ACH1.000010.009b

Attachment 2

NEPA Advisory Committee on Radiation Tolerance of Military Personnel. *Proceedings.* Report No. NEPA-1011-1ER-16. -- April 3, 1949. Washington, D.C. 104p.

NEPA

FAIRCHILD ENGINE AND AIRPLANE CORPORATION

PROCEEDINGS OF NEPA ADVISORY COMMITTEE ON RADIATION TOLERANCE OF MILITARY PERSONNEL

Report No. NEPA-1011-IER-16

DATE April 3, 1949

NEPA DIVISION

OAK RIDGE, TENNESSEE

NEPA ADVISORY COMMITTEE ON RADIATION TOLERANCE

OF MILITARY PERSONNEL

Carlton Hotel
Washington, D. C.
April 3, 1949

ORDER OF BUSINESS

Morning

9:30 A.M.	Call to order—Chairman Dowdy
9:35 A.M.	Remarks by Mr. Ward, Mr. Sims, Mr. Simmons
9:50 A.M.	Minutes of the Chicago Meeting, June 23, 1948—Dr. Selle
9:55 A.M.	Report of Executive Committee Meeting of December 10, 1948, in San Francisco—Dr. Dowdy
10:00 A.M.	Available Data: 1. Discussion 2. Recommendations
10:45 A.M.	Probable Results to Humans: 1. Discussion 2. Recommendations
11:20 A.M.	Report of Committee on Human Radiation Problems—Dr. Stone
12:00 A.M.	Lunch

Afternoon

2:00 P.M.	Report on Suggested Problems for Research
3:00 P.M.	Recommendations to NEPA relative to its Biology Program
4:00 P.M.	Adjournment

NEPA ADVISORY COMMITTEE ON RADIATION TOLERANCE

PARTICIPANTS

NAME	OF
ANDERSON, Rupert S., Ph.D.	U. of South Dakota
CALKINS, V. P., Ph.D.	Oak Ridge, NEPA
COWEN, Don	Oak Ridge, NEPA
DONALDSON, Lauren R., Ph.D.	U. of Washington
DOWDY, Andrew H., M.D.	U.C.L.A.
ELLINGER, Freideric, M.D.	Naval Med, Research Institute
EVANS, Robley D., Ph.D.	M.I.T.
EVANS, Titus C., Ph.D.	U. of Iowa
FAILLA, G., Ph.D.	Columbia University
FRIEDELL, Hymer L., M.D.	University Hospital, Cleveland
HARBISON, Mr.	Travelers Ins. Co., Hartford, Conn.
HOLLAENDER, Alexander, Ph.D.	Oak Ridge, Nat'l. Lab.
KALITINSKY, Andrew	Oak Ridge, NEPA
MILLER, A. J., Ph.D.	Oak Ridge, NEPA
NEWELL, R. R., M.D.	Stanford University
NIMS, L. F., Ph.D.	Brookhaven Nat'l. Lab.
SELLE, W. A., Ph.D.	U. of Texas
SIMMONS, Gordon	Oak Ridge, NEPA
SIMS, T. A.	Oak Ridge, NEPA
STOECKLE, H. E., Ph.D.	Oak Ridge, A.E.C.
STONE, Robert S., M.D.	U. of California

NAME	OF
STRATTON, R. C.	Travelers Ins. Co., Hartford, Conn.
WARD, J. Carlton	Fairchild Eng. & Airplane Corp., New York City
WARREN, Shields, M.D.	A.E.C., Washington, D. C.
WARREN, Stafford L., M.D.	U.C.L.A.
WHITING, Richard	Travelers Ins. Co., Hartford, Conn.

P R O C E E D I N G S

CHAIRMAN DOWDY: If the meeting will come to order, I think we will get ahead with our program. We are just a little late now.

I am changing the order of the program just a little. One of the first things is opening remarks by Mr. Ward, Mr. Sims, and Mr. Simmons, and Dr. Shields Warren. However, I think we will defer those remarks until just before closing this afternoon.

