R(E_c) + \int_{E_c}^\infty R(E)\,dE}{H}$$

where α is the ratio of the total number of neutrons to the number of neutrons which have an energy in excess of E_c, and $\bar{\sigma}$ is an average scattering cross- section of the proton, the value of which has to be determined from the energy spectrum of the neutrons. Since we have:

$$\int_0^\infty R(E)\,dE = \int_0^\infty P(E)\,dE$$

and since, for the reasons stated above, we have with good approximation:

$$R(E) \simeq P(E) \quad \text{for } E \leq E_c$$

we can express N in terms of P(E) instead of R(E). We then have:

$$N = \frac{\alpha}{\bar{\sigma}} \cdot \frac{E_c P(E_c) + \int_{E_c}^\infty P(E)\,dE}{H}$$

Let it now be assumed for the sake of argument that all the neutrons are emitted from a moving uranium fragment which has a mass number of about 120 and a kinetic energy of about 150 Mev. If all the neutrons were emitted

from such a moving fragment with a single energy E_o, the energy distribution of the neutrons in the laboratory reference system would stretch from:

$$E_{min} = (0.9 - \sqrt{E_o})^2 \text{ Mev.}$$

to:

$$E_{max} = (0.9 + \sqrt{E_o})^2 \text{ Mev.}$$

It is easy to see that the neutrons should be uniformly distributed in this energy interval if their distribution is spherically symmetrical in the center of mass system. One obtains accordingly:

$$\mathcal{L} = \frac{E_{max} - E_{min}}{E_{max} - E_c}$$

and

$$\frac{1}{\bar{\sigma}} = \frac{1}{E_{max} - E_c} \int_{E_c}^{E_{max}} \frac{dE}{\sigma(E)}$$

Using for $\sigma(E)$ the curve shown in Fig.4 which has been theoratically derived [10], the following values of $\frac{\mathcal{L}}{\bar{\sigma}}$ are obtained for various values of E_{max}

E_{max}	2.0 Mev	3.0 Mev	4.0 Mev
$\frac{\mathcal{L}}{\bar{\sigma}}$	0.316×10^{-24}	0.353×10^{-24}	0.388×10^{-24}

The variation of $\frac{\mathcal{L}}{\bar{\sigma}}$ with E_{max} is so slight because of the manner in which both $\mathcal{L}$ and $\bar{\sigma}$ decerase with rising E_{max}.

The value of the expression:

$$E_c P(E_c) + \int_{E_c}^{\infty} P(E)\,dE$$

was found from the observed pulse distribution (Curve II of Fig. 3) to be 13.7 pulses per minute. Since the number H of hydrogen atoms in the chamber was $H = 6.9 \times 10^{-21}$ the number of neutrons passing through the chamber is

$$N = \frac{\alpha}{\sigma} 1.98 \times 10^{-21}$$

per minute.

If the cadmium cap CG is removed the number of thermal neutrons reaching the uranium cell is increased and the number of fast neutrons passing through the ionization chamber is increased by the same factor. This factor was found to be 1.22 by filling the ionization chamber with pure hydrogen and then counting the hydrogen recoils giving rise to pulses above a certain arbitrarily level, both in the presence and absence of the cadmium cap. Thus the number of neutrons N^* which pass through the chamber in the absence of the cadmium cap, G, is

$$N^* = \frac{\alpha}{\sigma} 2.415 \times 10^{-21}$$

per minute.

From N the number, K, of neutrons emitted per minute by the uranium was calculated by taking into account the geometrical factors, including the variation of the thermal neutron density within the uranium cell. K is thus found to be:

$$K = \frac{\alpha}{\sigma} 8.25 \times 10^{-19}$$

per minute.

In order to obtain the number of neutrons emitted per fission it is necessary to compare K with the number of fissions ,L, which occur in the uranium under the conditions of this experiment.In order to do so both the ionization chamber and the uranium cell were removed and a parallel plate ionization chamber lined with a thick layer of uranium oxide was placed in the position previously occupied by the uranium cell.The number of fissions produced in this chamber was observed and found to be 19 per minute.The chamber was then calibrated by comparing the number of fissions ontained from the thick uranium oxide layer with the number of fissions obtained in the same chamber at the same thermal neutron intensity from a thin layer of uranium oxide containing 1.4 mgm. of uranium.The calibration was carried out by using a particularly strong neutron source,so as to obtain a sufficiently large number of counts from the thin layer.The ratio of the fission counts from thet thick layer and from the thin layer was found to be29.2,from which it is concluded that 196,000 fissions per minute should take place in the uranium cell containing 427.7 grams of uranium. This would be the number of fissions if the density of the thermal neutrons were not reduced in the uranium cell due to the absorption of such neutrons in uranium.We estimate that the average density of thermal neutrons within the cell is reduced by a factor of 0.715.The number of fissions L actually taking place within the cell is therefore

L =140,000 per minute.

In order to estimate the reduction of the average

thermal neutron density within the uranium cell which leads to the factor of 0.715, we first explored the anisothropy of the thermal neutron radiation near the uranium cell by means of a rhodium indicator, and then calculated the thermal neutron density within the uranium by superposing two parallel beams having a relative intensity of 3 to 1, moving in opposite directions along the axis of the arrangement, the first away from the neutron source, the second towards the neutron source. We assume exponential absorption for these two beams within the uranium and an exponent corresponding to a half - value thickness in uranium of 14 gm per cm^2.

The number of neutrons emitted per fission is

$$\frac{K}{L} = \frac{\alpha}{\sigma} \times 5.9 \times 10^{-24}$$

This number should be increased by perhaps 10% in order to correct for the fact that P(E) does not exactly coincide with R(E) even for $E = E_c$. The magnitude of this correction was estimated by comparing for D+D neutrons of 2.5 Mev energy the observed pulse distribution P(E) with the calculated distribution R(E) in the region of the low recoil energies. Making this correction one finds for ν the number of neutrons per fission $\nu = \frac{\alpha}{\sigma} 6.5 \times 10^{-24}$

Using for $\frac{\alpha}{\sigma}$ the value 0.353×10^{-24} which corresponds to $E_{max} = 3$ Mev rather han to the actually observed upper limit of the fission neutron spectrum, one finds

$$\nu = 2.3$$

Since the fission neutrons hardly will be wmitted with a single energy E_0, too high a value for $\frac{\alpha}{\sigma}$ would be obtained if the observed value of the upper limit of the energy

spectrum were used for E_{max}. In any case the error introduced by the uncertainty of the actual energy distribution of the fission neutrons should be small since one finds for

E_{max} = 2Mev $\quad \gamma = 2.0$ and for

E_{max} = 4 Mev $\quad \gamma = 2.5$

More serious, however, may be a number of experimental inaccuracies which might conceivably add up to give a considerable error.

The interest in the number of neutrons emitted per fission arose out of its obvious importance from the point of view of the possibility of nuclear chain reations. At present we have the following set of values: number of neutrons per fission, 2.3; fission cross-section[11], 2.0×10^{-24} cm^2; cross - section for radiative capture[12] 1.3 or 1.2×10^{-24} cm^2. According to these values, the number of neutrons emitted by uranium per thermal neutron absorbed should be 1.4 which agrees with the value of 1.5 recently obtained by another method by Anderson, Fermi and Szilard[13]. Too much significance should not be attrubuted to this agreement, since the values given above are subject to fairly wide experimental errors.

If required the present experiments could be repeated with greater accuracy since the method used is quite capable of being applied with greater precision. Moreover, it gives the number of neutrons per fission independently of the value of the fission cross-section which enters into the method used by von Halban, Joliot and Kowarski. These authors report a value of 3.5 ± 0.7 neutrons per fission.

It should be mentioned that it appears to be essential for the method presented here to work with a low background count due to the primary neutrons such as was here obtained by using a photo - neutron source. We did not find it possible to obtain quantitative results by using neutrons from radon-beryllium sources or from the D + D reaction on account of the high background count due to the primary neutrons.

We are indebted to Dr. G.N. Glasoe for suggesting the methof of obtaining the photographic records and for much valuable advice in this connection, and to Dr. E.T. Booth for determining by means of an alpha - particle count the uranium content of the thin uranium sheet which we used for purposes of calibration. Also, we wish to thank the Department of Physics of Columbia University for the laboratory facilities placed at our disposal and the Association for Scientific Collaboration for enabling us to obtain the radium used in this experiment.

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Abstract.

Fast neutrons emitted by uranium under the action of thermal neutrons were studied by using a radium - beryllium photo - neutron source.The background due to the primary neutrons can be neglected since only a few of the photo-neutrons are sufficiently fast to be counted,and it thus becomes possible to obtain quantitative results.Data are obtained concerning the energy spectrum of the uranium fission neutrons by recording photographically by means of a linear amplifier and cathode-ray oscillograph the pulses due to helium atoms projected in an ionization chamber.Visual inspection of the record gives an upper limit of the spectrum of 3.5 Mev. The number of neutrons emitted is estimated by analyzing the pulse distribution of hydrogen recoils projected by uranium neutrons in an ionization chamber filled with hydrogen and argon,and making use of the fact that the scattering cross-section of hydrogen is a known function of the neutron energy.The number found is brought into relationship with the number of fissions,observed under comparable conditions,in an ionization chamber lined with a thin film of uranium oxide containing a known amount of uranium.In this way it is found that about 2.3 neutrons are emitted per fission.The method used would permit a greater accuracy in the determination of this number than the actual

accuracy obtained in the present experiments. This number together with the fission cross-section and the cross-section for radiative capture gives the number of neutrons produced for each thermal neutron absorbed in uranium.

Albert Einstein
Old Grove Rd.
Nassau Point
Peconic, Long Island

August 2nd, 1939

F.D. Roosevelt,
President of the United States,
White House
Washington, D.C.

Sir:

Some recent work by E.Fermi and L. Szilard, which has been communicated to me in manuscript, leads me to expect that the element uranium may be turned into a new and important source of energy in the immediate future. Certain aspects of the situation which has arisen seem to call for watchfulness and, if necessary, quick action on the part of the Administration. I believe therefore that it is my duty to bring to your attention the following facts and recommendations:

In the course of the last four months it has been made probable - through the work of Joliot in France as well as Fermi and Szilard in America - that it may become possible to set up a nuclear chain reaction in a large mass of uranium,by which vast amounts of power and large quantities of new radium-like elements would be generated. Now it appears almost certain that this could be achieved in the immediate future.

This new phenomenon would also lead to the construction of bombs, and it is conceivable - though much less certain - that extremely powerful bombs of a new type may thus be constructed. A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with some of the surrounding territory. However, such bombs might very well prove to be too heavy for transportation by air.

The United States has only very poor ores of uranium in moderate quantities. There is some good ore in Canada and the former Czechoslovakia, while the most important source of uranium is Belgian Congo.

In view of this situation you may think it desirable to have some permanent contact maintained between the Administration and the group of physicists working on chain reactions in America. One possible way of achieving this might be for you to entrust with this task a person who has your confidence and who could perhaps serve in an inofficial capacity. His task might comprise the following:

a) to approach Government Departments, keep them informed of the further development, and put forward recommendations for Government action, giving particular attention to the problem of securing a supply of uranium ore for the United States;

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I understand that Germany has actually stopped the sale of uranium from the Czechoslovakian mines which she has taken over. That she should have taken such early action might perhaps be understood on the ground that the son of the German Under-Secretary of State, von Weizsäcker, is attached to the Kaiser-Wilhelm-Institut in Berlin where some of the American work on uranium is now being repeated.

Yours very truly,

A. Einstein

(Albert Einstein)

Chron 8/10/45 Corresp - Nat Def.

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Yours very truly,

Albert Einstein
(Signed)

(Original is blurred)

(17)

AN ENEMY ALIEN WORKS FOR UNCLE SAM

I found a copy of this letter more than ten years after it was written, when, in a fit of house-cleaning enthusiasm, I tackled a certain cabinet where we keep family papers.

March 16, 1939

Admiral S. C. Hooper
Office of Chief Naval Operations
Navy Department
Washington, D.C.

DEAR SIR:

. . . Experiments in the physics laboratories at Columbia University reveal that conditions may be found under which the chemical element uranium may be able to liberate its large excess of atomic energy, and that this might mean the possibility that uranium might be used as an explosive that would liberate a million times as much energy per pound as any known explosive. My own feeling is that the probabilities are against this, but my colleagues and I think that the bare possibility should not be disregarded and I therefore telephoned . . . this morning chiefly to arrange a channel through which the results of our experiments might, if the occasion should arise, be transmitted to the proper authorities in the United States Navy.

Professor Enrico Fermi who, together with Dr. Szilard, Dr. Zinn, Mr. Anderson, and others, has been working on this problem in our laboratories, went to Washington this afternoon to lecture before the Philosophical Society in Washington this evening and will be in Washington tomorrow. He will telephone your office, and if you wish to see him will be glad to tell you more definitely what the state of the knowledge on this subject is at present.

Professor Fermi . . . is Professor of Physics at Columbia University

. . . was awarded the Nobel Priz[illegible] There is no man more competent in this field of nuclear physics than [illegible] Fermi . . .

Professor Fermi has [illegible] permanently in this country and will become a[illegible] course. . . .

[illegible],

GEORGE B. PEGRAM
Professor of Physics

GBP:H

I had never set eyes [illegible] the excitement of the historian who discovers [illegible] document. That same evening I showed it to Enrico.

At first he appeare[illegible] as I was. He read the letter over carefully [illegible]

He turned to me

"Where [illegible]

"In a [illegible] together with a clipping from the newspaper [illegible] that criticized you because you did not give the [illegible] salute to the King of. . . .

"Now I remember!" Enrico interrupted. "I prepared that file when we became enemy aliens. I thought it might be used as evidence of our loyalty to the United States."

We became enemy aliens on December 8, 1941. On that day President Roosevelt declared that an "invasion or predatory incursion is threatened upon the territory of the United States" by Germany and Italy, and proclaimed Germans and Italians "alien enemies." Formal declaration of war to those countries occurred three days later.

I did not want this [illegible] of recollections to take me away from Professor Pegram's letter. I wanted to know the reasons why it was written.

It was self-explanatory, Enrico said. Professor Pegram had prepared it as an introduction to Admiral Hooper and had given a copy to Enrico so that he would be informed of its content.

"Did you really go see Admiral Hooper? What came out of your interview? Why did you never say anything about it?"

Enrico had seen the Admiral. To mention this fact would have been an indiscretion, although no official secrecy policy had yet been established. The interview had yielded little result.

"Couldn't you arouse the Admiral's interest in the atomic bomb?"

"You are using big words. You forget that in March, 1939, there was little likelihood of an atomic bomb, little proof that we were not pursuing a chimera." Enrico was no longer interested in this conversation. He had picked up the *New York Times*, and now he deployed it in front of him, thus indicating that the interview was at an end. I was left to my own thoughts.

Professor Pegram's letter, I reflected, is of historical significance as the first attempt of science to establish connections between research and government, not existing at that time. In this respect its most important part is its date: March 16. Only two months, to the day, had passed since Professor Niels Bohr had landed in America and received the celebrated telegram confirming uranium fission. During those two months experiments on fission had been performed at American universities, and Enrico's hypothesis that neutrons would be emitted had found experimental confirmation. The possibility of achieving a chain reaction and of placing the huge stores of energy in nuclei at man's disposal in a not too distant future had been envisaged and its implications probed. All this placed a burden of responsibility on the scientists too great for a small group to bear alone, even in a world at peace; and in March, 1939, the world was hardly at peace.

On that same March 16 when Professor Pegram had written his letter, Hitler had annexed what was left of Czechoslovakia after the Munich dismemberment. War was approaching. There could be little doubt of it. Results of nuclear studies ought not to be confined inside the laboratories. Hence the attempt at alerting the Navy.

It is not surprising that this attempt should have been inconclusive. Examined in the light of subsequent events, it appears to have been carried out with too great hesitation. That Fermi should wish to contact Admiral Hooper *because* he happened to be in Washington, that he should not plan his trip *in order* to see the Admiral, further minimizes that "bare possibility" of atomic explosives that sounds in itself overcasual, now that atomic weapons are a fact.

Professor Pegram's attitude was due to his cautious judgment that warned him against jumpiug to premature conclusions. His

skepticism about the outcome of the work in his own laboratories was shared by many other scientists and probably caused by a hope that nuclear weapons would prove impossible. And Enrico himself, when talking to Amaldi, questioned the relevance of his predictions.

Fission of Uranium under Deuteron Bombardment

SINCE the phenomenon of neutron-induced fission of the uranium and thorium nuclei is now indisputably established, attention may be turned to the possible efficacy of other bombarding particles in this respect. An outline is here presented of evidence suggesting that high-energy deuterons are, in fact, capable of producing fission in uranium.

The following experimental arrangement has been used. One inside surface of a copper box is covered with a 20 mgm./cm.^{2} layer of uranium metal, and is bombarded by deuterons passing through an aperture in the opposite side of the box. An inside pocket on one of the other walls contains metal foils for the purpose of collecting particles projected from the target through an aperture in the pocket. A thickness of 2 mm. S.P. aluminium is always present to protect the collecting foil from the low-energy products due to mechanical disintegration of the target. Bombardments of about 1 μamp. for ten minutes are convenient, and are found to produce on the collector activities of intensity suitable for measuring with a Geiger counter.

It is necessary to estimate what part of the activity on the collector is due to general neutron radiation and to deuterons scattered from the target. In the pocket are placed three foils of 3·3 mgm./cm.^{2} aluminium, F, D and N in order of proximity to the target, and a single thickness of half-millimetre copper sheet between the foils D and N. Five minutes after a 4 μamp./min. bombardment with 9 Mv. deuterons, it is found that the neutron effect of the irradiation has produced in N a small activity of 40 counts per minute; the neutron effect is eliminated from D and F simply by subtraction of this N effect. D's activity measures the scattered-deuteron effect and is initially about 100 counts per minute, decaying with a half-life of 2·3 min. to reach a negligibly small value after 20 min. F's activity, however, is about 1,500 counts per min., and is much longer-lived, although with a target entirely of lead, F as well as D shows only a D effect.

The decay curve for F's activity has been followed for about 7 hours and has been compared with that calculated from Frisch's formula[1] for the decay of multiplex activity. The measure of agreement is so substantial as to preclude the possibility of a merely accidental coincidence.

A stack of six 0·66 mgm./cm.^{2} aluminium foils has been placed in the pocket and after the lapse of 30 minutes (when the D effect has decayed) the activities are found to decrease regularly through the stack, reaching inappreciably small values in the fifth and sixth foils. Regarding another aspect, the fact that replacement of aluminium as the collector material by silver causes no significant difference in the decay curve, suggests that the activity has been implanted rather than induced. The evidence thus indicates that radioactive nuclei are projected from the uranium target with a range of the order of 2 cm.; this occurrence is evidently to be ascribed to a fission of the uranium nucleus.

It is important to assess the magnitude of the fission effect caused by background neutron radiation, and for this purpose there has been enclosed in the pocket itself a layer of uranium, separated from F by 2 mm. S.P. of aluminium, and from the target by the copper sheet, this arrangement necessarily reproducing fairly closely that activity in F which is due to neutron-induced fission. The intensity in this case is less than one twentieth of that observed in the positive experiment.

Approximate measurements have been made on the excitation function for deuteron-induced fission, using aluminium absorbers to modify the energy of the beam and a suitable disposition of diaphragms to protect the collector against deuterons scattered from the absorbers. The threshold for the process appears to lie at about 8 Mv., and the cross-section increases rapidly in the range 8–9 Mv.

The conclusions drawn from these preliminary experiments would receive unequivocal confirmation from a chemical identification of the fragment nuclei or a detection of their large ionization impulses in an ionization chamber. It might also prove profitable to examine the effects given by others of the heaviest elements (especially thorium).

Dr. R. S. Krishnan and other colleagues of the Cavendish cyclotron group have rendered me indispensable assistance, and I am indebted also to Prof. J. D. Cockcroft for pertinent suggestions.

Cavendish Laboratory, Cambridge. Aug. 26. D. H. T. GANT.

