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ADWD-148

Redacted VERSION

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LAMD-327

FAMILY COMMITTEE
Minutes of Thirteenth Meeting
June 8, 1950

A. Attendance.

The thirteenth meeting of the Family Committee was held Thursday, June 8, 1950 at 1:15 P.M. in Room B-117. Those present were:

H. M. Agnew	E. R. Jette
H. V. Argo	D. P. MacDougall
J. C. Clark	J. C. Mark
F. de Hoffmann	W. E. Ogle
D. K. Froman	F. Reines
R. W. Goranson	F. Seitz
A. C. Graves	R. F. Taschek
D. B. Hall	E. Teller, Chairman
M. B. Holloway	

B. Minutes of the Twelfth Meeting.

The Committee unanimously adopted the minutes of the Twelfth Meeting reported in ADWD-146, with the following additions and corrections:

(1)

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(3)

(2)

C. TENEX.

Agnew gave a summary of the conclusions which had led him and Argo to report favorably on the possibility of performing TENEX for the Spring '51 tests. (These arguments are outlined in detail in ADWD-142.) It was estimated that about two scopes would be required at both stations with possibly some more scopes as a timing tie-in. The scope figures are here reported in order to give an indication of the order of magnitude involved. The distant station is needed to measure the energy spread due to temperature in the 14 mev neutrons. The close station establishes the fact that the actual DT reaction occurred in a time short compared to the time signal in the distant station. The low number of scopes, in spite of the large ranges to be covered, is due to the fact that soft scopes, that is scopes driven at low voltage, will suffice for the time resolution required in this experiment.

It was reported that most likely the detecting chambers in the TENEX experiment would be of the hydrogen recoil type rather than fission chambers since in the former case one can get many more atoms per cm^2 so that the

reiterated a statement he had made in previous meetings that at the present time all information concerning crystal response could only be put into the class of "guesswork" because the experiments concerning these crystals were still in progress at various laboratories throughout the country and the results still seem to differ by large factors. Moreover, Taschek remarked that such crystal response would have to be studied under large current drawing conditions.

It was agreed that if the fall time were larger than one shake, then the advantages with respect to timing of FLUNEX over TENEX would be nil. However, if the effective fall time is less than or about a shake, then FLUNEX will give information about the crucial induction time in the case of a fizzle, whereas TENEX would not have sufficient time resolution.

Discussion took place concerning the way one would establish a fiducial timing mark for the FLUNEX experiment. Two possibilities present themselves:

(1) Observation of the point at which the curve breaks away from the constant alpha region and comparison with the theoretical time at which this should take place.

(2) An absolute calibration of the number of neutrons falling on the fluorescer by means of comparison with such experiments as ANEX or PHONEX.

It was noted that in the experiment as proposed by Teller, some cables and some shielding of these cables would be necessary, which is a deviation from the originally proposed experiment--not now deemed advisable because of the scattering in air. Reines pointed out that if one only wanted to obtain information in the DINEX experiment concerning the shape of the 14 mev neutrons, without much regard to absolute values, one might be able to eliminate a considerable amount of the shielding required for DINEX. It was noted, however, that in the case only a very small fraction of the DT burned, elimination of such shielding may give rise to difficulties in DINEX, due to swamping of the 14 mev neutrons by neutrons of lower energy, such as 5 mev. In the case of FLUNEX one would have the advantage that the distance at which the crystal is placed automatically insures some time of flight separation between the 14 mev and 5 mev neutrons.

The group then proceeded to try to examine whether in view of the above discussion it should recommend that the FLUNEX experiment was very worth while performing in addition to the other experiments now planned for the Spring of '51. Teller stated that he felt that insurance for the DINEX experiment was definitely needed and suggested that the Family Committee request appropriate people to make more detailed studies in the immediate future to determine the experimental feasibility of incorporating FLUNEX in the Spring '51 tests. He felt that the loss of data which would result in the case of a complete DINEX fizzle would be very serious and that the FLUNEX effort would definitely be warranted unless it proved to be an experimental monster.