I would like to call on Mr. Ward, however, to introduce some guests that he has with him. Mr. Ward.

MR. WARD: Dr. Dowdy, we have some members of the insurance fraternity with us this morning, as most of you may know. I would like to call on Mr. Harbison, who is a member of the Legal Department of the Travelers Insurance Company, to introduce his associates.

This is Mr. Harbison.

MR. HARBISON: We are very glad to be here. Travelers has been very much interested in this whole subject. Some years ago we were asked to insure a project out in Washington built by Dupont. We insured it, and we had no knowledge whatever of what we were insuring. I can assure you that is a type of activity on the part of insurance companies which was unknown, and is unknown today. They generally want to know very much as to what they are insuring.

We were very happy to go into that, and we have had a very great interest in this whole matter ever since. We feel very greatly indebted to Mr. Ward for making it possible for us to sit in and be here today.

Mr. Stratton of Travelers is on my immediate left; and Dr. Whiting of Travelers is next to him. They are very much interested in this whole subject of radiology. Mr. Stratton has had a great deal to do with it for a number of years past; and Dr. Whiting follows it very closely, too.

CHAIRMAN DOWDY: Thank you, Mr. Harbison. We are very glad to have you and your associates, Mr. Stratton and Dr. Whiting, with us this morning.

I would like to introduce Dr. Ellinger. I think most of you know him. He is with the Naval Research group at Bethesda.

Dr. Selle, will you read the minutes of our meeting that we had in Chicago on June 23.

DR. SELLE: (Reading)

"The first meeting of the NEPA Advisory Committee on Radiation Tolerance of Military Personnel was held June 23, 1948, at the Palmer House, Chicago. Dr. Shields Warren served as temporary chairman and Dr. Charles H. Perry as temporary secretary."

Mr. Chairman, I trust I will not have to read the list of the individuals who attended that meeting. This is similar to the one which most of you have before you at this time.

"The Chairman called upon Mr. T. A. Sims and Mr. J. Carlton Ward to brief the group relative to the objectives of the NEPA Project and to indicate some of the medical problems involved.

"Mr. Sims discussed the limitation of performance of chemically fueled aircraft in consequence of the incompatibility of speed and range. He presented certain fundamental aspects of aerodynamics and commented briefly on advantages to be gained from the application of atomic energy to aircraft propulsion. Mr. Ward summarized pertinent discussions of the Congressional Air Policy Board and of the Hoover Committee, and also commented briefly on the projected use of atomic energy for military purposes.

"Acting Chairman Shields Warren charged the Advisory Committee with the responsibility of deciding what can and should be done in order to insure protection to the personnel operating the hypothetical type of plane such as Mr. Sims and Mr. Ward described.

"An election of permanent officers of the Committee was then held. Dr. Andrew H. Dowdy was elected Chairman, and Dr. Charles H. Perry, Secretary.

"Mr. Simmons spoke of the radiation problem as it will apply to military personnel comprising the crew of a nuclear aircraft. He pointed out that under operating conditions, it is not desirable to accept the present radiation standards established for existing ground installations, such as the stationary pile and the usual hot radiation laboratory.

"Mr. Simmons indicated certain subjects of interest to NEPA about which further information was requested. Among those subjects mentioned were:

1. Actuarial statistics relating to human survival at various levels of radiation.

2. Variations in human sensitivity to mixed radiation such as would prevail in the vicinity of a reactor of the type contemplated.

3. Methods determining the degree of physiological sensitivity to radiations.

4. Additional study and refinement of the ren and rep concepts, and the arrival at a better correlation between various types of radiation and associated conversion factors.

5. Methods of increasing resistance to the physiological effects of radiation.

"Mr. Simmons spoke at some length on the problem of reactor shielding, particularly in relation to shield weight and to personnel protection and airplane performance. He mentioned that increasing the speed of the airplane by decreasing the weight of the shield would expose the crew to a higher radiation level, but would reduce the duration of exposure and would favor survival from enemy action. The configuration, design and weight of the shield, as well as the position of the reactor in the plane, would all vitally affect the detailed design of the power plant and the nature and extent of the radiation hazard.