[1] NATURE, 143, 852 (1939).

Absorption of Polymolecular Films in the Infra-Red

THE use of infra-red absorption spectra as a means of investigating molecular structure has still to be extended to surface films. Until very recently, the possibilities of this extension seemed remote, since a monomolecular layer does not contain enough molecules per square centimetre to produce measurable absorption. Thus it is found experimentally that approximately 10^{19} molecules are required to produce appreciable absorption in a beam of 1 sq. cm. cross-section, whereas the number of molecules per square centimetre in a monomolecular layer is about 5×10^{14}. With the production of polymolecular films close to 1,000 molecules thick, the possibility of detecting absorption still seemed slight, but technically feasible, if a method employing several reflections through such a film were employed.

As a matter of interest, it was decided to try whether any absorption could be detected using a single reflection through a film of methyl stearate 700 molecules thick, deposited on a chromium-plated strip of metal. We were surprised to find that the well-known absorption at 3·3 μ due to CH groups was easily detectable, showing about 30 per cent absorption, while other weaker bands were noticeable between 6 μ and 10 μ. Next, a film only 200 molecules thick was tried. This still gave the 3·3 μ band with appreciable intensity. These results mean that absorption spectra were being obtained from approximately 10^{17} molecules, instead of the customary 10^{19}. In other words, the absorption coefficient of a molecule in the infra-red seems to be considerably increased when it is in a surface film.

The possible causes of this will not be discussed here. The purpose of this note is merely to record this fact, which opens up a new and potentially interesting field of research in infra-red spectroscopy and its application to problems of molecular structure. In particular, this may prove to be a very suitable method of studying protein molecules.

We wish to express our thanks to Dr. Stenhagen, who prepared the films.

G. B. B. M. SUTHERLAND.
W. T. TUTTE.
Laboratory of Physical Chemistry,
Cambridge. Sept. 1.

Copy - Nat. Def

EDWARD TELLER
2610 GARFIELD STREET
WASHINGTON, D. C.

October 18-th, 1939

Mr Alexander Sacks
One South William St
New York City

My dear Mr Sacks,

Thank you very much for your letter. I shall be at the Carlton Hotel at 9: 15 Saturday morning.

Yours sincerely

Edward Teller

Edward Teller

Comp - Nat. Def.

J. W. Beams

E. Fermi

W. H. Furry

L. Szillard

E. Teller

M. A. Tuve

J. A. Wheeler

Two persons additionally suggested:

E. P. Wigner - Princeton

R. B. Roberts - Washington

Letters to the Editor

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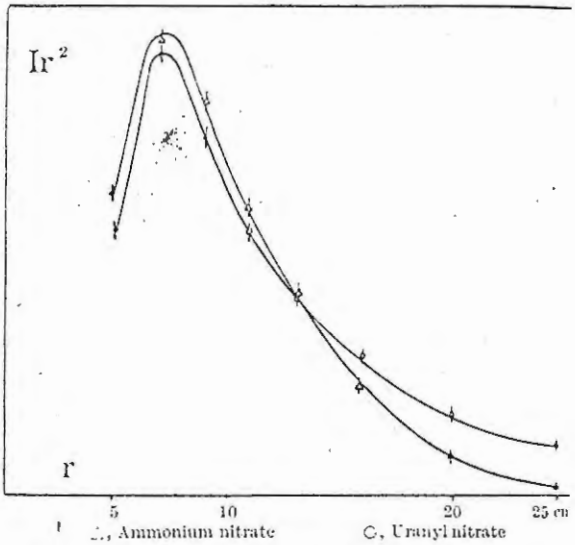
NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR ON P. 478.

CORRESPONDENTS ARE INVITED TO ATTACH SIMILAR SUMMARIES TO THEIR COMMUNICATIONS.

Liberation of Neutrons in the Nuclear Explosion of Uranium

RECENT experiments[1,2] have revealed the existence of a new kind of nuclear reaction: neutron bombardment of uranium and thorium leads to an explosion of the nucleus, which splits up into particles of inferior charge and weight, a considerable amount of energy being liberated in this process. Assuming a partition into two particles only, so that the nuclear mass and charge of uranium have to be distributed between two lighter nuclei, the latter contain considerably more neutrons than the heaviest stable isotopes with the same nuclear charges. (A splitting into, for example, ^{98}Rb and ^{141}Cs means an excess of 11 neutrons in the first, and of 8 neutrons in the second of these two nuclei.) There seem to be two possibilities of getting rid of this neutron excess. By the emission of a β-ray, a neutron is transformed into a proton, thus reducing the neutron excess by two units; in the example given above, five and four successive β-activities respectively would be needed to restore the neutron-proton stability ratio. In fact, the explosion products have been observed to be β-active and several periods have been recorded, so that a part, at least, of the neutron excess is certainly disposed of in this way. Another possible process is the direct liberation of neutrons, taking place either as a part of the explosion itself, or as an 'evaporation' from the resulting nuclei which would be formed in an excited state.

In order to find some evidence of this second phenomenon, we studied the density distribution of the thermal neutrons produced by the slowing down of photo-neutrons from a Ra γ - Be source in a 1·6 molar solution of uranyl nitrate and in a 1·6 molar solution of ammonium nitrate (the hydrogen contents of these two solutions differ by only 2 per cent). Plotting Ir^2 as a function of r (where r is the distance between the source and a given point, and I is the local density of thermal neutrons at the same point, measured by the activity induced in a dysprosium detector), a curve is obtained the area of which is proportional to $Q.\tau$, Q being the number of neutrons per second emitted by the source or formed in the solution and τ the mean time a neutron spends in the solution before being captured[3,4]. Any additional nuclei, which do not produce neutrons, brought into the solution, will increase the chances of capture and therefore decrease τ and the area. If, however, these dissolved nuclei are neutron-producing, Q will be greater and the area of the curve will tend to increase. Evidence of neutron production, as indicated by an actual increase of the area, will only be obtained if the gain through Q (neutron production) is greater than the loss through τ (neutron capture). This loss can anyway be studied separately, since it has been shown[5] that the introduction of nuclei which act merely by capture or by increasing the hydrogen content of the solution can affect the shape of th density curve only in a characteristic way: the modi fied curve can always be brought to coincide wit the primitive curve by multiplying all abscissæ b a suitable factor and all ordinates by another facto



The accompanying graph shows the two curve obtained. At small distances from the source th neutron density is greater in the ammonium solutio than in the uranyl solution; at distances greate than 13 cm., the reverse is true. In other words the decrease of the neutron density with the distanc is appreciably slower in the uranyl solution.

The observed difference must be ascribed to th presence of uranium. Since the two curves cannot be brought to coincide by the transformation mentioned above, the uranium nuclei do not act by capture only; an *elastic* diffusion by uranium nuclei would have an opposite effect: it would 'contract' the abscissæ, instead of stretching them. The density excess, shown by the uranyl curve beyond 13 cm. must therefore be considered as a proof of neutron production due to an interaction between the primary neutrons and the uranium nuclei. A reaction of the well-known $(n,2n)$ type is excluded because our primary neutrons are too slow for such a reaction (90 per cent of Ra + Be photo-neutrons have energies smaller than 0·5 Mev. and the remaining 10 per cent are slower than 1 Mev.).

The degree of precision of the experiment does not permit us to attribute any significance to the small increase of the area in the uranyl curve (as compared to the ammonium curve), which we obtain by extrapolating the curves towards greater distances. In any event, an inferior limit for the cross-section for the production of a neutron can be obtained by

ssuming that the density excess due to this production is equal throughout the whole curve to the xcess observed at $r = 25$ cm.; this limit, certainly nferior to the actual value, is 6×10^{-25} cm.^{2}.

Our measurements yield no information on the nergy of the neutrons produced. If, among these eutrons, some possess an energy superior to 2 Mev., ne might hope to detect them by a (n,p) process, or example, by the $^{32}S(n,p)^{32}P$ reaction. An experiment of this kind, Ra γ - Be still being used as the rimary neutron source, is under way.

The interest of the phenomenon observed as a step owards the production of exo-energetic transmutaion chains is evident. However, in order to establish uch a chain, more than one neutron must be produced for each neutron absorbed. This seems to be the ase, since the cross-section for the liberation of a eutron seems to be greater than the cross-section for he production of an explosion. Experiments with olutions of varying concentration will give informaion on this question.

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Collège de France,
Paris.
March 8.

Joliot, F., *C.R.*, 208, 341 (1939).
Frisch, O. R., NATURE, 143, 276 (1939).
Amaldi, E., and Fermi, E., *Phys. Rev.*, 50, 899 (1936).
Amaldi, E., Hafstad, L., and Tuve, M., *Phys. Rev.*, 51, 896 (1937).
Frisch, O. R., von Halban, jun., H., and Koch, J., *Danske Videnskab. Kab.*, 15, 10 (1938).

Products of the Fission of the Uranium Nucleus

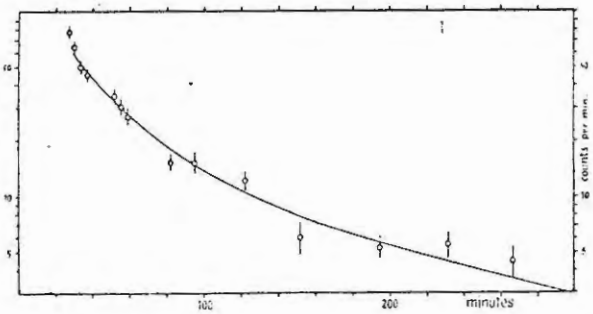
O. Hahn and F. Strassmann[1] have discovered a new type of nuclear reaction, the splitting into two smaller nuclei of the nuclei of uranium and thorium under neutron bombardment. Thus they demonstrated the production of nuclei of barium, lanthanum, strontium, yttrium, and, more recently, of xenon and cæsium.

It can be shown by simple considerations that this type of nuclear reaction may be described in an essentially classical way like the fission of a liquid drop, and that the fission products must fly apart with kinetic energies of the order of hundred million electron-volts each[2]. Evidence for these high energies was first given by O. R. Frisch[3] and almost simultaneously by a number of other investigators[4].

The possibility of making use of these high energies in order to collect the fission products in the same way as one collects the active deposit from alpha-recoil has been pointed out by L. Meitner (see ref. 3). In the meantime, F. Joliot has independently made experiments of this type[5]. We have now carried out some experiments, using the recently completed high-tension equipment of the Institute of Theoretical Physics, Copenhagen.

A thin layer of uranium hydroxide, placed at a distance of 1 mm. from a collecting surface, was exposed to neutron bombardment. The neutrons were produced by bombarding lithium or beryllium targets with deuterons of energies up to 800 kilovolts. In the first experiments, a piece of paper was used as a collecting surface (after making sure that the paper did not get active by itself under neutron bombardment). About two minutes after interrupting the irradiation, the paper was placed near a Geiger-Müller counter with aluminium walls of 0·1 mm. thickness. We found a well-measurable activity which decayed first quickly (about two minutes half-value period) and then more slowly. No attempt was made to analyse the slow decay in view of the large number of periods to be expected.

The considerable intensity, however, of the collected activity encouraged us to try to get further information by chemical separations. The simplest experiment was to apply the chemical methods which have been developed in order to separate the 'transuranium' elements from uranium and elements immediately below it[6]. The methods had to be slightly modified on account of the absence of uranium in our samples and in view of the light element activities discovered by Hahn and Strassmann[1].



In these experiments, the collecting surface was water, contained in a shallow trough of paraffin wax. After irradiation (of about one hour) a small sample of the water was evaporated on a piece of aluminium foil; its activity was found to decay to zero. It was checked in other ways, too, that the water was not contaminated by uranium. To the rest of the water we added 150 mgm. barium chloride, 15 mgm. lanthanum nitrate, 15 mgm. platinum chloride and enough hydrochloric acid to get an acid concentration of 7 per cent. Then the platinum was precipitated with hydrogen sulphide, in the usual way; the precipitate was carefully rinsed and dried and then placed near our counter.

The results of three such experiments were found to be in mutual agreement. The decay of the activity was in one case followed for 28 hours. For comparison, a sample of uranium irradiated for one hour was treated chemically in the same way. The two decay curves were in perfect agreement with one another and with an old curve obtained by Hahn, Meitner and Strassmann under the same conditions. In the accompanying diagram the circles represent our recoil experiment while the full line represents the uranium precipitate. A comparison of the activity (within the first hour after irradiation) of the precipitate and of the evaporated sample showed that the precipitate contained about two thirds of the total activity collected in the water. After about two hours, however, the evaporated sample was found to decay considerably more slowly than the precipitate, presumably on account of the more long-lived fission products found by Hahn and Strassmann[1].

From these results, it can be concluded that the 'transuranium' nuclei originate by fission of the uranium nucleus. Mere capture of a neutron would give so little kinetic energy to the nucleus that only a vanishing fraction of these nuclei could reach the water surface. So it appears that the 'transuranium'

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SUPPLEMENT

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LETTERS TO THE EDITOR

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NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR AT P. 811.

NUCLEAR PHYSICS

Control of the Chain Reaction involved in Fission of the Uranium Nucleus

IT has recently been shown that the number[1] of neutrons liberated[2] in the nuclear fission of a uranium nucleus is sufficiently high to make the realization of a self-perpetuating reaction chain seem possible. The danger that a system containing uranium in high concentration might explode, once the chain is started, is considerable. It is therefore useful to point out a mechanism which gives the possibility of controlling the development of such a chain.

We form an expression which is characteristic for the behaviour of the chain:

$$\nu'' = \frac{A_f}{A}\nu(1-\alpha), \quad . \quad . \quad (1)$$

A_f being the product of the cross-section for nuclear fission for a thermal neutron of the uranium nucleus with the concentration of the uranium; A_i the product of the absorption cross-section for thermal neutrons of the nucleus of kind i multiplied with its concentration; A the sum of all A_i's, which is to be taken over all kinds of nuclei present in the solution; ν is the average number of neutrons liberated in one fission, α the average probability for a neutron to diffuse out of the system before being absorbed.

The energy liberated by the chain will be

$$E = NF, \quad . \quad . \quad (2)$$

F being the energy liberated in one fission and N the number of fissions produced by the chain. We have

$$N = \nu'' + \nu''^2 + \nu''^3 + \ldots\ldots \quad (3)$$

The chain gives thus a quantity of energy, which is increasing rapidly with time, if ν'' is greater than 1. Let us consider the case of a chain which is due to fission produced by thermal neutrons; that is, a chain propagating itself in a system containing sufficient hydrogen for the slowing down of the neutrons.

If the cross-sections for capture or fission of all nuclei present follow the $1/v$ law, ν'' will not depend on the velocity of the neutrons and therefore not on the temperature of the system (since α will in practice be small and since it depends in the first place on the distance necessary for slowing down the neutron; the temperature has, of course, an effect, although it will be very small).

Let us, however, introduce an absorbent, such as cadmium, the cross-section of which does not depend on the neutron energy in the thermal region. We will have, instead of (1),

$$\nu'' = \nu\frac{A_f}{A' + A_c}(1-\alpha), \quad . \quad . \quad . \quad (4)$$

where A' is the sum of all A_i's following the $1/v$ law and A_c is a constant, the term due to the newly added absorbent. ν'' will now decrease with increasing temperature. At a temperature, which will be characteristic for the composition and the geometrical constants of the system, ν'' will become smaller than unity and the system will stabilize itself somewhere near this temperature; the equilibrium being determined by the fact that the amount of energy given out per unit of time by the system (in the form of heat and nuclear radiation) is equal to the energy produced by the system. Similar questions have been discussed by F. Perrin[3].

Added in proof: In the case of a chain propagating itself by thermal neutrons, the time necessary for the slowing down and for the absorption of a neutron, that is, its mean life, is of the order of 10^{-4} sec. If one makes ν'' as small as 1007, it needs 100 times the mean life of a neutron or about 10^{-2} sec. to double the number of neutrons, and with that the energy liberated per unit of time. It is therefore possible to control the development of the chain by a periodical interaction of absorbers which break up the chains by entering the system.

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[1] von Halban, jun., H., Joliot, F., and Kowarski, L., NATURE, 143, 470 (1939).

[2] von Halban, jun., H., Joliot, F., and Kowarski, L., NATURE, 143, 680 (1939). Roberts, R., Meyer, R., and Wang, P., *Phys. Rev.*, 55, 510 (1939). Haenny, C., and Rosenberg, A., *C.R.*, 208, 898 (1939). Szilard and Zinn (private communication). Huber and Buddinger (private communication).

[3] Perrin, F., *C.R.*, in the Press.

A series of experiments with varying number of α-rays per c.c. gave the following figures.

Time of irradiation in days	α	r from equation (2)	c/c_0	
			Experimental determination	Theoretical values from equation (1)
0·8	$0·9 \times 10^{13}$	0·07	0·91	0·93
2·0	1·9 ,,	0·14	0·85	0·87
4·1	3·3 ,,	0·26	0·82	0·77
11·8	5·6 ,,	0·43	0·72	0·65

In view of the approximations made and the experimental difficulties involved, the agreement between the experimentally determined and the calculated relative concentrations of unchanged molecules is quite satisfactory and supports the assumption that every haemocyanin molecule hit by an α-particle is split. This means that, out of the energy received in any part of the molecule, a portion large enough to cause splitting is transferred to the bond holding the two halves together.

As yet we have not carried out measurements of the absolute number of ultra-violet light quanta active in this splitting, but the independence of temperature points to a quantum yield of unity. It is of interest to note that from studies of the action of various rays on the mutation rate, it has been inferred that the production of a single ion pair within a sensitive spot of the cell, probably the gene, produces mutation[3]. There seems to be a certain analogy between this finding and our results.

The different behaviour of haemocyanin on one hand and haemoglobin and serum albumin on the other seems to us to be of considerable importance. It shows that the former molecule is easily dissociated by absorption of energy while the latter ones are very stable against a raising of the energy-levels so long as secondary chemical reactions are excluded.

THE SVEDBERG.
SVEN BROHULT.

Institute of Physical Chemistry,
The University,
Uppsala.
April 28.

[1] Svedberg, T., and Brohult, S., NATURE, 142, 830 (1938).
[2] Sanigar, E. B., Krejci, L. E., and Kraemer, E. O., *Biochem. J.* 33, 1 (1939).
[3] Cf. Stubbe, H., "Genmutation", 327 (Berlin, 1938).

Energy of Neutrons liberated in the Nuclear Fission of Uranium induced by Thermal Neutrons

IT has been shown that *fast* neutrons are liberated in the process of nuclear fission induced in uranium by primary *thermal* neutrons. Two different methods of detection have been used: in the first method[1], the primary and (if any) secondary neutrons are absorbed in a medium in which an endo-energetic reaction can take place, leading to the formation of an easily detectable radioactive nucleus. If the energy threshold is situated above the maximum energy of the primary neutrons, any positive results observed must be ascribed to the secondary neutrons. In the second method[2], elastic collisions of fast neutrons with heavier nuclei are observed by means of an ionization chamber filled with a gas at atmospheric pressure and connected to a linear amplifier. In order to study separately the effect due to the primary thermal neutrons, the experiment is performed with, and without, a cadmium shield between the source and the uranium mass.

The first method having shown us that fast secondary neutrons are produced with energies of at least 2 Mev. (sufficient to transform ^{32}S into radioactive ^{32}P in detectable quantities), we sought to ascertain, by the second method, whether neutrons of energy notably higher than 2 Mev. are also present in the secondary radiation. In our experiment, the oxygen-filled ionization chamber was placed in a nearly cubical box (9 cm. × 9 cm. × 8 cm.) containing uranium oxide and surrounded by a thick layer of paraffin wax. The source (300 mgm. Raγ + Be), surrounded by a lead shield (5 cm. in the direction of the chamber) was buried in the wax. In order to absorb thermal neutrons, the uranium box could be screened on all sides with a cadmium foil. The pulses were recorded either in the presence or in the absence of this foil and the part of the effect (projection of oxygen nuclei by fast neutrons liberated in the uranium) due to thermal neutrons could thus be evaluated.