Taschek, Froman, the experimentalists present and others felt that the insurance obtained from FLUNEX is not likely to counterbalance the detracting effect that the FLUNEX experiment would produce. In particular, they believe that such an experiment is of considerable magnitude because the experimental data on which it is to be based do not yet exist, and that there is no certainty that they can be obtained in time--at least without disrupting efforts which must proceed on DINEX and other larger experiments now scheduled for the Spring of '51. Graves reported that J-Division would have to know within the order of two or three months whether FLUNEX would be incorporated or not in order to make experimental arrangements at Eniwetok. (The outside time limit would assume that certain construction items on the testing tower and at the bottom of it would be irreversibly frozen and the FLUNEX experiment would have to adjust itself to the given physical conditions.)

The Committee found it impossible to resolve the conflict concerning what scale of possible effort (ranging between a nil and a full-fledged effort) would be recommended with respect to FLUNEX. In view of this, the Committee requested that Graves examine the situation further and, keeping in mind the commitments made at this meeting, make a decision concerning the scope and type of effort which should be expended on FLUNEX. It was requested that Graves report his conclusions to the Family Committee.

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E. [redacted]

Goranson summarized the conclusions incorporated in Konopinski's and his memorandum of June 7, which is included as Appendix A to these minutes. It was noted that the sketch represented in Figure 1 of Appendix A represents a revision of Figure 6 of the Eleventh Minutes. [redacted]

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F. Future Meetings.

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[redacted]

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The subsequent meeting will take place on Wednesday, June 28th. [redacted] The following are to be

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INTER-OFFICE MEMORANDUM

APPENDIX A

Date 7 June 1950

TO: - The Family Committee

FROM: Emil Konopinski and Roy Goranson

SUBJECT:

DOE b(3)

REF: ADND-3-3

PART I. THEORETICAL CONSIDERATIONS

Presented herewith are the results of discussions in which the following have participated: Teller, Goranson, Mark, Wheeler, Smith, Taylor, Hammer and Konopinski.

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The types of advantage considered as possible are:

1. The increased H.E. dimensions will result in a greater compression of core and tamper if their mass is unchanged or if their mass is not scaled up fully together with the H.E. dimensions. The greater compression will produce higher central temperatures. This is partially offset, as far as the yield of radiation is concerned, by the greater density of core material which the radiation must penetrate on its way out.

2. The tamper may be thickened.

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3.

For a more quantitative comparison of the above advantages, Wheeler proposed as a criterion that the quantity

E

should be maximized. Here φ is the undepleted efficiency, which primarily determines the temperature.

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The second column of the table gives the mass of 25 in kilograms. The third column gives the radius R of the compressed 25 in cm.

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ρ/ρ_0 is the core compression. φ and φ' are undepleted and depleted efficiencies, respectively. The column headed Y gives the expected final yields in kilotons. The final column lists the values for Wheeler's criterion.

The next two lines deal with models obtained by scaling up all radial dimensions from the first line, so as to keep the same compression.

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The models of lines 5 and 6 keep the same mass of tamper as obtained in line 3.

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Calculations at present indicate that this yield will be needed to assure adequate radiation temperatures.

The tamper mass in the line 6 model is the same as in lines 3 and 5

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The latter model has the added advantage that it

makes the early energy evolution more secure against interruption by the tamper rarefaction. It is the model of line 6, therefore, which is recommended as the result of these discussions.

One qualification upon this recommendation was pressed by Teller.

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This may constitute a hazard for the observers of the test, for the observation records of the test, or for the sites of subsequent tests. The main argument for the existence of this small chance of an excessive yield is the following: The Hippo calculations lead to much higher yields than were actually observed at Trinity.

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PART II - DESIGN CHARACTERISTICS

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The figures are drawn to scale as presently conceived. However, dimensions have been omitted where there exists a possibility that later minor modifications may be found to be advisable.

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In Table II

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These figures are compared with the three earlier preliminary design considerations listed in columns 1 to 3 inclusive.

I.B.M. calculations have been made on F-4 and F-5.

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The implosion of F-4 on 0.7 scale and of F-6 on 0.6 scale have also been studied photographically and by the pin technique. Compressions given in the third and fourth columns are estimated from those calculated for the first two columns.

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There exists a small but finite probability (perhaps 5%) that the calculated efficiency may be underestimated by a factor of 50% (or perhaps even by 75%).

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). It should be pointed out, however, that the pre-detonation probability (a fizzle) is expected to be greater than the probability of an excessive yield.

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Emil Konopinski
Roy Goranson

RWG:rvc

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