"Mr. Simmons indicated that as a starting point for the design of the shield, an arbitrary exposure value of 1 rep per hour for 25 hours might be used as a basis of discussion. This figure represents an increase over present laboratory standards by a factor of 100 as based on the so-called permissible exposure of 0.1 r per working day, or approximately .01 r per hour.

"Considerable general discussion ensued concerning the probable time required to train the crew of a nuclear powered plane and to complete a specific military assignment.

"Mr. Kalitinsky discussed further shield design from the viewpoint of hazards from enemy action and indicated that such hazards are inversely dependent upon the degree of radiation protection afforded the crew.

"Dr. Shields Warren commented briefly on phases of work in the A.E.C. and on data available from the Manhattan District Operations which have immediate bearing upon the radiation protection problem under consideration.

"Dr. Calkins discussed a prepared mimeographed report which he distributed among the members present. This report concerned certain well-known physiological effects of whole body radiation and offered proposals for biochemical research aimed at increasing the physiological tolerance to ionizing radiations.

"Among the chemicals suggested by Dr. Calkins for study were:

1. For preventing the hemorrhagic syndrome: toluidine blue, protamine sulphate, antioxidants, to be used with or without rutin and Vitamin C.

2. For fortifying against the non-descript toxic phenomenon: ethylene disulphonate, theophorin, antioxidants.

3. For combatting bacterial infection: penicillin and sulfa compounds.

"Dr. Calkins' comments were followed by remarks by Drs. Dowdy, Shields Warren, Brues and Newell on anti-histaminics, rutin and Vitamin P, and antibiotics.

"There ensued an involved open discussion on radiation data from human sources, and on methods and procedures necessary for obtaining additional radiation data on man. Dr. Newell and Dr. Dowdy further stressed the obvious need for systematic collection of all available data on radiation effects and the exploration of methods of ameliorating damage and increasing the body tolerance to ionizing radiations.

"Prior to the noon recess, Mr. Ward injected the wish that there be given no immediate statement or comment relative to the allowable or permissible radiation exposure of personnel. Dr. Dowdy indicated that the present meeting is preliminary and that available information is inadequate to support any statement of permissible exposure at this time, and that long-range planning seems necessary to obtain the exact information needed for the Project.

"On reconvening following the noon recess, Dr. Dowdy appointed an Executive Committee to assist the Chairman and Secretary in conducting the affairs of the Advisory Committee. He appointed to this subcommittee Drs. Robert S. Stone; Robley Evans; Stafford L. Warren; and C. H. Perry.

"The Chairman also appointed a subcommittee on Available Data to assist in the collection of pertinent information being obtained at various Atomic Energy Installations and Universities conducting biological investigations on radiation. He named on this committee: Dr. G. Failla and Dr. Titus C. Evans for Columbia University; Dr. Austin M. Brues and Dr. Raymond E. Zirkle for the University of Chicago and Argonne National Laboratory; Dr. Alexander Hollaender and Dr. Raymond E. Zirkle for Oak Ridge National Laboratory; Dr. Robert S. Stone for California; Dr. Wright Langham for Los Angeles; and Dr. Rupert Anderson from South Dakota, who is especially requested to obtain data on the biological aspects of radiation.

"Dr. Dowdy charged each member of the Subcommittee on Available Data with the responsibility of summarizing, or having summarized, for NEPA the pertinent information on radiobiology coming from his own area of activity; and presenting the material to the Executive Committee within six months, so that the latter committee might have ample time to edit the material for presentation to the Advisory Committee at its next meeting.

"This summary is to serve as a basis for recommendations relative to additional research which will be required to answer specific questions arising from the radiation protection problem.