In view of the large number of of accidental pulses due to the strong γ-radiation emitted by the source, only nuclei recoiling with at least 1·5 Mev. could be taken into consideration. The distribution curve shows that the frequency of pulses observed falls off rapidly between 1·5 Mev. and 2·5 Mev.; between 2·5 Mev. and 3·7 Mev. the frequency decreases much more slowly, pulses observed in this second region being, however, very rare. The total number of pulses recorded is small (with cadmium: 84 pulses in 90 minutes; without cadmium: 161 pulses in 90 minutes); but it appears clearly that recoils with energy of about 2·5 Mev. are notably more frequent in the absence of cadmium and, therefore, that *neutrons possessing an energy of at least* 11 Mev. *are liberated in uranium irradiated with thermal neutrons.*

The high energy of these fast neutrons shows that their parent nuclei are in a highly excited state at the moment of their liberation, which is probably simultaneous with the fission. In this way a non-negligible fraction of the fission energy is disposed of; a further fraction is carried off by the β- and γ-rays afterwards emitted by the nuclei produced in the fission. The remainder available as kinetic energy for these recoiling nuclei is therefore considerably smaller than the total amount of energy liberated in the fission process (about 200 Mev.).

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May 20.

[1] Dodé, M., von Halban, jun., H., Joliot, F., and Kowarski, L., *C.R.*, 208, 995 (1939).
[2] Szilard, L., and Zinn, W., *Phys. Rev.*, 55, 799 (1939).

Homometric Structures

IN connexion with a recent discussion[1] of the question of the uniqueness of an X-ray crystal analysis, Prof. Linus Pauling has directed my attention to a curious property of the point position[2] $Th^7 - Ia3 - 24(d)$. Pauling and Shappell[3] have shown that this point position, which involves a single parameter u, has a structure factor which is even in u, while parameter values $+u$ and $-u$ correspond to the structures which are not identical

temperatures below the critical range, and the structures produced in steels of different carbon content cooled at different rates or allowed to transform at different constant sub-critical temperatures.

Since the investigations of Dejean in 1917 and Portevin in 1919, it has been known that a progressive increase in the rate of cooling results in the first place in a gradual lowering of the normal changes, and in the second place, when a certain critical rate is exceeded, in the discontinuous depression of the changes to a lower temperature at which martensite is formed. Researches carried out since 1930 in the Kaiser Wilhelm Institute have contributed to present knowledge about the various changes and the relations between them. It has been shown, for example, that once the rate of cooling is sufficiently rapid to prevent any changes taking place above the martensite point, further increase in the rate of cooling does not produce further lowering of the martensite change. Thus a line may be drawn in the iron-carbon diagram to indicate the temperature at which the martensite change begins in steels of different carbon content, or a three-dimensional diagram may be drawn to show the relations between carbon content, rate of cooling and the temperatures of the transformations. It has also been known for some time that when steel is cooled to about 500° C. at a rate that suppresses the normal change, and is then cooled more slowly or maintained at constant temperature, an intermediate transformation occurs. The investigations with which Prof. Wever has been associated indicate, however, that this intermediate transformation may also proceed to some extent during continuous cooling at certain rates. Thus by cooling steels at progressively increasing rates, three kinds of changes may be obtained, namely, the normal, the intermediate and the martensitic.

By means of a magnetic balance, the progress of the normal and the intermediate changes at different constant temperatures has been studied. This work has shown that the rate of the normal austenite-pearlite transformation increases to a maximum as the temperature at which it is caused to take place is lowered, but further lowering leads to a diminution in the rate and it finally becomes extremely slow. A characteristic of this change is that at all temperatures there is a period of delay before it begins. At temperatures below that at which the normal change can take place the intermediate change occurs. This begins immediately, proceeds at a rate that becomes more rapid as the temperature is lowered, and continues to an extent that increases as the temperature is lowered. At still lower temperatures the martensite change takes place with great rapidity, and investigations on iron-carbon-nickel and iron-carbon-manganese alloys have shown that the martensite change in carbon steels corresponds to the $\gamma \rightarrow \alpha$ change in iron-nickel or iron-manganese alloys.

FISSION OF URANIUM NUCLEI

PROF. OTTO HAHN of the Kaiser Wilhelm Institut für Chemie, Berlin-Dahlem, was guest lecturer, on the invitation of the Royal Society, at the Royal Institution on Friday, June 23. Prof. Hahn's subject was "The Fission of Uranium Nuclei by Neutrons", and his account was chiefly historical. As the first definitely to establish the production of elements of medium atomic weight (fission products) when uranium is bombarded by neutrons, Prof. Hahn naturally confined himself mainly to describing the chemical method of investigation used by his colleagues and himself.

He described in detail how, in the process of confirming and extending the earlier investigations of Fermi and Curie and Savitch, Dr. Strassmann and he were forced to conclude that short-lived isotopes of barium and lanthanum were produced from uranium. These bodies could be separated from radium and actinium isotopes, but not at all from inactive barium or lanthanum, respectively, either by fractionation or by chemical means. Meitner and Frisch discussed these results in terms of division of the heavy nucleus into roughly equal fragments and Frisch showed, for the first time by a physical experiment, that the predicted large amount of energy is released in the act of fission. Thereafter, said Prof. Hahn, the whole subject was widely studied in a great many laboratories throughout the world. In his own laboratory, the chemical investigation was continued and the production of xenon, as fission product together with strontium, was established. The active barium and lanthanum, on this showing, arise chiefly as decay products following xenon and cæsium isotopes.

Up to this point, Prof. Hahn had described only previously unknown activities, but he went on to show how first one and then another of the so-called trans-uranic elements, postulated to explain most of the early work on the uranium disintegration, were proved, by purely chemical means, also to belong to the middle, rather than the end, of the periodic table. This was established by physical methods in other laboratories; but his own experiments had carried the matter further in one particular respect. After a great deal of labour, he had been able to show that the eka-iridium of the earlier classification contained molybdenum as well as tellurium—even though the half-value periods of the two active isotopes were distressingly similar! There remained only the task of applying the same methods of exact chemistry to the other activities which had not so far been studied, Prof. Hahn said, for his own contribution to the problem to be complete.

In the discussion which followed this most interesting lecture, Prof. Niels Bohr gave an account of the theoretical treatment of the fission problem recently undertaken by Dr. J. A. Wheeler and himself in the United States, and further contributions were made by Prof. M. L. E. Oliphant, Dr. E. Bretscher, Prof. G. P. Thomson and others.

Dr. Bretscher mentioned the results of preliminary experiments using large quantities of lead tetra-ethyl, which appear to show that fission occurs (though with a very small cross-section) with lead under fast neutron bombardment.

N. F.

Number of Neutrons Liberated in the Nuclear Fission of Uranium

RECENT experiments have shown that neutrons are liberated in the nuclear fission of uranium induced by slow neutron bombardment: secondary neutrons have been observed which show spatial[1], energetic[2] or temporal[3] properties different from those which primary neutrons possess or may acquire. Such observations give no information on the mean number of neutrons produced per nucleus split; this number ν may be very small (less than 1) and the result of the experiment will still be positive.

We are now able to give information on the value of ν. Let us consider the curve representing the density distribution of neutrons slowed down in an aqueous solution surrounding a primary neutron source[1]; the area S of this curve is proportional to $Q.\tau$, Q being the number of neutrons per second emitted by the source or formed in the solution, and τ the mean time a neutron spends in the solution before being captured. Assuming that the solution contains only nuclei which absorb neutrons according to the $1/v$ law (the only exception to this rule will presently be dealt with), τ is proportional to $1/\Sigma c_i\sigma_i$, where c_i is the concentration (atom grams per litre) of an absorbing nucleus, σ_i its cross-section for the capture of neutrons of velocity 1 and the index i is extended to all kinds of neutron-absorbing reactions attributable to nuclei present in the solution. Substituting the symbol A_i for $c_i\sigma_i$ and A_{tot} for ΣA_i, we have identically:

$$\frac{\Delta S}{S} = \frac{\Delta Q}{Q} - \frac{\Delta A_{tot}}{A_{tot}}, \qquad (1)$$

neglecting all terms of higher orders, such as those containing $(\Delta Q)^2$, $\Delta Q.\Delta A_{tot}$, etc.

Let the symbol Δ stand for the differences observed between the two solutions (uranyl and ammonium) used in our previous experiment[1]. Neglecting ΔA_{tot} before A_{tot} introduces an ambiguity in the definition of A_{tot} (uranyl *vs.* ammonium value) which is numerically unimportant and can be reduced by adopting the arithmetical mean $(A_{tot}(\text{amm.}) + \Delta A_{tot})/2$.

In the quantity ΔA_{tot} the uranium nuclei are represented by several separate terms standing for the different modes of neutron capture (see below); let A_f be the term for the capture leading to fission. Every neutron has the probability A_f/A_{tot} of causing a fission and, since one individual fission process liberates ν neutrons on the average, the total number ΔQ of neutrons thus created is $Q.\frac{A_f}{A_{tot}}.\nu$, and the equation (1) can be rewritten as follows:

$$\nu = \frac{\Delta S}{S} \cdot \frac{A_{tot}}{A_f} + \frac{\Delta A_{tot}}{A_f}. \qquad (2)$$

Let us estimate the values of all quantities necessary to calculate ν according to this formula. The area variation $\Delta S/S$ can be read from the graph given in our previous letter with an error of less than 20 per cent (due to the uncertainties of inter- and extrapolation; in order to facilitate the latter, we added to the curves a further experimental point for $r = 29$ cm.). The value of A_{tot} for the ammonium solution can be easily calculated from the known concentrations and capture cross-sections (hydrogen, nitrogen and oxygen). A_f is equal to c_U (1·6 in our experiment), multiplied by the value of σ_f given in a recent paper by Anderson *et al.*[4]. ΔA_{tot} contains a term expressing the small difference of the hydrogen content between the two solutions; and three terms relative to uranium, namely, the fission term A_f, already dealt with, the thermal capture term A_{ct} which can be calculated by using a recently found value for σ_{ct} [5] and finally the resonance capture term A_r which requires some explanation.

Our reasoning assumed that all neutrons introduced into the solution spend practically all their life, and are absorbed, in the thermal state. This is true in so far as the $1/v$ law is valid for absorption of neutrons in all nuclei concerned; and, therefore, not wholly true for uranium, which shows a pronounced resonance capture of neutrons of about 25 volts[6]. A certain proportion of neutrons entering the solution is bound to come within this resonance band and to be absorbed by resonance; therefore, it will never reach the thermal state. This proportion depends on the width of the resonance band and on the concentration c_U; its value in our system of symbols is equal to A_r/A_{tot} and was numerically determined by an experiment reported elsewhere[5].

Putting all numerical values in the formula (2) (with 10^{-24} cm.^{2} as the unit of cross-section), that is: $\Delta S/S = 0{\cdot}05 \pm 0{\cdot}01$; $A_{tot} = 36 \pm 3$; $A_f = 1{\cdot}6 \times 2 = 3{\cdot}2$; $\Delta A_{tot} = 8{\cdot}7 \pm 1{\cdot}4$ decomposable into $\Delta A_H = 1{\cdot}2 \pm 0{\cdot}1$, $A_{ct} = 1{\cdot}6 \times (1{\cdot}3 \pm 0{\cdot}45) = 2{\cdot}1 \pm 0{\cdot}7$, $A_r = 6{\cdot}4 \pm 1{\cdot}1$ and $A_f = 3{\cdot}2$, we find:

$$\nu = 3{\cdot}5 \pm 0{\cdot}7.$$

We were not able to allow for an error in A_f, since the value of σ_f given by Anderson *et al.* contains no indication of probable error. Any error in σ_f will affect $\nu - 1$ in an inversely proportional way; in any case ν will remain greater than 1.

The interest of the phenomenon discussed here as a means of producing a chain of nuclear reactions was already mentioned in our previous letter. Some further conclusions can now be drawn from the results reported here. Let us imagine a medium containing only uranium and nuclei the total neutron absorption of which, as compared to that of uranium, may be neglected (containing, for example, only some hydrogen for slowing down purposes). In such a medium, if $\frac{A_f}{A_{tot}} \cdot \nu > 1$ (A_{tot} includes now only uranium terms), the fission chain will perpetuate itself and break up only after reaching the walls limiting the medium. Our experimental results show that this condition will most probably be satisfied (the quantity $\frac{A_f}{A_{tot}} \cdot \nu - 1$, though positive, will be, however, small), especially if one keeps in view that the term A_r, because of the self-reversal of the resonance absorption line, increases much more slowly than the other uranium terms when the uranium content of the medium is increased.

H. VON HALBAN, JUN.
F. JOLIOT.
L. KOWARSKI.

Laboratoire de Chimie Nucléaire,
Collège de France,
Paris.
April 7.

[1] von Halban, jun., H., Joliot, F., Kowarski, L., NATURE, 143, 470 (1939).
[2] Dodé, M., von Halban, jun., H., Joliot, F., Kowarski, L., *C.R.*, 208, 995 (1939).
[3] Roberts, R., Meyer, R., Wang, P., *Phys. Rev.*, 55, 510 (1939).
[4] Anderson, H., Booth, E., Dunning, J., Fermi, E., Glasoe, G., Slack, F., *Phys. Rev.*, 55, 511 (1939).
[5] von Halban, jun., H., Kowarski, L., Savitch, P., *C.R.* (in the Press).
[6] Meitner, L., Hahn, O., Strassmann, F., *Z. Phys.*, 106, 249 (1937).

Corresp - Nat. Def.

UNDER
KNOTT MANAGEMENT

TELEPHONE
UNIVERSITY 4-2700

King's Crown Hotel

420 WEST 116TH STREET
NEW YORK
OPPOSITE COLUMBIA UNIVERSITY

August 25th, 1939

Dr. Alexander Sachs
One William Street
New York City

Dear Dr. Sachs:

Enclosed you will find a copy of the memorandum containing the changes which you suggested. I am also enclosing two copies of a reprint. Reprints of my joint paper with Fermi will be sent to you as soon as I receive them.

Yours sincerely,

Leo Szilard

(Leo Szilard)

[Corresp: Rosenblatt]

TEL. HANOVER 2-0590

CABLE ADDRESS:
"ROSEFEDER" NEW YORK

WILLIAM ROSENBLATT
27 WILLIAM STREET
NEW YORK CITY

October 8, 1935

Dr. Alexander Sachs,
c/o Lehman Corporation,
1 William Street,
New York City.

Dear Alexander:

Could you possibly use an extremely able economist from Germany, who has just recently arrived? He is Mr. Hans Meyer, who was the editor of the financial sheet of the "Berliner Tageblatt". His actual title was editor of the "Handels Zeitung", of the "Berliner Tageblatt".

Mr. Meyer has written extensively on economics, and is supposed to be extremely able. Mr. Meyer is a nephew of Professor Albert Einstein, and it is at the earnest request of Professor Einstein that I have undertaken to try to place Mr. Meyer somewhere, so that he may be able to continue the same kind of work that he did in Germany. If you could be of any assistance, both Professor Einstein and I would be greatly indebted to you.

With kind personal regards,

Sincerely yours,

Will Rosenblatt

WR:BC

[Ar-Ay]
[also Chron File - Feb 1939

Weekend February 3 - 4 1939

Meeting with Frank Aydelotte Director Designate (successor to Abraham Flexner) Institute for Advanced Study, Princeton, N. J. Also with Robert B. Warren and W.W. Stewart at the Institute.

Continued my Cassandra warnings of imminence around the Ides of March new Nazi-timing for aggression by a coup in Czechoslovakia, knocking the pins under the ramshackle Munich "settlement." Testing of ideas of imminent outbreak World War around August - September - in connection forthcoming Report for FDR arranged early March.

The approaching all - encompassing eclipse of civilian life for a most terrible technological war with unspeakable Nazi cruelties coincides in mysterious Providence with hints new scientific revelation through convergent discoveries in atomic physics. Under spell of just received book Background to Modern Science (Cambridge University Press late 1938) which includes the lectures of Lord Rutherford that I had heard in 1936 - With what modesty the late Lord Rutherford calls "the work of Bohr to be one of the greatest triumphs of the human mind." That applies to himself and not only to those giants but to the whole assemblage of workers in Europe on atomic physics, many of whom have been scattered(?) to our country and struggling to get laboratory spots and to pierce the surrounding indifference from the establishment of science research, as already well known from people within my ken among the refugee scientists.

The sequel lecture by F.W. Aston on Forty Years of Atomic Theory concludes with this terrifying challenge: " Personally I think that there is no doubt that sub-atomic energy is available all around us, and that one day man will release and control its almost infinite power. We cannot prevent him from doing so and can only hope that he will not use it exclusively in blowing up his next door neighbor" - What terrible portent if Nazis should get it ?!!!!

On return Sunday February 5 1939

By strange coincidence the subject uppermost in my mind been found dominant in the whole Institute Community under the impetus and inspiration of Niels Bohr . F.A. showed me copy of a letter written by Niels Bohr while at the Institute bearing the date of January 20, 1939 to the Editor of Nature, entitled "Disintegration of Heavy Nuclei." The letter opens: "Through kindness of the authors I have been informed of the contents of the letters recently sent to the Editor of Nature by Professor L. Meitner and Dr. Frisch." It ends with a plea for "the xxxxx continuation of the experiments in the new type of nuclear disintegration."

The communication to be published presently refers to the revolutionary accomplishment by two German chemists, Hahn and Strassmann, of atomic fission, and carries this further in the light of their corroborative experiments in the Stockholm Institute.

* * *

Follow-Up Weekend Visit, March 17-18 -

At the Institute the since received copies of Nature containing the Meitner-Frisch communication was more of a resounding subject than the fulfillment of my xxxxx Cassandra forebodings voiced in my early-February visit. xx In the same Nature for February at pages 239-40 there appears the letter-type of communication under the heading "Disintegration of Uranium by Neutrons" by Meitner and O. R. Frisch under respective xxxxxxx academic auspices in Stockholm and Copenhagen. Interestingly, the same issue contains a leading article which features the Hahn-Strassmann experiments of the turn of the year, 1938-1939, based on two reports in Naturwissenschaft: "Isotopes of the Alkaline Earth Metals from Uranium." The issues received also contain report by Professor Otto

Hahn and F. Strassmann: "Chemical Properties of the Trans-Fission Elements."

Read the paper which had the sympathetic audience of the Chief - the prior week and which had been presented in full before the xx same St. John's College on March 10th. Alas, confirmed on the Ides of March through the Nazi seizure of conservative Prague Government and the introduction of a mere satrap, Hacha. The address which became the center and circumference of the Institute conferencings is entitled "Imminence World War in Perspective Accrued Errors and Cultural Crisis of the Xng Interwar Decades." The emergence of the new crucial scientific experimentations needs to be brought to the Chief's attention, but he must remain preoccupied with the shadow activities on the assumption that war Nemesis is avertible - and then will have to remove the self-imposed shackles of the Neutrality Act.

* * *

Further Visit With KBW and FA, Weekend April 21-22, 1939

The Princeton community continues to resound with new disclosures in the scientific world through the medium of Nature. The issue of March 18th contains another communication from L. Meitner and O. R. Frisch entitled "Products of the Fission of the Uranium Nucleus." The sensation of the issue, however, to the scientists of the Institute and Princeton University, xxx especially the xxxxxxxxx refugee Professor Wigner, is the communication from the Joliot-Curie group. His collaborators as listed are Dr. L. Kowarski and Hans von Halban, Jr. (the extraordinary mixture of man-of-the-world, well-married to a Rothschild and inventive experimenter). Their communication from the Radio Laboratoire de Chemie Nucliaire is entitled "Liberation of Neutrons from the Nuclear Explosion of Uranium." Wigner xxxxx thinks the Joliot-Curie group further advanced than any in the world - in view of Fermi's continued skepticism about reinforcing cumu- xxxx lative energy processes.

Origins of Concern with Significance of Atomic Research for US Defense and for U.S. Role in the Overwhelming World Crisis from Beginning/39

a

(Frank Aydelotte)

Week End February 3-4 1939

Meeting with Frank Aydelotte Director (successor of Abraham Flexner) Institute for Advanced Study Princeton NJ

Also with [illegible] Warren Weaver [illegible] the Institute

Continued my Cassandra warning of imminence around the [illegible] of [illegible] Nazi-timing [illegible] of [illegible] Czechoslovakia [illegible] knocked the props [illegible] ramshackle Munich "Settlement". Testing of idea of imminent outbreak world war around August September — in connection forthcoming Report for [illegible] arranged early March —

The approaching all-encompassing eclipse civilized life by a [illegible] technological war with unspeakable Nazi cruelty, contrasts in mystifying [illegible] with hints [illegible] scientific revolution through [illegible] occurring in atomic physics. Under spell of [illegible] [illegible] Background Modern Science (Cambridge University Press 1938) which [illegible] the

century [illegible] that I had [illegible] 1936 — [illegible] what [illegible] calls "the [illegible] the [illegible] human mind". That applies to himself and not only to those giants but to the whole [illegible] working [illegible] in atomic physics, many [illegible] scattered [illegible] countries and struggling to get laboratory posts and to [illegible] the [illegible] from the Establishment [illegible] science [illegible], as already [illegible] from people [illegible] the refugee scientists.

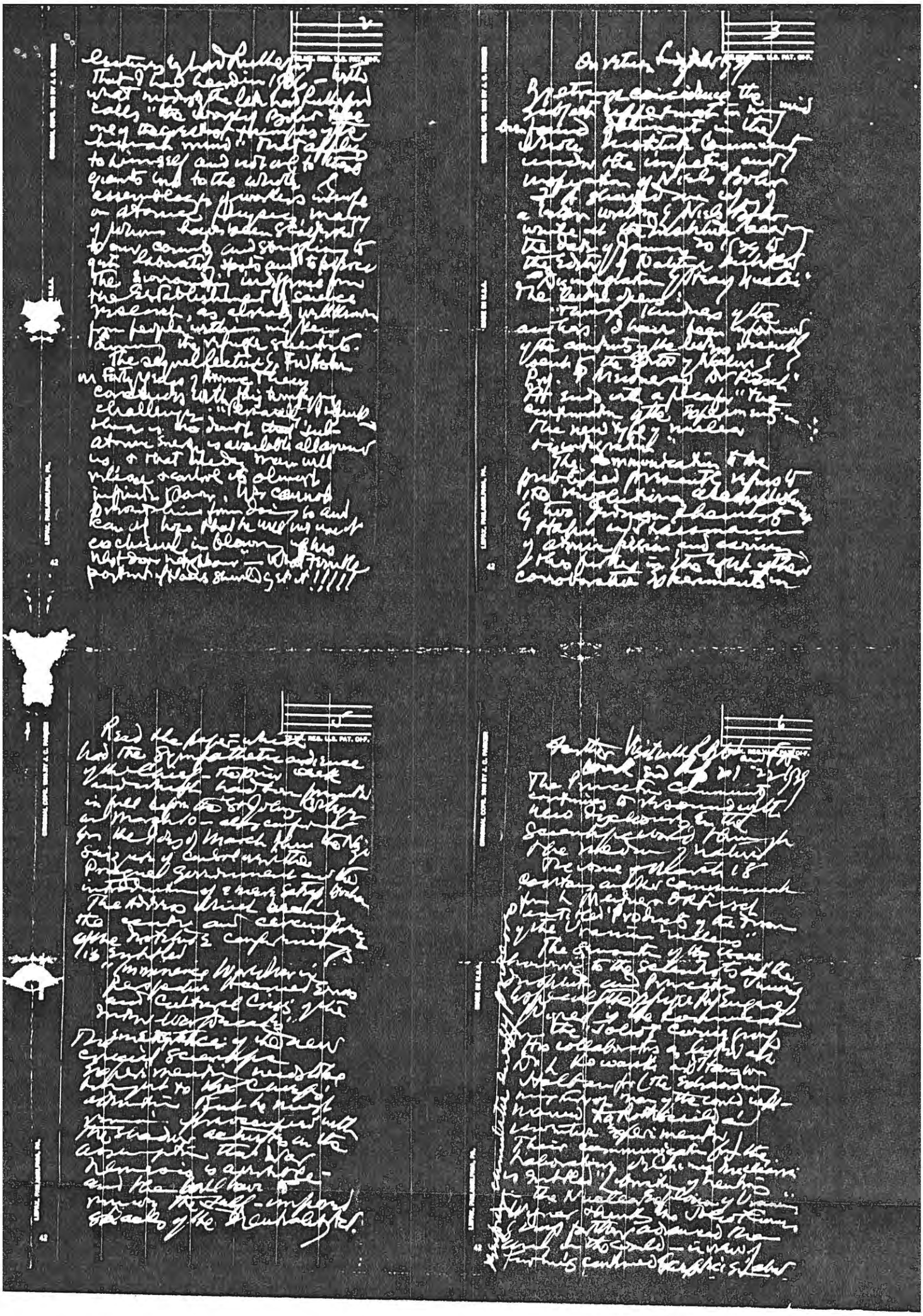
The [illegible] in Forty Years [illegible] concludes with this [illegible] challenge: "[illegible] there is the doubt that [illegible] atomic energy is available all around us & that [illegible] man will release & control its almost infinite power. We cannot prevent him from doing so and can only hope that he will not use it exclusively in blowing up his next door neighbour — What [illegible] portent of Nazis should get it!!!!!

Read the paper — [illegible] and the sympathetic audience [illegible] had been [illegible] in full [illegible] March 10 — [illegible] for the Ides of March [illegible] seizure of control [illegible] Protectorate government and the introduction of a new [illegible] The [illegible] the [illegible] and [illegible] of the [illegible] is entitled "Imminence World War in Perspective [illegible] and Cultural Crisis of the [illegible] War Decade".

The [illegible] of the new [illegible] scientific experiment needs [illegible] to the [illegible] [illegible] But [illegible] the [illegible] in the assumption that War [illegible] is [illegible] — and [illegible] [illegible] the [illegible] of the [illegible].

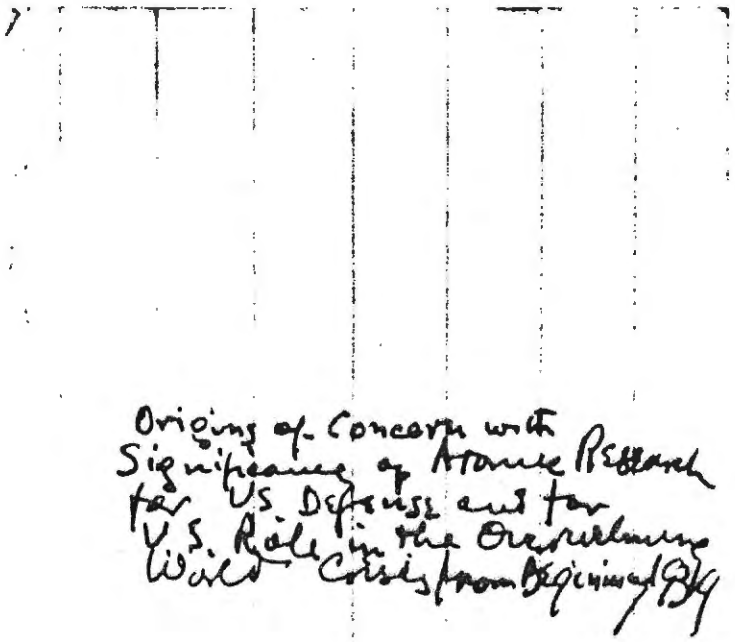
To Stockholm Institute

Following up Week End Visit February 7-18 at the Institute [illegible] received copies of Nature containing the Meitner-Frisch communication [illegible] of a [illegible] the fulfillment [illegible] Cassandra [illegible] in my [illegible] [illegible] [illegible] [illegible] the [illegible] of communication under the Heading "Disintegration of Uranium by Neutrons" by [illegible] [illegible] in Stockholm [illegible] Interesting [illegible] contains a [illegible] article which features the Hahn-Strassmann experiments of the [illegible] 1938-1939 [illegible] in Naturwissenschaften "Isotopes of the Alkaline Earth [illegible] from Uranium". The [illegible] [illegible] Prof. Otto Hahn & Strassmann "Chemical Properties of the [illegible]".



Origins of Concern with
Significance of Atomic Research
for U.S. Defense and for
U.S. Role in the Overwhelming
World Crisis from Beginning 1944

[Feb. 1939]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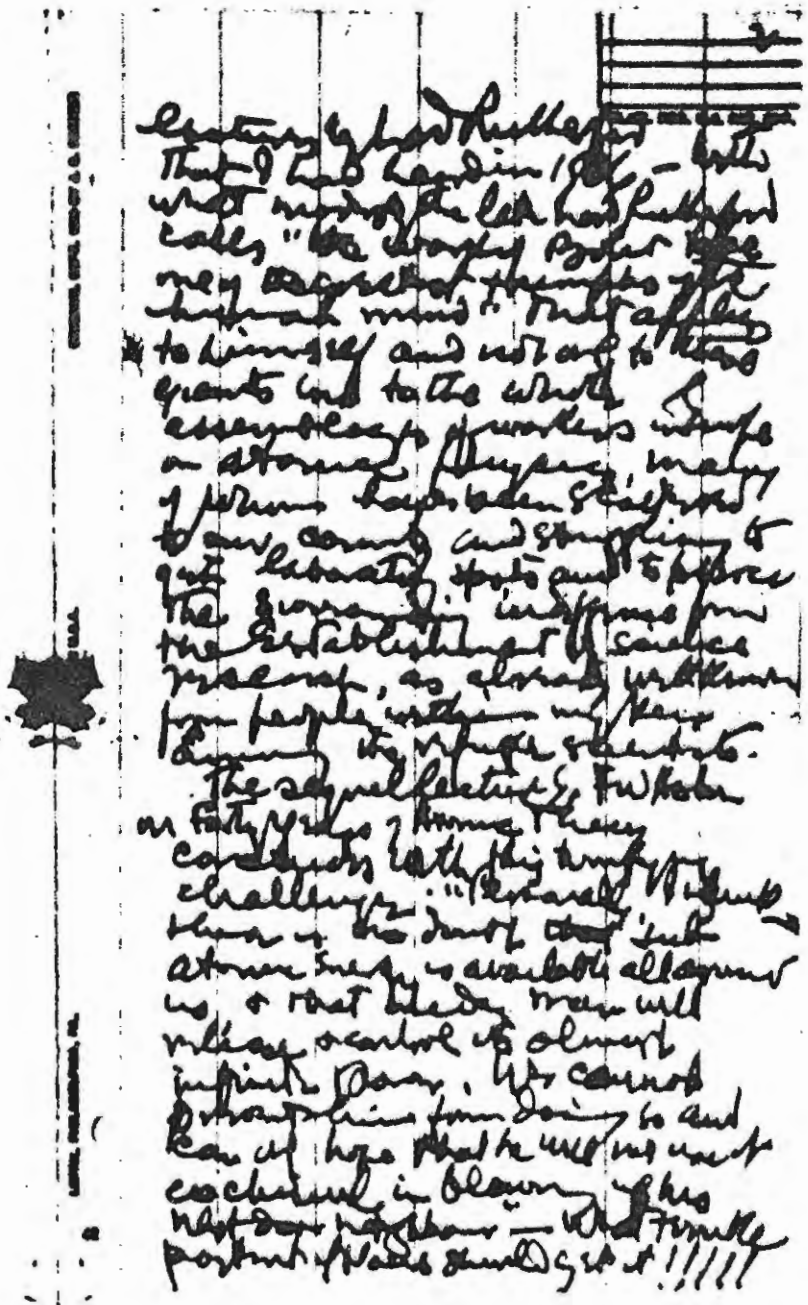
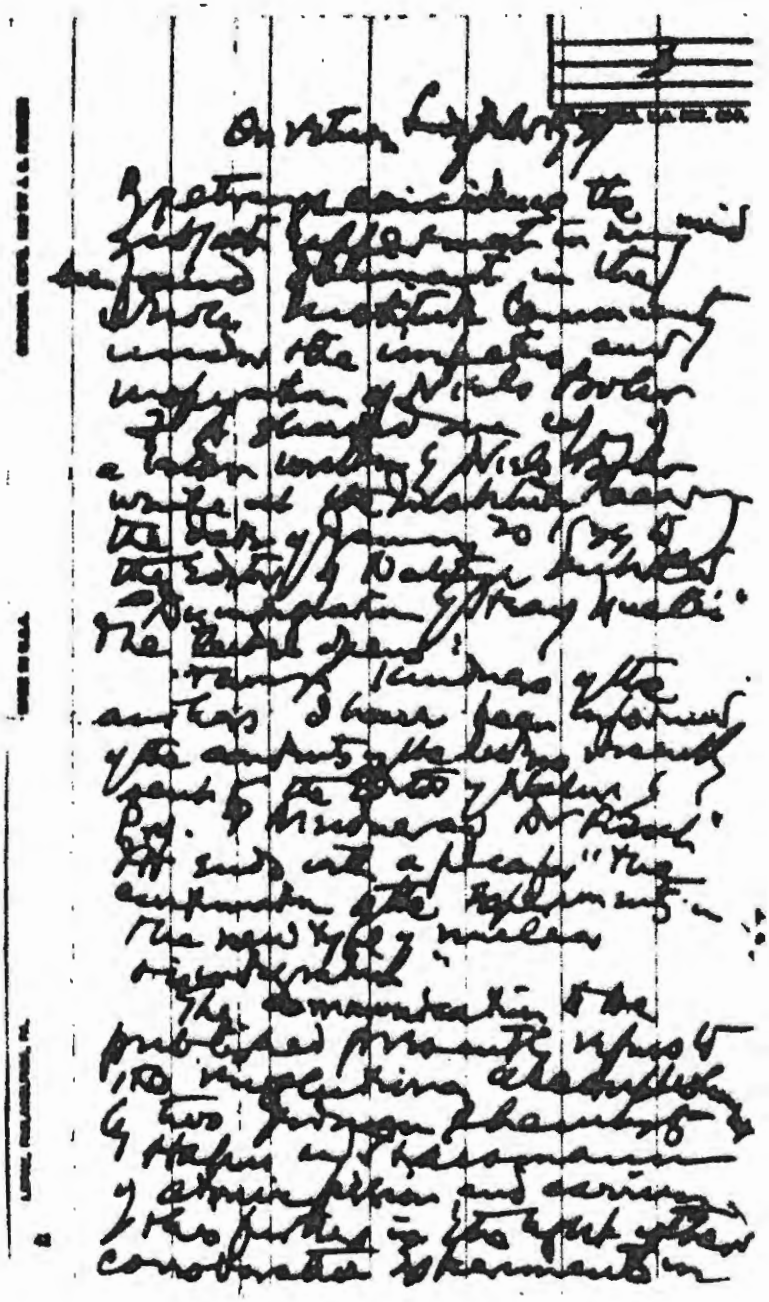
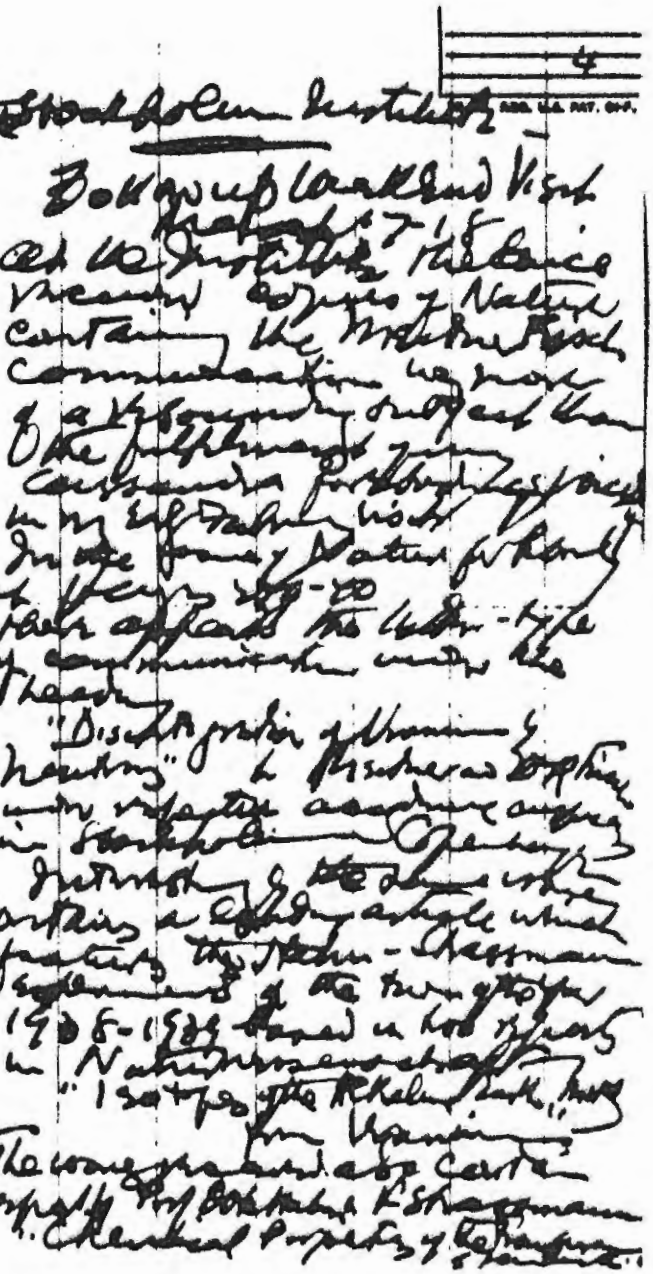
Institute for Advanced Study
Princeton NJ
Also with [illegible]
[illegible] the Institute

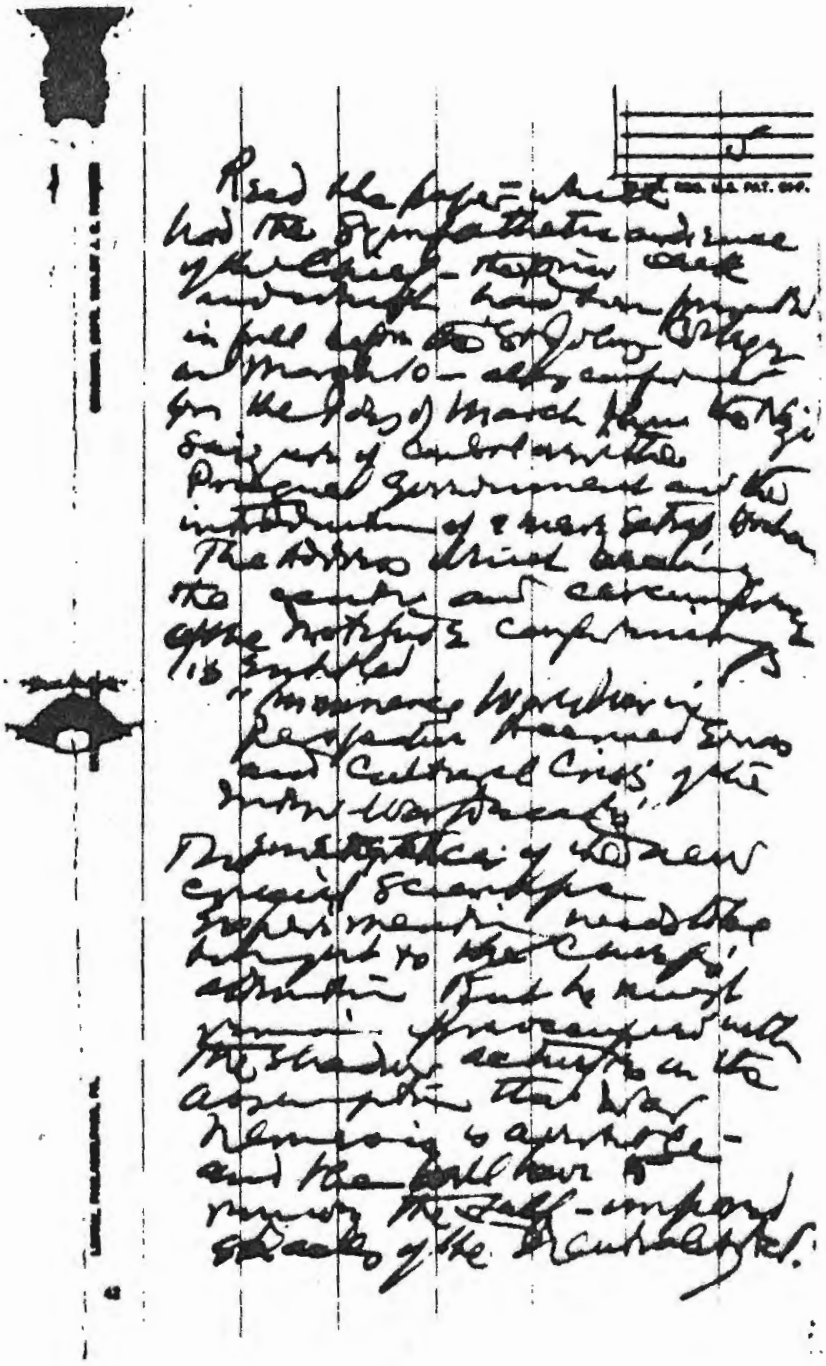
Continued my Cassandra warning of [illegible] around the [illegible] Nazi-timing happening [illegible] Coup in Czechoslovakia, [illegible] knocking the pins [illegible] unworkable Munich "Settlement". Testing of idea of imminent outbreak [illegible] around August September — in connection [illegible] forthcoming Report for [illegible] arranged [illegible] -