"The Chairman requested that in summarizing the available information, the members keep in mind the following items:

1. Methods of detecting slight biological changes, particularly those having a practical clinical application.

2. Recovery rates.

3. Variation in biological effects with intensity and rate of administration.

4. Summation of multiple doses of the same type of radiation or combinations of different types of radiation.

5. The biological effective ratio of "n" to "r."

"Dr. Perry stated that Dr. Dowdy would also serve as Chairman of the Subcommittee on Available Data and that all summaries from committee members should be sent to Dr. Dowdy.

"The Chair then appointed a Subcommittee on Human Radiation Problems, which included Drs. Robert S. Stone, Chairman; Simeon T. Cantril; H. L. Friedell; Shields Warren; and R. R. Newell.

"Members of this subcommittee were also admonished to keep in mind:

1. Dosage rate.

2. The r to n ratio.

3. Effects of combined radiation.

4. Recovery rates.

5. The smallest detectable biological reaction.

"During the latter part of the afternoon discussion was directed to:

1. Clearance of technical information.

2. Radiation exposure levels between 25 and 100 r.

3. Engineering problems concerning the reactor, shielding, and ship design.

4. The anticipated neutron to gamma ray ratio of aircraft reactor radiations.

5. Specific physiological effects of ionizing radiations and possible methods of preventing radiation effects by chemical and pharmacological means.

"Following a few remarks from Mr. Ward in appreciation of the efforts of the Committee in their deliberations, the meeting was adjourned at 5:00 o'clock. Respectfully submitted, W. A. Selle, who has now replaced Dr. Perry as Secretary of the NEPA Advisory Committee on Radiation Tolerance of Military Personnel."

CHAIRMAN DOWDY: Thank you, Dr. Selle. Are there any corrections to the minutes as read?

DR. ROBLEY EVANS: Dr. Langham was read as "Los Angeles" instead of Los Alamos.

CHAIRMAN DOWDY: Any further corrections? (There was no response.) If not, the minutes will stand approved as read.

This Committee in June was assigned two tasks. The first was to give information to NEPA relative to the probable results on humans of various dosage levels of acute total body radiation. The second was to make recommendations to NEPA relative to a research program which would further clarify or fill in any gaps in the information we now have relative to human exposure under the conditions pertaining to the NEPA Project.

The available data was sent in. Dr. Perry; Dr. Selle; Dr. Anderson, and I edited this material. We presented it to the Executive Committee on December 10 in San Francisco, at which time the information was gone over very, very carefully; corrections made; and also the probable results to humans from various dosage levels which had been presented to the Executive Committee was thoroughly discussed, not only as to the results but to the exact wording.

Corrections were made on this. We returned to Los Angeles and corrected the report and the probable results in line with the recommendations of the Executive Committee.

The probable results on humans was then sent out to the Executive Committee members following this revision for their further correction; following which it was then put out in mimeographed form to all members of this Committee.

I might tell you the reason for the classification which we have on here as "Restricted." This information as it is contained in this little blue, legal-size, very convenient package has nothing in it that any divulgence would be detrimental to our country's security. It does, however, have information from one report on human accidents, so it is classified in this respect from a personal relationship or from a medical legal problem aspect. Some of these cases are still pending, and the Atomic Energy Commission felt that the report as such cannot be put out for public consumption.

So, if any discussion relative to these particular cases should ensue this morning, we might have to ask some of our visitors to leave the room while this is being discussed. However, I hope that a detailed discussion of the data as compiled will not be necessary, because it has been in the hands of each member for at least approximately a week. I notice it was issued on February 15.

DR. NEWELL: Two or three weeks.

CHAIRMAN DOWDY: So I hope we will not have to go into detail on it. We probably will have to go into some detailed discussion on Section IX of it relative to the various dosage levels and probable results on humans.

However, again I would like to emphasize that this has been very carefully studied by the Executive Committee and has been issued to each of you; and if there are any particular comments on it, I think they would probably have been made.

I received a letter from Dr. Cantril expressing regrets in not being able to be here, but he did in general accept Article IX of this report. So, at this time, if there is any discussion on the available data as such, it is open for discussion.

DR. NEWELL: You don't want discussion on Article IX at the moment?