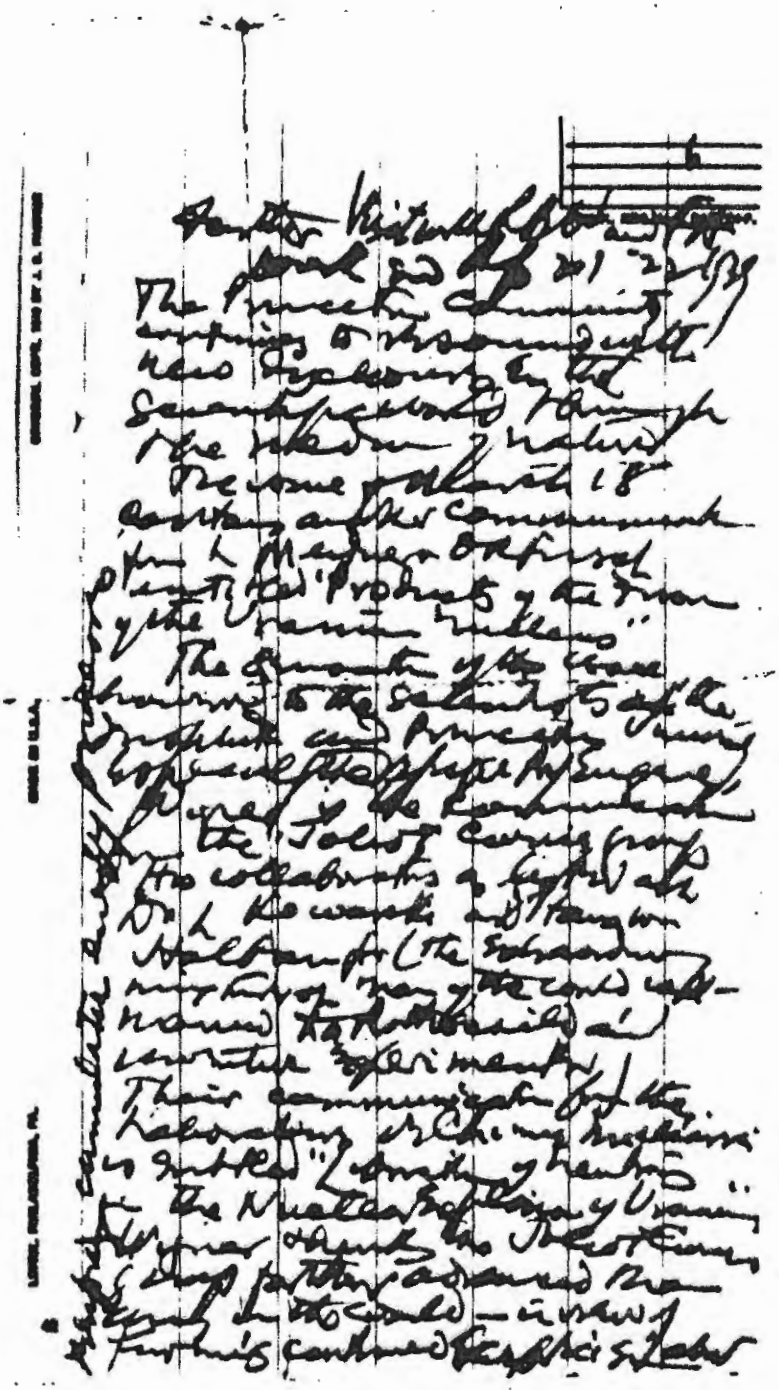
The approaching all-encompassing eclipse [illegible] life by a [illegible] technological war with unforeseeable ways [illegible] resulting in mystifying [illegible] with hints [illegible] scientific revolution through [illegible] occurring in atomic physics. Under spell of [illegible] book Background of Modern Science (Cambridge [illegible] 1938) [illegible] the











[Chem 8/10/45]

Exhibit 3a

V. FORTY YEARS OF ATOMIC THEORY

by

F. W. ASTON

Fellow of Trinity College, Cambridge

ATOMIC THEORY

I SHOULD like to state first that I am not responsible for the title of the lecture "Atomic Theory", but fortunately a good deal of that has been covered by Lord Rutherford in the earlier lectures, so I shall confine myself to that part of the subject dealing with elements and atomic weights.

When I started to learn chemistry in the early 'nineties my teachers were very confident when they spoke of elements and atomic weights. They had no doubt what they meant when they told me that the atomic weight of oxygen was 16, chlorine 35·5, magnesium 24·3 and hydrogen 1·008. The reason for this confidence arose from complete trust in Dalton's atomic theory of 1803. Dalton was a Manchester chemist, and he had put forward a theory that contained the famous postulate that atoms of the same elements were similar to one another and equal in weight. Shortly after that an Edinburgh physician made the suggestion that all atoms were made of the same primordial atoms of a substance which he called "protyle" and which he endeavoured to identify with hydrogen. The physician was Dr Prout, and he had said that the combining weights of all the elements should be whole numbers, but when the chemists examined these they found it was quite impossible that both theories should be right. The combining weights of the elements were found to be fractional, and they had to drop one or the other. They

chose to work with the sound working hypothesis of Manchester, rather than with the more philosophical speculations of Edinburgh. An illustration of "what Manchester thinks to-day the world will think to-morrow"; it went on thinking so for something like a hundred years. During that time atomic weights were determined with greater and greater precision, and that important pioneer, Stas, did wonderful work in discovering the accurate atomic weights of chlorine, hydrogen and other elements. But in 1886 Crookes suggested that it was just possible that Dalton's postulate might not be true, and in his presidential address to the British Association at Birmingham said:

> I conceive, therefore, that when we say the atomic weight of, for instance, calcium is 40, we really express the fact that, while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42, and so on.

Later, he developed this idea in connection with his pioneer work on the rare earths. He called the components "meta-elements", but unfortunately for his reputation as a prophet the experimental results on which his idea was founded were later proved to be fallacious, and Dalton's postulate was reinstated as an article of scientific faith more firmly than ever.

Dalton's postulate cannot be tested in general by chemical methods, for the smallest quantity of a substance of use in chemical operations contains countless myriads of atoms. I propose with the model I have in front of me to give you some idea of the extreme smallness of

the atoms when we reach the stage at which further division will alter their properties, and they remain the atoms of the substance no longer. The substance I shall take for my example is lead. You may assume that this cube for the purposes of the lecture is made of lead. It is a 1 dcm. cube. The section will be made by means of the infinitely sharp knife in three dimensions in such a manner that the first cube formed is half the linear dimensions, and one-eighth the volume of the original cube. I will now repeat the operation in exactly the same way, and we reach what I shall call the second cube of the series; again repeating the operation with the model, I shall reach the third cube of the series. What I want you to notice is the extreme rapidity with which that series diminishes. Each time you get only half the linear dimensions, and one-eighth the cubical dimensions. The question is how long can we go on repeating this operation. Well, I cannot go on very far in actual practice with models, because the results would become invisible; but I can carry on the series to an indefinite degree by means of lantern slides. In Fig. 1 are shown the eleventh to the fifteenth cubes of the series, and to compare their sizes you have a few familiar objects drawn to scale. I may say in this series you reach the limits of accuracy of several means of analysis. The chemical balance will fail at the ninth cube, which does not figure on this at all. The quartz micro-balance will fail at the fourteenth, though it is capable of detecting one-millionth of a milligram. Spectrum analysis fails at the fifteenth; but the surprising thing is that with the

ordinary microscope one can still see objects smaller than that. That is to say, in the detection of minute particles of matter spectrum analysis is not so sensitive

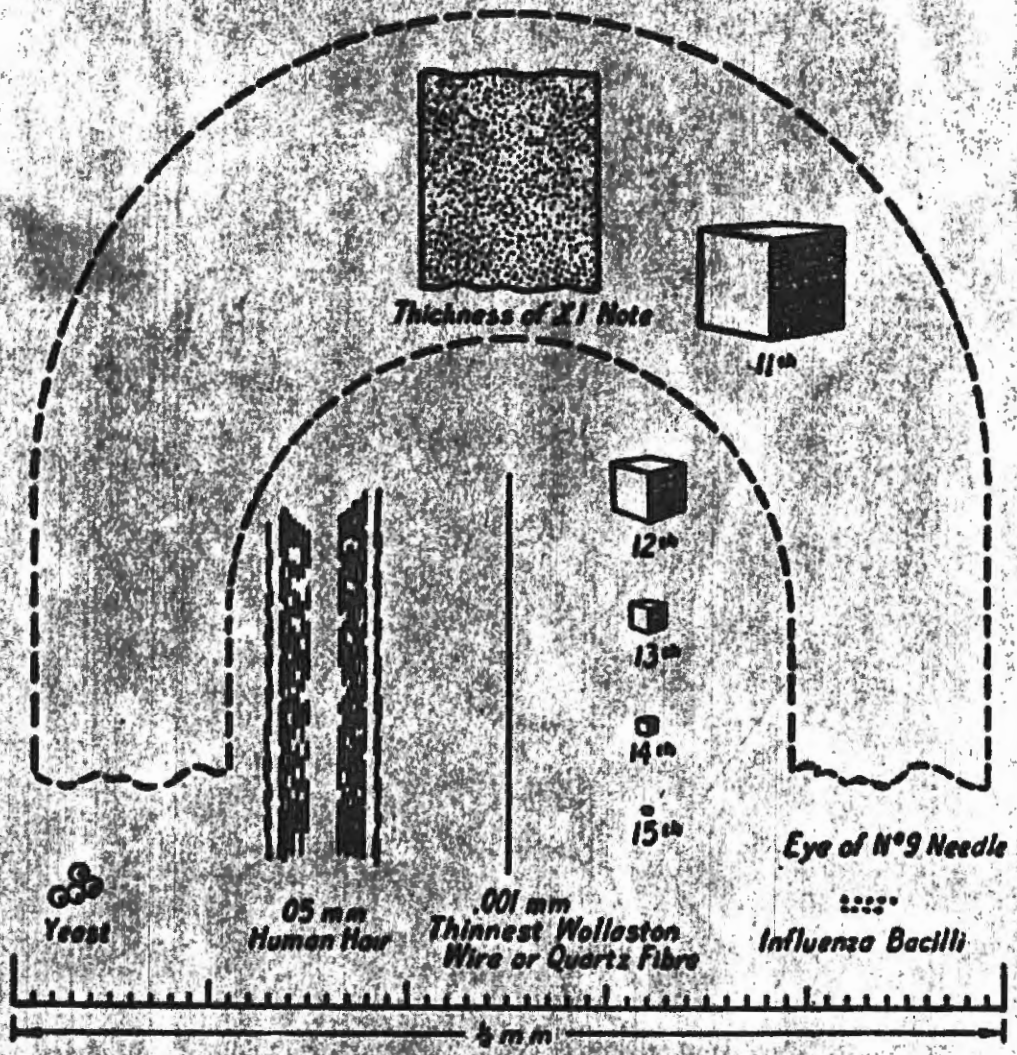


Fig. 1.

as the ordinary microscope. Fig. 2 shows the seventeenth, eighteenth, nineteenth, twentieth, and twenty-first cubes of the series. Here there is some difficulty in finding the familiar objects. You see the wave-length of cadmium red light represented symbolically is very much bigger

than the oil film above it, and that is the reason the oil film shows its colours. It is obvious that, although we have not reached the atom at this stage, we are never

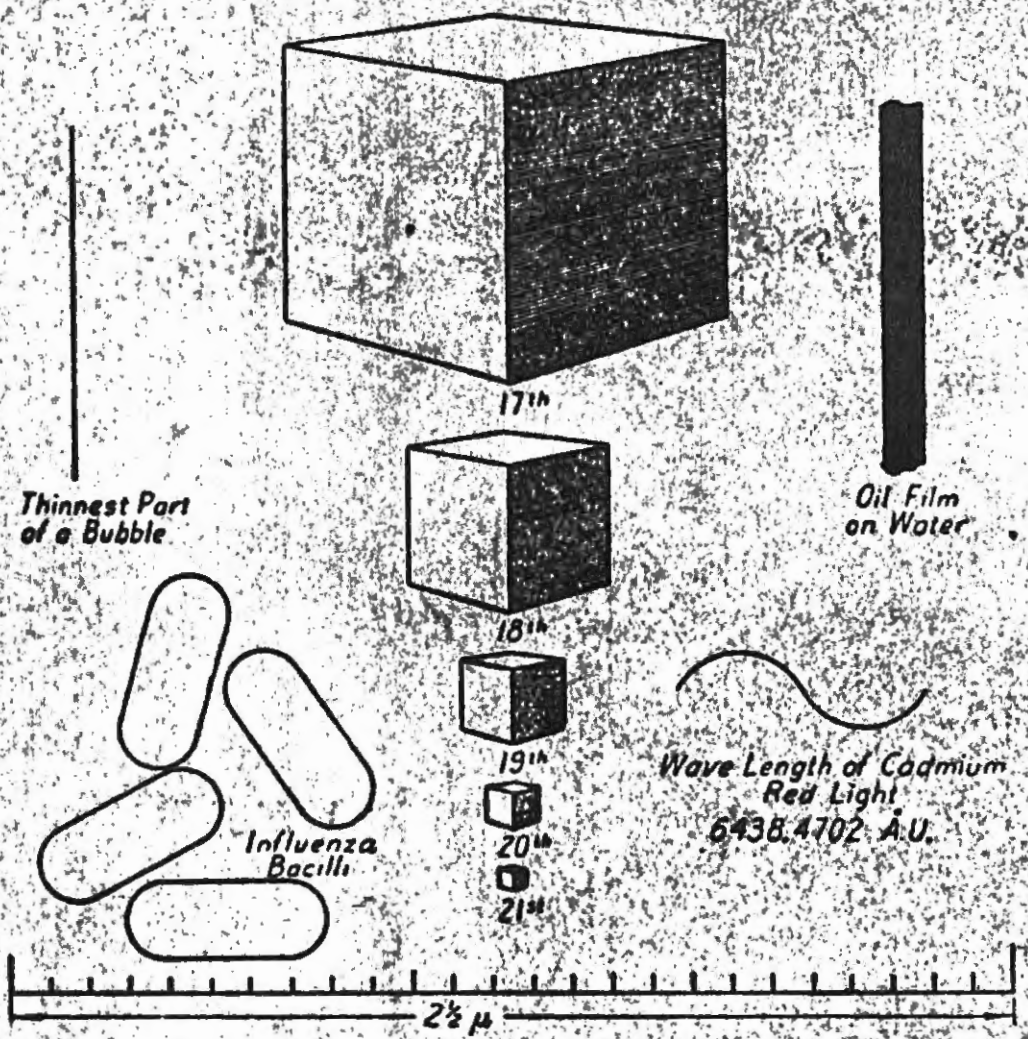


Fig. 2.

going to see it, because this wave-length of light is enormously greater than the atom, so that you can never see this with the eye. Fig. 3 shows the twenty-sixth cube, and you see that two more operations will reduce it to the single atom, so that this series of sections

can be carried on with lead 28 times before the atom of lead is reached. There are no familiar objects to compare on this slide, but up on the right you see drawn to scale

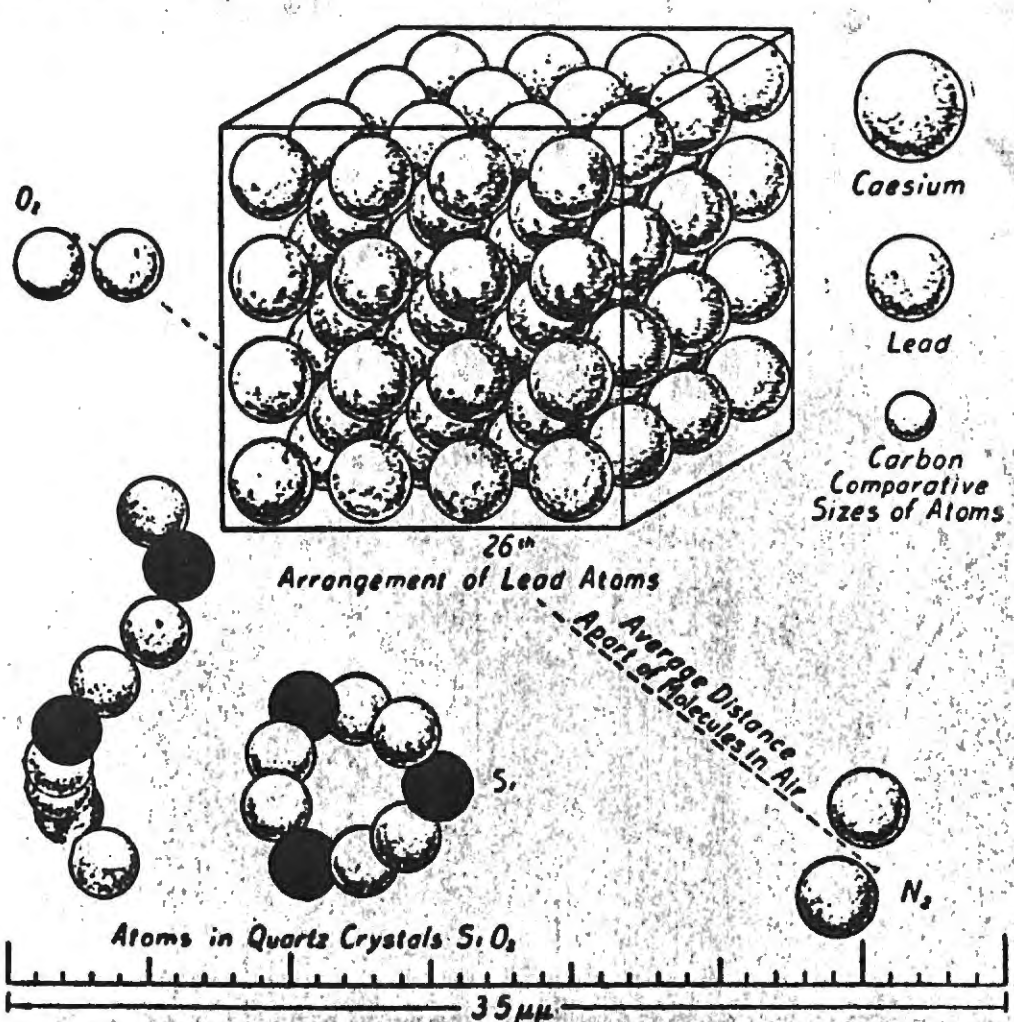


Fig. 3.

spheres representing the largest atom known, that of caesium, the smallest, that of carbon, and one of lead intermediate between these two. There are also represented on the same scale two molecules of air; a molecule of nitrogen, and a molecule of oxygen at their average

distance apart in the air we breathe. There are also representations of the curious spiral form of the atoms of silicon and oxygen in quartz, which gives some indication for the reason of rotation of polarized light. Our definite knowledge of atoms, their size and position, and so on, is obtained almost entirely from the work of the Braggs and others on crystal analysis by X-rays. To give you some idea of the numbers of these atoms is difficult, as the numbers are so colossal. If the atoms in the original decimetre cube of lead were all put into a chain side by side the same distance apart as they are in the normal lead, the strings of atoms so formed would reach over six million million miles. A better idea is given by the fact that suppose you make a hole in an ordinary evacuated electric light bulb and allow the air molecules to pass in at the rate of 1,000,000 a second, the bulb will become full of air in approximately 100,000,000 years. Perhaps the most impressive illustration of all is to suppose that you could label the molecules in a tumbler of water. Suppose one was able, by some means, to do this in such a way that you would know them again, and you took the tumbler of water and threw it anywhere you please on the earth, and went away from the earth for a few million years while all the water on the earth, the oceans, rivers, lakes and clouds had had time to mix up perfectly. Now supposing that perfect mixing had taken place, you come back to earth and draw a similar tumbler of water from the nearest tap, how many of those marked molecules would you expect to find in it? Well, the answer is 2000. There are 2000

times more molecules in a tumbler of water than there are tumblers of water in the whole earth.

Dalton's postulate can be attacked experimentally by two entirely different methods. Physically by the development of methods by which the weights of individual atoms can be compared, and chemically by showing that it was possible to have samples of the same element with different atomic weights. The development of these two lines of attack took place about the same time, early in the twentieth century, and since the second and less direct scored the first success this will be considered first.

It was a direct outcome of the discovery of radioactivity in which the effects of individual atoms, as opposed to those of vast multitudes, were observed for the first time. Chemists could examine elements in the actual process of the making. In 1906 Boltwood observed that his newly discovered element ionium was so similar to thorium that if, by chance, their salts became mixed it was impossible to separate them by any chemical process. Other chemical identities among the products of radioactivity were soon observed and the most painstaking and delicate methods failed to effect or detect the slightest separation.

Discussing these, Soddy, in 1910, boldly stated:

> These regularities may prove to be the beginning of some embracing generalization, which will throw light, not only on radioactive processes, but on elements in general and the Periodic Law. . . Chemical homogeneity is no longer a guarantee that any supposed element is not a mixture of several of different atomic weights, or that any atomic weight is not merely a mean number.

The generalization underlying his views was the law connecting radioactivity and chemical change, in the discovery and enunciation of which he played so prominent a part. This law asserts that a radioactive element when it loses an alpha particle goes back two places in the periodic table; when it loses a beta particle it goes forward one place. It follows that by the loss of one alpha particle followed by two beta particles, the atom, though weighing four units less, will have regained its nuclear charge and returned to its original place.

Such changes result in bodies to which Soddy applied the following words:

> The same algebraic sum of the positive and negative charges in the nucleus when the arithmetical sum is different gives what I call "isotopes" or "isotopic elements" because they occupy the same place in the periodic table. They are chemically identical, and save only as regards the relatively few physical properties which depend upon atomic mass directly, physically identical also.

This theory of Isotopes received the strongest criticism from all sides; it seemed so completely against the generally accepted facts. Particularly the idea that atoms of different weights could have identical spectra was extremely repulsive to orthodox physicists. Fortunately it was possible to put these revolutionary views to an experimental test in the case of one element—lead, the final inactive product of the thorium and uranium transformations. Uranium of atomic weight 238 loses eight alpha particles to become lead of atomic weight 206, while thorium of mass 232 loses six to become lead

of atomic weight 208. Soddy maintained that the lead found in uranium minerals should be lighter, and that in thorium minerals heavier, than ordinary lead of atomic weight 207·2, and by 1914 had satisfied himself that this actually was so.

We will now look at the subject from the other and more general point of view, namely the measurement of the masses of the individual atoms. In order to weigh an atom we must give it a charge of electricity. This is most conveniently done by the electric discharge through gas at low pressure. In the intense field in front of the cathode of the discharge tube the atoms are broken up or "ionized". The negatively charged parts fly away from the cathode, forming cathode rays. These are electrons or atoms of the negative electricity, and are the same whatever the elements in the tube. There are also positive rays which travel towards the cathode. These will be the atoms which have had one or more electrons knocked off them and remain with a positive charge, and *these* will be the atoms of the gas you put into the tube. Owing to the very high field in front of the cathode, they shoot right through it, if a hole is provided, and cause a glow in the gas.

It was by this glow that they were first discovered in 1886 by Goldstein, who called them "canalstrahlen". It was more than twenty years before they were successfully analysed by Sir J. J. Thomson, who called them "positive rays" because they carried a positive charge of electricity. In his well-known parabola method of analysis the rays, generated by means of an electric

discharge, after reaching the surface of the cathode enter a long and very fine metal tube. By this means a narrow beam of rays is produced which is subjected to deflection by electric and magnetic fields and finally falls upon a screen of fluorescent material or a photographic plate. The fields are arranged so that the two deflections are at right angles to each other. Under these conditions particles having the same mass but different velocities will strike the target on a parabola, and the position of this parabola will depend upon the mass. When this method of weighing atoms was used, all the results seemed at first to support Dalton's postulate; indeed the appearance on a sensitive screen of a clear-cut parabolic streak, caused by the impact of the atoms of hydrogen, was the first experimental proof that it was in any sense true of any element. Previously it had been purely an article of scientific faith. Hydrogen, carbon, nitrogen, and oxygen, present either as atoms or molecules, gave parabolas in the positions expected, and it was only when the rare gas neon was examined that an anomaly was observed. Neon. however pure, always gave two parabolas, a strong one at 20 and a weak one at 22. Referring to the latter in January 1913, Sir J. J. Thomson said:

> The origin of this line presents many points of interest; there are no known gaseous compounds of any of the recognized elements which have this molecular weight. Again ,if we accept Mendeléev's Periodic Law, there is no room for a new element with this atomic weight....There is, however, the possibility that we may be interpreting Mendeléev's law too rigidly, and that in the neighbourhood

of the atomic weight of neon there may be a group of two or more elements with similar properties, just as in another part of the table we have the group iron, nickel and cobalt.

It was my privilege to be associated with him in this work, and as his attention was fully occupied with the investigation of a parabola of mass 3—now known to be triatomic hydrogen—it fell to my lot to search for a proof that neon was not homogeneous. This I endeavoured to do by partial separation of its hypothetical constituents, using as a test its density measured by a quartz micro-balance specially designed for the purpose. The first method, that of fractional distillation from charcoal cooled with liquid air, failed, as we now know was inevitable. The second, diffusion through pipeclay, though extremely tedious, had more success and I was able to announce in 1913 that, after thousands of operations, a definite change of density, amounting to about 0·7 per cent, had been achieved. Further data from positive rays were obtained, and, when the war stopped work, there were several lines of reasoning indicating that neon consisted of two bodies of different mass, and that the behaviour of these was exactly that predicted by Soddy for isotopes, but none of these was sufficiently strong to carry conviction on so important a conclusion.

During the war Soddy's prediction concerning the atomic weights of leads from uranium and thorium minerals had been triumphantly vindicated by some of his most severe critics, the experts in chemical atomic weights, and when work was started again, although

I continued for a time to experiment on separation by diffusion by means of an automatic apparatus, I realized that the most satisfactory proof of the existence of isotopes among the elements in general was only to be obtained by much more accurate analysis of positive rays. This was done by means of a sequence of electric and magnetic fields which gave focused images of fine collimating slits, thus forming a spectrum dependent upon mass alone. This I called a "mass-spectrograph". It had a resolving power of about 1 in 130 and an accuracy of mass measurement of 1 in 1000. This was ample to prove in 1919 that neon consisted, beyond doubt, of isotopes 20 and 22, and that its atomic weight 20·2 was the result of these being present in the ratio of about 9 to 1. Chlorine was found to contain 35 and 37, and bromine, of atomic weight almost exactly 80, and hence expected to be simple, gave two equally intense lines 79 and 81. Other elements were shown to be much more complex. Krypton, the first of these, had six isotopes, 78, 80, 82, 83, 84, 86; xenon and tin even more. Of the greatest theoretical importance was the fact that the weights of the atoms of all the elements measured, with the exception of hydrogen, were whole numbers to the accuracy of measurement. This "whole number rule" enabled the simple view to be taken that atoms were built of two units, protons and electrons, all the former and about half the latter being bound together to form the nucleus.

The mass of the hydrogen atom was determined by a special method and proved to be nearly 1 per cent

greater than a whole number. This measurement made by means of the mass-spectograph in 1920 was of far-reaching significance because it proved quite definitely the possibility of sub-atomic energy. It is reasonably certain that the electrical particles forming four atoms of hydrogen are precisely the same as those forming one atom of helium, so that if we were able to transmute one into the other nearly 1 per cent of mass would be annihilated. On the relativity equivalence of mass and energy now experimentally proved, the quantity of energy liberated would be prodigious. Thus to change the hydrogen in a glass of water into helium would release enough energy to drive the "Queen Mary" across the Atlantic and back at full speed.

The discovery of isotopes in the elements generally made a very great change in the significance of atomic weight. I well remember interviewing Sir William Pope on this matter, and he suggested that in a few years time we should be making tons of chlorine 35 and tons of chlorine 37. Thinking of my recent experience with neon I said that I did not think this was at all likely, and so it was decided, rightly or wrongly, that the word "element" should be left undisturbed to be used as it always had been. This decision has been justified, for although sixteen years have elapsed only two elements have been separated into their component isotopes at all completely, in reasonable quantities, the one, hydrogen, is entirely exceptional, and the other, neon, has no chemical significance.

Although the change from the point of view of the

practical chemist is so small, to the philosopher it is profound, as is well illustrated by two quotations I will read. One is from Stas (1860–65):

> I have arrived at the absolute conviction, the complete certainty, so far as it is possible for a human being to attain to certainty in such matters, that the law of Prout is nothing but an illusion, a mere speculation definitely contradicted by experience.

The other is from Soddy (1932):

> After many vicissitudes and the most convincing apparent disproofs, the hypothesis thrown out so lightly by Prout, an Edinburgh physician, in 1815, has, a century later, become the corner-stone of modern theories of the structure of atoms. There is something surely akin to if not transcending tragedy in the fate that has overtaken the life work of that distinguished galaxy of nineteenth century chemists, rightly revered by their contemporaries as representing the crown and perfection of accurate scientific measurement. Their hard won results, for the moment at least, appear as of as little interest and significance as the determination of the average weight of a collection of bottles, some of them full and some of them more or less empty.

Although the interpretation of mass-spectra was often far from simple owing to the difficulty of distinguishing between lines due to compound molecules and those representing true atomic mass-numbers the analysis of the more suitable elements advanced rapidly. Dempster at Chicago discovered the isotopes of magnesium, calcium, and zinc by means of an instrument of his own design with semicircular magnetic focusing. By 1925, when I replaced my first mass-spectograph, now in the Science Museum, South Kensington, with one

of higher resolving power, information on the isotopic constitution of more than half the elements had already been obtained. The new instrument was designed primarily for measuring the minute variations of the masses of atoms from the whole number rule, and had a resolving power ample for the heaviest elements. By its means many new isotopes were discovered.

The difficulty of obtaining the necessary rays for analysis varies enormously from element to element. Two main devices are employed: the ordinary gas discharge which requires the element to be volatile or form suitable volatile compounds; and the anode ray discharge, in which the halide or other compound of the element is treated as the anode in a discharge at low pressure. The inert gases are particularly suitable to the first method, the alkali metals to the second, other groups of elements being intermediate. Our knowledge of the mechanism of the discharge in both methods is far from complete, so that working with them is still rather an art than a science. The element of luck has played an important part in cases where the properties of the materials are unfamiliar and unfavourable to the conditions of the discharge. The technique of anode rays is capricious, but when successful, yields spectra almost free from the lines of compound molecules, and is for this reason particularly suitable for the identification of new isotopes. I was able to apply it to my second mass-spectograph in the analysis of the large group of the rare earth elements, which yielded some thirty new isotopes.

From the point of view of the identification of the more abundant isotopes our knowledge is now complete. Two years ago only four elements, palladium, iridium, platinum, and gold, remained, and since then all these have been analysed by Dempster by the use of a new method employing an intense vacuum spark.

In all over 260 stable isotopes are known of which seven were discovered by observations on optical spectra, and have since been confirmed by the mass-spectograph. This large assembly shows many empirical laws, of which perhaps the more remarkable is that no odd numbered element has more than two isotopes. Even elements are not so limited. The most complex element so far observed is tin, with ten isotopes ranging in mass-number from 112 to 124. One of the most astonishing results is that for practically every natural number up to 210, a stable elementary atom is known, many are filled twice over and a few three times with "isobares", that is atoms of the same weight but different chemical properties. Schemes of tabulation of all the known species have led to the prediction of isotopes and to theories of nuclear structure to account for their occurrence.

Study of the relative abundance of isotopes in the mixture we still call, for convenience, an element, is of interest from two entirely different points of view. In the first place, since it appears to be perfectly invariable in Nature, not only in terrestrial but also in meteoric matter, there was a slight hope that a systematic measurement of abundance ratios might disclose some simpler relations bearing on the great problem of how the nuclei

of atoms were evolved. The relative abundance of isotopes can be estimated by several methods, but that of the most general application is the photometry of mass-spectra. A technique of this was worked out in 1929, and a number of elements examined, but the ratios, obtained in numbers large enough for statistical treatment, showed no groupings other than would have been expected from pure chance. These measurements have a second important practical value. If we know the masses of the isotopes of an element and their relative abundance it is easy to calculate their mean weight. This, with proper corrections, can be used to check the chemical atomic weight. During the past six years nearly every atomic weight has been determined by this purely physical method, which has the great advantage of being, in general, independent of purity, and requiring an almost infinitesimal quantity of material.

Instead of the original view that the nuclei of atoms consisted of protons and electrons, it is now considered more likely that they are built of protons and neutrons. In either case the binding forces holding the particles together must represent loss of energy, that is, loss of mass. Hence it is that the atom of hydrogen has abnormally high mass, and that the accurate determinations of divergences from the whole number rule are of such profound theoretical importance. As I have stated, my second mass-spectograph was designed for this and found capable of an accuracy, in favourable cases, of 1 in 10,000. The atom of oxygen 16 was chosen as standard and the percentage divergences, expressed in

parts per 10,000 called "packing fractions", were determined for a large number of elements. These, when plotted against mass-number, were found to lie roughly on a hyperbolic curve. This drops rapidly from hydrogen, passes through a minimum of about −10 in the region of iron and nickel, and then rises gradually, crossing the zero line in the region of mercury. Our knowledge in this field has been notably increased by the brilliant work of Bainbridge, who set up at Swarthmore a powerful mass-spectograph of an original design which made use of a velocity selector and semicircular focusing. With this instrument he discovered new isotopes of tellurium, rectified results on zinc and germanium, and has made many of the most accurate comparisons of mass so far known.

The events which led up to the discovery of the remarkable isotope "heavy hydrogen" are of particular interest. The first accurate comparisons of the masses, now termed "isotopic weights" of the atoms ^{1}H, ^{12}C, ^{14}N with the standard ^{16}O were made with my second mass-spectograph and published in 1927. The mass of ^{1}H could only be obtained indirectly through the intermediate mass ^{4}He, and was given as 1·00778. This and the others agreed very accurately with the atomic weights of the elements obtained by chemical means.

This satisfactory agreement was completely upset in 1929 by the startling discovery of the heavy isotopes of oxygen 17 and 18 which, present in small quantity, had naturally been overlooked on mass-spectra of that element owing to the technical difficulty of ensuring the

absence of the isobaric compound lines OH and OH_2. The discovery was made by Giauque and Johnson by observations on band-spectra, which are free from this confusing disability, and the careful quantitative work of Mecke, made later, showed that, owing to the presence of these isotopes, the chemical standard of atomic weight O = 16 was about 2 parts in 10,000 heavier than the physical one $^{16}O = 16$. Examination of compounds of carbon and of nitrogen by the same method showed not only that these elements also contained heavy isotopes ^{13}C and ^{15}N but that their apparent abundance, by a most incredible coincidence, was just about enough to bring their mean weights into line with that of oxygen.

Birge pointed out that to satisfy my low estimate of ^1H hydrogen must also contain at least one heavy isotope. Urey took up the problem and, happily unaware of the real uncertainty in the figures concerned, with the collaboration of Brickwedde and Murphy fractionated liquid hydrogen and proved by examination of the Balmer lines that ^2H was present. Washburn showed that its heavier atoms could be concentrated by the electrolysis of water. This method was developed so rapidly and brilliantly by Lewis that, soon after its discovery, pure heavy water had been obtained in appreciable quantity. The isotope of hydrogen of mass 2 cannot be treated as a normal isotope. Its exceptional difference in mass enables it to be separated with comparative ease in a pure state. It has been given the name deuterium, symbol D, and heavy water D_2O is now

obtainable in quantity at reasonable prices, one of the most surprising reagents in the history of science and certainly one which would have dismayed the founders of the C.G.S. system of units.

Now that deuterium is available the mass of ^1H can be measured with far greater certainty by the "doublet" method and turns out to be 1·00812. Now had that value been obtained at first it is quite possible that no one would have looked for heavy hydrogen, so it was very fortunate that the mistake was on the right side.

The only moral to be drawn from this seems to be that you should make more, more and yet more measurements. Even a bad one *may* be of service, but, fortunately, it will be essential for you to make a considerable number of good ones first, or no notice will be taken of it.

In recent years the accuracy of measurement has been steadily improving. In my third instrument which has second-order focusing, and in a still more powerful double focusing mass-spectrograph constructed by Bainbridge at Harvard, it approaches 1 in 100,000. The need for this high accuracy is in the recently discovered artificial transmutations, the nuclear chemistry of the future. The equations of this can only be founded upon accurate knowledge of the masses concerned. Armed with such knowledge the nuclear chemists, I am convinced, will be able to synthesise elements just as ordinary chemists synthesise compounds, and it may be taken as certain that in some reactions sub-atomic energy will be liberated.

There are those about us who say that such research should be stopped by law, alleging that man's destructive powers are already large enough. So, no doubt, the more elderly and ape-like of our prehistoric ancestors objected to the innovation of cooked food and pointed out the grave dangers attending the use of the newly discovered agency, fire. Personally I think there is no doubt that sub-atomic energy is available all around us, and that one day man will release and control its almost infinite power. We cannot prevent him from doing so and can only hope that he will not use it exclusively in blowing up his next door neighbour.

[Str Ecm. Extracts] [Atomics Subj.] S

ON THE EXISTENCE AND BEHAVIOUR OF ALCALINE EARTH METALS ARISING FROM EXPOSURE OF URANIUM TO NEUTRONS

by

O. Hahn and F. Strassmann, Berlin-Dahlem[1)]

(Die Naturwissenschaften, 1939, No.1, pp.11-15)

translated by E. J. Gumbel.

In a recent preliminary note[2)] published in this journal, it was stated that the exposure of uranium to neutrons gives rise to a series of transformation products apart from those trans-uranium elements - 93 to 96 - described in detail by Meitner, Hahn and Strassmann. The formation of these other transformation products is evidently due to a successive radiative alpha dissociation of a transient uranium 239. By such a dissociation the element with a nuclear charge 92 gives rise, by necessity, to an element with a nuclear charge 88 which means, a radium. In the note cited above, a scheme of dissociation, meant as yet preliminary, stated the existence of three isomeric isotopes of radium and their transformation products, namely three isomeric isotopes of actinium which, themselves, obviously turned into thorium. The half lives of these three isomeric isotopes of radium were estimated approximately.

At the same time, attention was drawn to the unexpected observation that the radium isotopes transformed from thorium with separation of alpha radiation are created not only through

1) From the Kaiser Wilhelm Institute for Chemistry in Berlin-Dahlem. Received December 22, 1938

2) O. Hahn and F. Strassmann, Naturwiss. 26, 756 (1938)

~~radiation are created not only through~~ quick, but also through retarded neutrons.

The conclusion that the initial members of these three new isomeric series are isotopes of radium was based on the fact that these substances may be isolated through the use of barium salts, and show all reactions proper to the element barium. All other elements we know of, starting from the trans-uranium elements to uranium, protactinium, thorium till actinium, have other chemical properties than barium and may easily be separated from it. The same applies to the elements below radium, as for instance Bismuth, lead, Polonium, Ekacäsium. Therefore, ~~regardless of~~ disregarding the barium itself, only radium remains as a possible choice.

In the following, a short description of the isolation of the isotope mixture and the extraction of the different members is given. From the course of activity of the different isotopes follows their half life and the different sequences of products generating in the process. The latter shall not yet be described in detail in this note because the half lives

[illegible] products have not yet been fully stated by [illegible] of the very complex ~~procedure~~ processes involved. ~~Presumably~~ At least 3, probably even four, series containing three substances each, are involved.

Naturally, barium was always used as the bearer substance for the "Radium isotopes". The most obvious procedure was the precipitation of barium as barium sulphate, the least soluble barium salt besides chromate. According to previous experiences, and after some preliminary experiments, the procedure of isolating the "Radium isotopes" by barium suphate was given up because these precipitations carry along, besides small amounts of uranium, also not negligible amounts of actinium- and thorium isotopes, and consequently also the probable transformation products of the radium isotopes. Therefore they do not allow a preparation of the initial members in a pure state. Instead of the quantitative precipitation by sulphate ~~reacting~~ concentrated mainly on the surface, barium chloride which is difficult to solve in concentrated hydrochloric acid was chosen as precipitation agent, ~~because of its difficult solubility in concentrated hydrochloric acid~~, a method which proved to be highly efficient.

Since the formation of radium isotopes from uranium

by exposure to slow neutrons is not easily understood from the standpoint of energy, it was imperative to determine with extreme thoroughness the chemical character of the newly generated artificial radio-active elements. By separating individual analytic groups of elements from the solution of the exposed uranium, activity was always found, apart from the large group of trans-uranium, among the alcaline earth metals (bearer substance Ba), the rare earths (bearer substance La) and among the elements of the fourth group of the periodic system (bearer substance Zr). First, the barium precipitations obviously containing the initial members of the observed isomeric series, were investigated more thoroughly.

It will be shown that trans-uranium, uranium, protactinium, thorium and actinium may always be easily and completely separated from the activity arising from barium.

1) To this end, the trans-uranium elements were separated together with platinum sulphide from an exposed uran by means of hydrogene sulphide, and dissolved in nitromuriatic acid. From this solution barium chloride was precipitated by hydrochloric acid. From the filtrate of barium sediment, platinum was precipitated again by hydrogene sulphide. The barium chloride was

inactive. The platinum sulphide still had an activity of about 500 particles per minute. Corresponding experiments with trans-aranium substances ~~with~~ having longer half lives lead to the same results.

2) A precipitation of barium chloride from ten grams of unexposed uranyl nitrate which was in equilibrium with $UX_1 + UX_2$ (thorium and protactinium isotopes) and has an activity of about ~~4000~~ 400 000 particles per minute, showed an activity of about 14 particles per minute and thus was practically inactive. This means that neither uranium nor protactinium nor thorium are precipipated with crystallized barium chloride.

3) Finally barium chloride was precipitated from a solution of an actinium preparation ($MsTh_2$) of about ≥500 particles per minute. The sediment showed an activity of about ~~2~~ 3 particles per minute thus being practically inactive too.

In a similar way the very active sediments of barium chloride precipitated from radiated uranium were carefully examined. Yet the sulphide precipitations from neutral weakly acetic or weakly mineral acedious solution of active

barium were practically inactive whereas the Lantnanum-and Zirkon precipitations showed activities the origin of which was easily proved to be due to the activity of barium precipitations.

The simple precipitation by $BaCl_2$ from concentrated hydrochloric acid solution does of course not allow any distinction between barium and radium. In concequence of these reactions, mentioned here only in a summary way, the activity separated from barium salts can only be due to radium, if the barium itself is neglected for the time being on account of its improbability.

We shall now briefly enter into the examination of the activity curves due to barium chloride. These curves allow, on the one hand, to state the number of "radium-isotopes" and, in addition, the determination of their half lives.

Graph (1) shows the course of activity of the active barium chloride after 4 days of exposure of uranium. Curve (a) gives the measurements during the first 70 hours; Curve (b) shows the measurements of the same preparation continued during 800 hours. The scale of the lower curve is 10 times smaller than the scale of the upper curve. The initial quick [illegible] becomes gradually slower and is transformed, after

about 12 hours, into a slow increase. After about 120 hours a very gradual decrease of activity starts. It occurs exponentially with a half life of about 13 days.

The shape of these curves clearly shows that there are several substances involved. However, it is impossible to state without further investigation what these substances are: whether several "radium-isotopes" or one "radium-isotope" with a series of sequence-products determine this activity.

Let it be stated in anticipation that the three isomeric radium isotopes mentioned in the first note were confirmed. Let them be called RaII, RaIII, and RaIV (concerning a presumed RaI, see below). The proof of their existence and the determination of their half lives is briefly shown by means of the following graphs.

Graph (L) shows the analysis of a declining curve of "Radium" after 6 minutes' exposure of uranium. Curve (a) gives the activity directly measured during 218 minutes. It is composed of the activity of 2 "Radium"-isotopes RaII and RaIII

(see Graph 3) and a small actinium activity arising from RaI. This substance called AcII has a half life of about 2½ hours as shown by other experiments into which we cannot enter here. The theoretical curve of increase of such an actinium isotope arising from RaII is shown in the graph as curve (b). A value of 14 minutes has been anticipated as the half life of RaI. Curve (c) is obtained by subtraction of the values of curve (b) from those of curve (a). This activity practically originates only from radium isotopes, and mainly from the short-lived RaII, and, in a minor degree, from the longer-lived RaIII. The latter has a half life of about 86 minutes as shown later in Graph (2). The activity of RaIII is shown in curve (d) of Graph (2). The activity curve (e) of the pure RaII is obtained by subtraction of curve (d) from curve (c). The decrease follows an exponential law with a half life of 14 minutes. This value may be maintained exact within a limit of about ± 2 minutes.

We now enter into the proof of existence of RaII and into the determination of its half life. If an uranium preparation is radiated for 1 hour or a few hours, there will be

found, apart from an initial quick decrease of activity, a very considerable activity with a half life of about 100 to 110 minutes again diminishing ~~disintegrating~~ toward the end. In order to prove that this activity likewise is mainly due to a radium isotope, the following procedure was adopted: From the radiated uranium preparation the "radium" was separated by barium chloride. 2½ hours later, the barium chloride was again dissolved and precipitated anew. The short lived RaII is completely decomposed in this time, and the AcII (2½ hours' half life) originated from the RaII in the barium chloride is eliminated in the process of recrystallisation of barium chloride. The barium chloride is still considerably active. Consequently there exists still another "radium-isotope". The procedure is the same as already used by Meitner, Hahn and Strassmann [1] ~~already~~ in the explanation of the artificially transformed products arising from thorium. The course of activity thus obtained is given in Graph (3) curve (a).

1) L. Meitner, F. Strassmann, and O. Hahn. Z. Physik, 109, 538 (1938)

The decrease occurs, during the first hours, almost purely exponential with about 86 minutes' half life. There remains a small rest of activity. It may be assumed without doubt that it consists of a long-lived actinium isotope originatingfrom RaIII. Its probable course of activity may be concluded from the difference between curve (a) and a purely exponential curve. The resulting curve of activity is shown as curve (b) in Graph (3). (The generating of a relatively long-lived actinium isotope from decomposition of RaIII was also proved chemically). Subtraction of curve (b) from curve (a) yields curve (c) for the RaIII which now is pure. Curve (c) shows a very elegant exponential decrease with a half life of 86 minutes, This value may be considered correct within ± 6 minutes.

We shall now consider the third "Radium-isotope", hence called RaIV. The end of curve (b) in Graph (1) proves the existence of a substance with a half life of about 12 to 13 days. It was proved in a similar way as in the case of RaIII that this slower decrease of activity is mainly due to a

"Radium-isotopp" . The isotopes RaII and RaIII are completely decomposed if an uranium subjected to long exposure remains standing for about a day after the source of neutrons is removed. If a new barium precipitation and, for security's sake, a new recrystallization is made, then a remaining activity of the barium chloride can only be due to a further "radium isotope." Such activities were found even after the substance was left standing for days. The course of activity is very characteristic. The activity increases slowly during several days, reaches a maximum and vanishes with a half life of about 300 hours (12.5 days).

~~In Graph (4)~~ Some of these curves are reproduced in Graph (4).

The preparation represented by curve (c) was eliminated from a uranium preparation exposed to non-intensified radiation. The other curves correspond to barium precipitations from uranium exposed to intensified radiation. (Nothing may be concluded from the curves as to the intensification because the work was not done under the same geometric conditions. Under the same conditions, the same amount of uranium exposed, and so forth, we found an intensification factor of about 7). The course of the

three curves is much alike. During the increasing period, the activity doubles in less than 40 hours. The decrease of activity corresponds to a half life of about 300 hours. Let the half life of this long lived RaIV is, without doubt, less than 300 hours. The generation of AcIV from RaIV is mainly responsible for the increase. The AcIV probably creates a long-lived "thorium isotope" such that the half-life of the RaIV is not determined exactly. A value of 250 to 300 hours is probably near to the true length. The curves (a), (b) and (c) clearly show that the beta radiation of RaIV is more easily absorbed than the radiation of the consequential products. Otherwise the increase could not be so sharp.

Summarizing the preceding results, we have proved the existence of three isomeric alcaline earth metals called RaII, RaIII, and RaIV. Their half lives are 14 ± 2 minutes, 86 ± 6 minutes, 250 to 300 hours. It may have been remarked that the substance with 14 minutes' half life has not been called RaI, and the other isomeric substances have not been called RaII and RaIII. The reason for this is our belief in the existence of "Ra" still more unstable although it has not yet been found.

In our first note concerning the new transformation products

we stated the existence of an actinium of about 40 minutes' half life, and assumed as the most evident hypothesis that this most unstable actinium isotope generates from the most unstable radium isotope. In the meantime we proved that the "actinium" generated from the radium with half life 14 minutes (previously 25 minutes) has a half life of about 2.5 hours (previously claimed to be 4 hours). Yet, the above mentioned more unstable actinium isotope exists too. Its half life is somewhat smaller than previously stated - probably less than 30 minutes. Since this actinium isotope cannot be generated either from the 14 minuts' substance or from the 86 minutes' substance, or from the long-lived "Ra" - since, besides, this "actinium-isotope" can be ~~shown to exist~~ found already after 5 minutes' exposure of uran to radiation - the simplest hypothesis as to its origin is a "radium isotope" the half life of which must be smaller than 1 minute. If its half life would have been longer than 1 minute, we ought to have found it, we tried very hard. Therefore this unknown mother-substance which might doubtless ~~have~~ been found ~~by~~ by means of a more intense

source of radiation is called "RaI."

The scheme given in our first note must therefore be subject to a certain correction. The following scheme takes these changes into account and gives the half lives of the initial members now more correctly

$$\text{"Ra I"}\ ? \xrightarrow[<1\ \text{min.}]{\beta} Ac\,\bar{I} \xrightarrow[<30\ \text{min}]{\beta} Th\ ?$$

$$\text{"Ra }\underline{\text{II}}\text{"} \xrightarrow[14 \pm 2\ \text{min}]{\beta} Ac\,\underline{\text{II}} \xrightarrow[\sim 2.5\ \text{hours}]{\beta} Th\ ?$$

$$\text{"Ra }\underline{\text{III}}\text{"} \xrightarrow[36 \pm 6\ \text{min.}]{\beta} Ac\,\underline{\text{III}} \xrightarrow[\sim \text{several days}\ ?]{\beta} Th\ ?$$

$$\text{"Ra }\underline{\text{IV}}\text{"} \xrightarrow[250-300\ \text{hours}]{\beta} Ac\,\underline{\text{IV}} \xrightarrow[<40\ \text{hours}]{\beta} Th\ ?$$

So far, the large group of transuranium elements has no visible connection with these series.

The transformation series stated in the preceding scheme may, without doubt, be considered correct in their genetic relation. We have also been able to prove the existence of some of the final members which are stated as "Thorium" at the end of the isomeric series. But we refrained from making ~~non-exact~~ statements about their half lives since, so far, it was not possible to determine them ~~respective half lives~~ correctly.

Now we have to mention some new researches which we publish only hesitantly due to their peculiar results. In order to prove beyond any doubt the chemical nature of the initial members of the series which were separated with barium and were called "radium isotopes" , we established fractionate crystallisations and fractionate precipitations of the active barium salts, in the way known for the concentration or deconcentration of radium in barium salts.

Barium bromide strongly concentrates radium at fractionate crystallisation; barium chromate is still more efficient if the small crystals do not originate too quickly. Barium chloride is less concentrating than bromide. Barium carbonate is deconpentrating. Corresponding ~~Similar~~ experiments made with our active barium preparations cleaned from sequential products all led to negative results: The activity remained equally distributed among all barium fractions, at least as far as we may state it within the possible errors of observation which are by no means small. We then experimented with fractionation of the radium isotopes ThX and the radium isotope $Ms.Th_1$. They

gave exactly the same results, as could be expected from prior experiences with radium. Then the "indicative method" was applied to a mixture of purified long lived RaIV with pure $MsTh_1$ free of radium: the micture with barium ~~chloride~~ bromide beare. substabce was fractionatedly crystallised. $MsTh_1$ became concentrated, but not the "RaIV" ; instead its activity remained the same ~~with~~ for the same barium content of the fractions.

We conclude: Our "radium isotopes" have the properties of barium. Speaking as chemists, we ought to say that the new substances are not radium, but barium since other elements ~~but~~ than radium and barium are excluded.

Finally we made an indicative experiment with our purely isolated "AcII" (half life about 2.5 hours) and with the pure actinium isotope $MsTh_2$. If our "Ra isotopes" are no radium, then the "Ac isotopes" are no actinium either, but should be lanthanum. Following the procedure of Mme Curie,[1] we made a fractionation of lanthanum oxalate containing the two active substances from nitrate solution. The $MsTh_2$ was found ~~greatly~~ strongly

1) Mme Pierre Curie, J. Chim. physique etc. 27, 1 (1930)

concentrated in the final fractions as indicated by Mme Curie. But no concentration at the end could be found with our "AcII." In accordance with the statement of Curie and Savitch[1] concerning their inhomogenous substance of 3.5 half life, we found that our earth metal originating from an active earth alcaline metal by beta radiation is no actinium. Curie's and Savitch's result that the activity of lanthan~~um was~~ became concentrated, speaks against equality of their substance to lanthanum and we intend to investigate this experiment more thoroughly since the mixture used by Curie and Savitch might have shown a spurious concentration. It has not yet been controlled whether the final members of our series which we called "Thorium" and which were derived from "Ac-La preparations" proved to be Cerium.

Concerning the "trans-Uranic elements" they are chemically related yet different from their lower homologues Rhenium, Osmium, Iridium, Platinum. It has not yet been investigated whether they might be chemically equal to the still lower homologues Masurium, Ruthenium, Rhodium, Palladium. We could not possibly have thought of such things earlier! The sum of the mass numbers

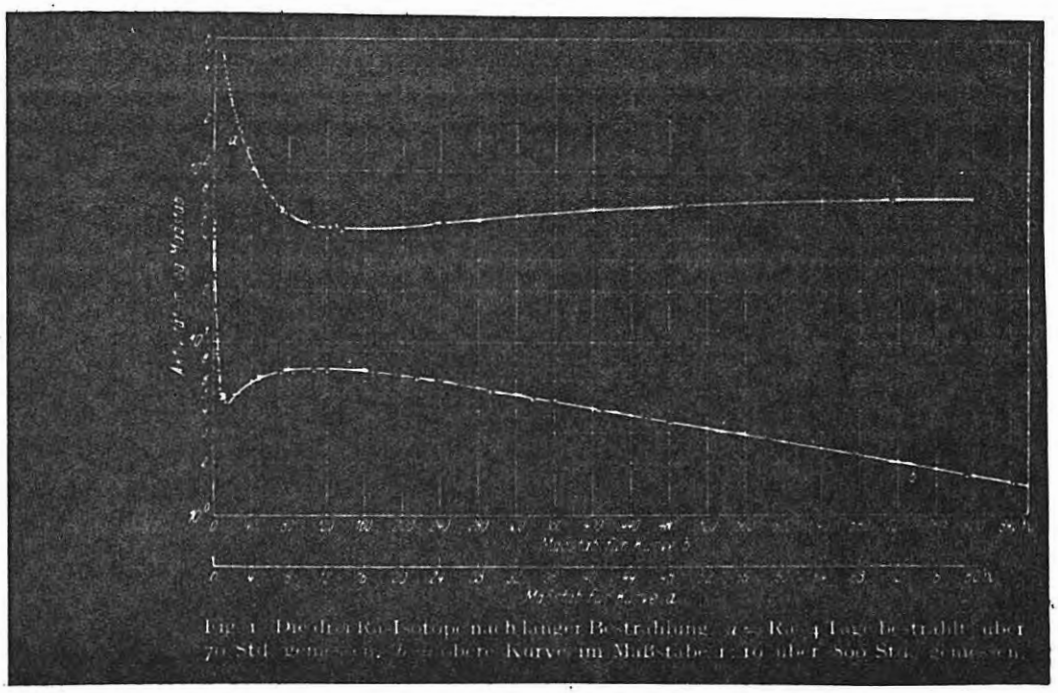
Ba+Ma, for instance 138+101, gives 239!

As chemists, we ought to re-name the previous scheme according to the briefly stated experiments and ~~xxxxx~~ replace Ra, Ac, Th by the symbols Ba, La, Ce. As "Nuclear Chemists" being to a certain degree ~~in contact~~ connected with Physics, we could not make up our minds to make such a sudden jump contradictory to all known results of nuclear physics. There ~~could~~ is still ~~be~~ a possibility that a series of queer coincidences might have deceived us in our results.

It is intended to make new indicative experiments with the new transformation products. Especially we shall try a common fractionation of the radium isotopes generated from thorium through radiation with quick neutrons as described by Meitner, Strassmann and Hahn[1] with our earth alcali metals generated from uranium. This could be done much more easily in places where strong artificial sources of radiation are at disposal.

Finally we thank Miss Cl.Lieber and Miss I.Bohne for their efficient help with the numerous precipitations and measurements.

Activity in logarithmic scale

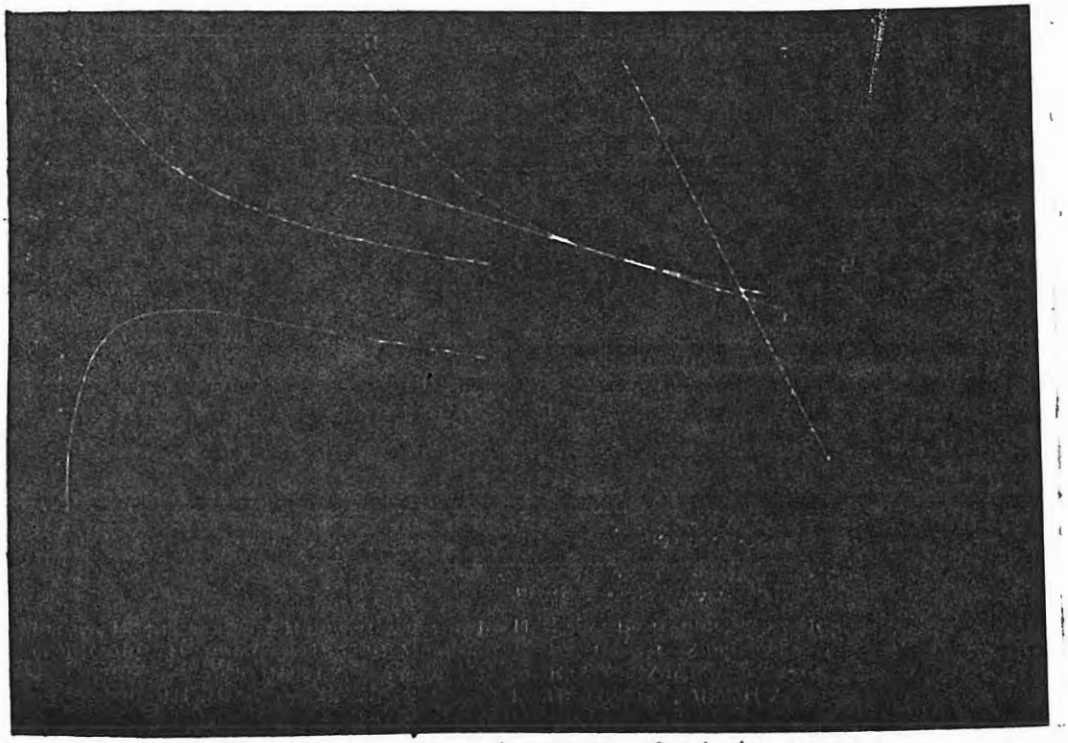


Scale for curve b

Scale for curve a

Graph I. The three Ra - Isotopes after long exposure. a = Ra [4 days' exposure] measured for more than 70 hours. b = upper curve in scale 1:10 measured for more than 800 hours.

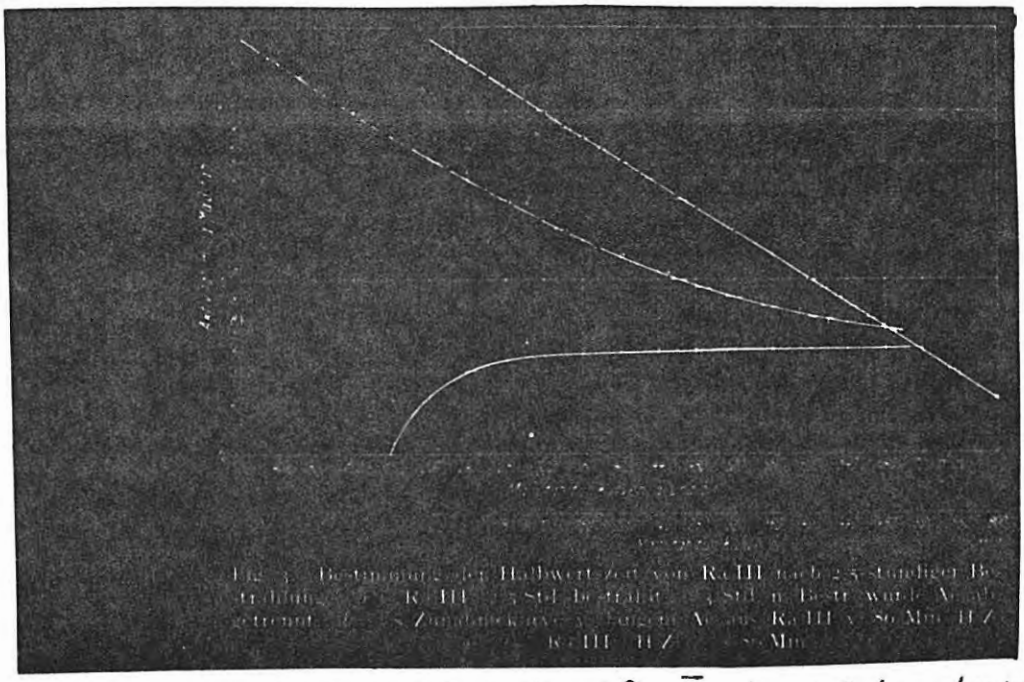
Activity in logarithmic scale



Scale for curves a and b Scale for curve c

Scale for curves c and d

Graph 2. Determination of half lives of Ra II (short exposure). a = Ra after 6 minutes' exposure: Direct curve of decrease. b = theoretical curve of increase of the 2.5 hours' Ac from Ra II, half life = 14 minutes. c = a[Ra] - b [increase 2.5 hours] d = Ra III, half life = 86 minutes. e = c - d = Ra II; gives 14 minutes half life.

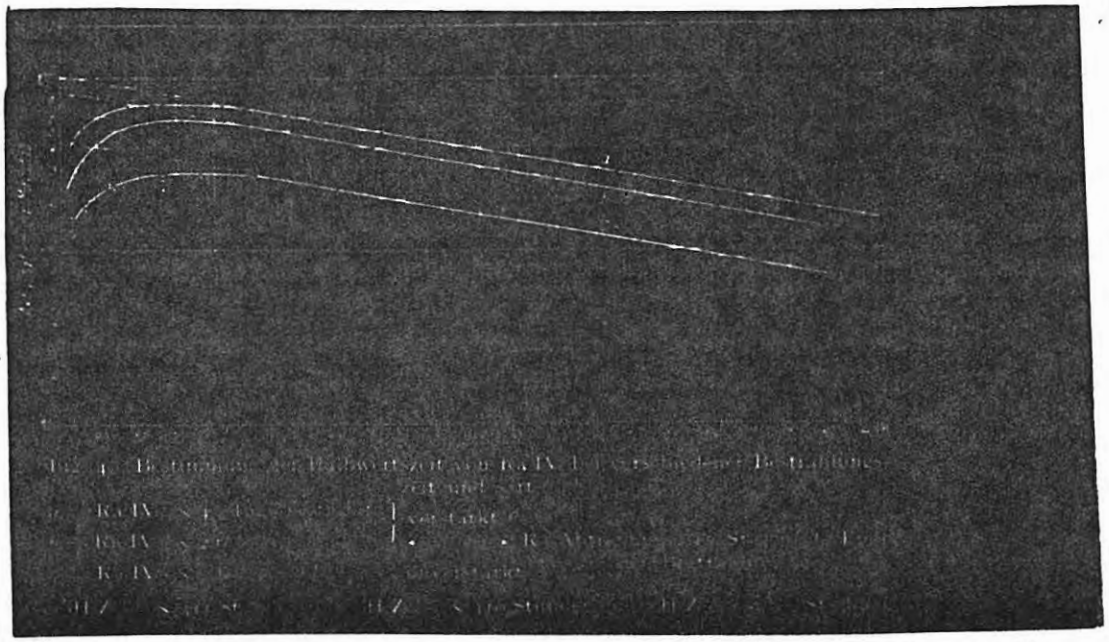


Activity in logarithmic scale

Scales for curves a and b

Scale for curve c

Graph 3. Determination of half life of Ra III after 2.5 hours' exposure.
a = Ra III [2.5 hours' exposure]. 3 hours after exposure Ac was separated.
b = approx. curve of increase of long-lived Ac from Ra III of 86 minutes' half life.
c = a − b = Ra III. Half life about 86 minutes.



Activity in logarithmic scale

Graph 4. Determination of half life of Ra IV at different times and method of exposure.

a = Ra IV [about 4 days' exposure] } concentrated
b = Ra IV [" 2.6 " "] } ⟵⟶ Ra separated 15 hours after end of exposure
c = Ra IV [" 2.6 " "] not concentrated

a: half life = about 311 hours, b: half life = about 310 hours,
c: half life = about 300 hours.

Summary of this paper given in the Smyth-Report:

1.63, p.24: O. Hahn and F.Strassmann... proved that an isotope of barium was produced by neutron bombardment of uranium.

p.265: In January 1939 they (O.Hahn and Dr. Strassmann) published a most important paper in which they reported positive chemical evidence to show that one, at least, of the new isotopes which were believed to be of higher atomic number and mass than uranium was, in fact, an isotope of the element barium which has an atomic number and mass not very different from half that of uranium.

* Ex.1

Notes for:

1. April-Scheduled Address Before Faculty and Students of St. Johns College, Annapolis, on: "War Imminence and the Cultural Crisis of the Inter-War."

2. Eventual Book on:
"The Inter-War Retreat from Reason as Exemplified in the Mis-history of the Recent Past and in the Contemporaneous Conduct of International Political and Economic Affairs by the United States and Great Britain."

NOTES ON IMMINENCE WORLD WAR II IN PERSPECTIVE ACCENT ERRORS AND CULTURAL CRISIS OF THE INTER-WAR DECADES

by

ALEXANDER SACHS

March 10, 1939

This inter-war generation has been living on the edge of a smoldering volcano; and the predominant attitudes among both what is called "Right" and what is called "Left" have been variants of escapism, very much like peasants situated on the edge of a volcano who go on cultivating the slopes in the hope that the eruptions will not take place in their lifetime.

1.

A sense of fatality overhangs this Spring, which is the twentieth anniversary of the preliminary phases of the Versailles Peace Conference before it became enshrouded in the fogs of confusion and frustration. The present sense of fatality arises from the recognition that the Munich settlement that was entrusted from a somnambulist Allied "leadership" was no "peace in our time," but the reverse. Rather, it marks the end of the series of costless victories given to Germany; the limit will shortly be passed when, as is inherently inevitable, Czechoslovakia becomes absorbed into the Reich as was Austria just a year ago.

The leaders and publicists on both sides of the Atlantic -- the people in high places in government, banking and business -- continue to recoil from admitting to the public and even to themselves that the Furies phase of this world tragedy has been entered, and they still want to believe that war can and will be averted. Running through the tragedy is an audible chorus that, concerned over the enormities that have been perpetrated by Nazism against human rights and decencies, senses the oncoming ordeal by battle.

Yet that chorus, while now keyed to a high pitch of indignation against the authors of Munich, is unaware that the very complacencies of liberals and conservatives towards the preserved power-factors in the reconstructed Germany of the Twenties and the resurgent aggression-factors in the Germany of the Thirties have contributed to the rearmament of Germany and the breakdown of collective security. For, without an order of security maintained by the victors, the rearmament of Germany was inevitable. So bemused was the so-called leadership of this generation with its own shibboleths that disarmament became identified with the lack of equality between the armaments of Germany and the victorious Allies. The result was that the agitation over the period from Versailles to the Great Depression - during which President Hoover curiously projected disarmament as a solution to the world crisis - was focused on France, a France fatigued by the war, deprived of the control of the Rhine, and denied the joint guarantee of its defense by Great Britain and the United States. Meanwhile, Germany, in the wake of the Rapallo Agreement of 1922, was able to carry on the testing of new armaments and the training of the original Luftwaffe in Russia. What is more flagrant as a reflection of the eclipse of reason and ignorment of self-interest by the Allies, the circumvention of disarmament through the building of pocket battleships was decided upon by the Weimar Republic and permitted to be implemented by a Second Reich that has been misregarded as pacific-minded and been the object of the common solicitude of the Left and the Right.

The gravamen of this charge is that the heritage of victory has been frittered away with shocking thoughtlessness by the Left and the Right alike. In the Twenties the brilliant people were engaged in undermining the ethical and practical validity of Versailles and of reparations, and demanding concessions to Germany at the expense of our Allies and of ourselves. By way of a synoptic picture of the systematic short-sightedness that during the postwar period was regarded as great international economic planning, it will suffice to give the following highlights on the procedure and the consequences of the planning.

When Germany, in pursuit of its Samson complex, destroyed its own currency to prevent reparations to France, the English-speaking world devised the Dawes Plan, and in implementing that Plan dropped the best feature of the Versailles reparations scheme - namely, reparations in kind - because in the postwar difficulties, the major creditors were afraid that reparations in kind would have the effect of "dumping."

There was an obvious alternative, which this economic practitioner, among others, suggested — to establish an international clearing institution so that the coal which Jugoslavia would need and could take would be credited to the account of England or France; and in this [illegible]'s plan, proportions of German exports would be similarly cleared, thus making goods transfer serve for the later international loans that the Allies themselves had to give to the former smaller Allies. Finally, in the face - or, rather, in defiance - of the evidence summed up in Parker Gilbert's report at the end of 1927 that the Second Reich was not living up to the responsibilities of making reparations "work," it was deemed in that age of unreason "sound" to commercialise reparations and

assimilate them close to the status of ordinary commercial loans. In the process of formulating what came to be known as the Young Plan, the flexible safeguard for dealing with critical economic situations as reflected through changes in the price level was dropped out. By the time the mills of international conferences ground out that agreement, the late summer of 1930, the commodity price depression was already in full swing. But notwithstanding the organic defect of the Plan, it was declared operative. A year later the further economic retrogression due, as this speaker maintained at the time, to the irrationalism of the whole postwar reconstruction, rightly necessitated a demand for revision which led, in turn, to the Hoover Moratorium. Then, a year later, at the Lausanne Conference, reparations were practically eliminated.

The economic upshot of all the economic planning of the Twenties in respect to reparations and reconstruction was, paradoxically, that the Allies had advanced to Germany enormously more than Germany had remitted on reparations account, and had financed a technological reconstruction of Germany that, while uneconomic as to costs for normal international trade, was admirably suited as foundation and structure for the accelerated rearmament on which Germany embarked within a year of the settlement of Lausanne.

Lest in the still prevalent orthodox errors about the responsibility of reparations for Germany's difficulties, the foregoing summary be distorted as proof of the impracticability instead of the mismanagement of reparations by this generation, it is advisable to call attention to a fact scarcely noted at the time and neglected since. The Lausanne Conference for the practical wiping out of reparations was primarily in the nature of

an adjustment to the impact of the Great Depression, but secondarily was intended as an aid to the much harassed and precarious Bruenning regime, which was the penultimate cabinet of Republican Germany, since the g von Papen cabinet was but a device for negotiating the coup d'etat for Hitler's assumption of power. Coincident with the Lausanne negotiations was the resumption of technical discussions on disarmament and, very shortly after the Lausanne settlement, the German delegation functioning under the aegis of the Weimar Republic and the Bruenning cabinet in effect broke off the negotiations, not merely out of dissatisfaction with the Allied proposals, but in furtherance of their express demands for an increase in armaments and a unilateral assertion of the right of rearmament.

The culminating act of Republican Germany with respect to disarmament and rearmament showed that the economic plight was secondary to the aim of resurgence of political and military power in Europe, and provided a disclosure as to where the real source of authority in German foreign policy was and what its ultimate objectives were. The systematic self-delusiveness of our generation has hid from the view of people in authority and the public the plain purpose of Stresemann - the super-Bismarck of the postwar - writ large in his Diaries that were posthumously published just prior to the advent of the Nazis to power. That ~~xx~~ purpose was not a Locarno of reconciliation but cumulative exploitation of the gullible English-speaking world. The fostered pro-German sympathies were to be and were so used a) for the aggrandizement of German economic power, b) for the permanent impairment of French capacity to resist, c) for the German domination over Central Europe, and d) for the reversal of the

whole Eastern settlement represented by Dansig and Silesia.

4.

The foregoing condensation of the international economic and political history of what is imminently the end of the inter-war period is so much at variance from the orthodox views that even those who are not active adherents of that orthodoxy would be tempted to be more than skeptical and demand qualifications and reservations to the foregoing all to blunt statements. Yet as a heretic who was not afforded scope for the presentation of his critique of the prevailing theories because contrary to those of the "authorities" and "experts," this writer can only reaffirm that whether over the issue of the deemed insuperable reparations-transfer problem or disarmament, the "authorities" and the "experts" have been profoundly and elaborately mistaken. The difficulties experienced by a relatively unknown though assiduous student of these problems should occasion no surprise, considering the systematic neglect of the past and recent writings and speeches of Winston Churchill. Of outstanding and decisive importance was the original critique by Clemenceau and his aides at the Peace Conference of the British and American attitudes and policies ~~xx~~ that so signally neglected the elementary geopolitical lesson of that war, to wit: that the Rhine is a vital boundary for Britain and the United States and that France was and might again ~~xx~~ be needed to serve as a base of operations against an aggressive Germany on the military march through the Continent. Thus the reoccupation of the Rhineland, instead of being used by our generation as a warrant for ~~xxxxx~~ repentance of our sins towards France, was made into an appeasement offering to Germany in the naive hope, expressed by the so-called practical people, as well as by the pacifists, that the removal of the

barrier to France would be treated by the Nazi Moloch as a self-set limit to all further aggression.

This invocation of the names of two statesmen from the last war, Churchill and Clemenceau, is suggestive of the paucity of available public figures that have not been afflicted by the illusions of our generation. Of all the participating statesmen who have left a record of their reaction to the last war, Churchill and Clemenceau are supreme for never having faltered in the conviction that the war against Germany was right and righteous, and that the major defect in the Versailles Peace was that it failed to implement victory. For in the obfuscations produced by the formulas of "peace without victory", the treaty of guarantee which was offered by Wilson as a substitute for the French occupation of the Rhineland was not only rejected by the United States as an incident to the Senate's rejection of the League and the treaty, but, on account of the technicalities of its drafting, was never implemented by Great Britain.

In the post-Versailles period the leaders and people of Britain became addicted to the illusory side as distinguished from the constructive side of Wilsonian idealism,- the assumption that a pacific-minded German people had been victimized by an aggressive militarist Monarchy and that the introduction of a Republic would by itself once and for all eliminate the desires and the opportunities for military aggression, and that a World League as a sounding board detached from peace enforcement would, whenever international difficulties arose, suffice to rally "the people" against their ambitious and misleading rulers. The cultural epidemic of "debunking" and of uncritical historical revisionism led to an inversion of the Versailles declaration regarding the origins of the

war Out of deference to German sensibilities regarding the so-called "guilt-clause," it became fashionable on both sides of the Atlantic to take for granted the validity of German grievances against the various provisionsof the Treaty of Versailles. Peimsure became the "bete noire" of the Anglo-American intelligentsia. Practical men of affairs in the English-speaking world went on the assumption that German consent had to be secured at all costs. The subsequent years proved that such consent could only be through concession after concession. In the process, the conduct of international politics and economics of the inter-war period became an illustration of Santayana's definition of fanaticism as a redoubling of effort after losing sight of the aim.

The inter-war period has thus exhibited in the large the operation of the Gresham's law applied to ideas: the errors and illusions of this period had succeeded in gaining sway, first in the United States, then in Great Britain, then through the combined power of the two countries finally over France; and so far as effective opinion goes in governments, banking and business, and the technical and general press, the errors and illusions had become conventional for the Allied world by the time of the German remilitarization of the Rhineland.

5.

Since the advent of Hitler a process of re-education about Germany has been proceeding in the English-speaking world. And while there have been trailing clouds of illusion regarding the important objectives of the Stresemann conduct of foreign affairs for the Second Reich, the portent and the enormity of the Third Reich has at last been borne in upon the consciences of Britain and of the United States. But alas, France is suffering from a cultural lag. Having first understood that the German ambition for domination was not eclipsed by the change

from a Monarchy to a Republic, leading parts of French society have deceived themselves to the point of hoping that the German totalitarian tyranny could be made to tolerate the survival and promote the prosperity of a reactionary France. The prevailing political irrealism in France is the result of that internal social schism which now, more than ever, makes French parties hate one another more than they hate the common enemy.

c.

The present period is too late for that reversal of error which prevents the consequences of error. The real "Munich" took place in 1936 when France and Britain, still members of the League, and the self-isolated United States, that stayed away from the League, allowed Hitler to march into and remilitarize the Rhineland. Then was the last opportunity missed for preventing that cumulative German aggression that was bound to culminate in a new and more terrible war by Germany. But what can and must be done for our salvation and safety is self-clarification and self-reorientation towards the crushing dangers.

There is still time for Western Civilization, and especially for the exceptionally and fortunately situated United States, to use the time-drafts that can still be made on the Bank of History, for the preparedness that has and will become more and more urgent and inevitable for all members of Western Civilization as a result of the past errors committed and in the course of the prospective unfolding aggressions of Nazi Germany.