CHAIRMAN DOWDY: Not right now. That is coming up later. We realize that this information is not all the information in the country on this subject. It is information, however, that was sent to us by the various members, and information which I and Dr. Selle and Dr. Anderson were able to glean from the various reports, both classified and unclassified. It contains Dr. Stone's personal experiments. It includes a group of cases from Chicago. It is put in tabular form.

DR. HOLLAENDER: I have a statement that is more up-to-date.

CHAIRMAN DOWDY: Would you give us the page?

DR. HOLLAENDER: I can give you the page.

CHAIRMAN DOWDY: Why not give it to me and we will put it in the minutes and make those corrections where they are necessary.

Dr. Hollaender has handed in a sheet of minor corrections which he would like for us to make and include in the revised edition. Is that correct, Dr. Hollaender?

DR. HOLLAENDER: That is right.

DR. FAILLA: Section IX is included in this?

CHAIRMAN DOWDY: I put that under two different headings.

DR. FAILLA: We will accept this, with the exception of Section IX.

CHAIRMAN DOWDY: Then we will go on to Section IX.

If there are no further corrections, the Chair will entertain a motion that we accept the available data as presented and recommend it to NEPA.

DR. FAILLA: I so move.

CHAIRMAN DOWDY: Do you second the motion?

DR. STONE: I don't know whether we can discuss it here, but there is still a question of the interpretation of some of the data from Los Alamos.

CHAIRMAN DOWDY: Yes. We will have to take that for its face value here, I think.

DR. FAILLA: I would suggest that a note be added to the effect that the doses are subject to revision. The doses as stated in terms of roentgens are subject to revision in those cases. That is being done at the present time, is it not?

DR. STONE: Yes. I think with that, we can accept them. Will you put that in your motion, that we accept this with the understanding —

DR. FAILLA: Yes.

DR. STONE: Which way is the revision going to be? Do you have any idea?

DR. FAILLA: I haven't the slightest idea. I don't know what they took as to the biological effects of neutrons and gamma rays.

CHAIRMAN DOWDY: A note will be added that the doses on those cases will be subject to revision?

DR. FAILLA: Yes.

CHAIRMAN DOWDY: Do you want to give me that motion again?

DR. FAILLA: I move that the report be accepted, with the addition of a note referring to the Los Alamos cases, to the effect that the doses as expressed in the report are subject to revision.

CHAIRMAN DOWDY: Dr. Failla re-moves that the report be accepted with the addition of a note referring to dosage of the Los Alamos cases are subject to revision.

DR. ROBLEY EVANS: I will second that.

CHAIRMAN DOWDY: The motion has been made and seconded. Is there any discussion?

(There was no response.)

CHAIRMAN DOWDY: If not, those in favor will say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

The second order of business is on Article IX, which is on Page 34, entitled, "Acute Exposure. Estimated Results to Humans Exposed to Filtered, 200-1000 KVP X-Rays, Measured in Air."

It was thought by the Executive Committee that in this particular part of our recommendations that we should recommend various dosage levels and probable results, feeling that we as civilians could not state what the risks should be, that the risk of any particular mission is the responsibility of the military; and the best that we could do would be to give them what we feel, in light of our present information, would be the relative risks that they would incur at these dosage levels.

So that accounts for the way we have expressed these various dosages. Dr. Friedell.

DR. FRIEDELL: The only recommendation I might have in regard to this is, we have not given any indication as to any division of response in a group of personnel. In other words, for example, "C, 100 r; at this level, nausea and fatigue may be a problem."

I think there is enough individual response so that some individuals may have no nausea and fatigue. Some may have considerable. Some may have a moderate amount.

I think we ought to see if we can decide upon what percentage might be affected by those particular levels.

Other than that, I agree in general with the character of this report.

DR. FAILLA: The wording is intended to take care of that, Dr. Friedell. When you say "may be a problem," it means that some may get nausea and some may not.

If you start to put down percentages, we will never agree to anything, because we don't know what percentages they are.

DR. FRIEDELL: If, for example, you have a crew of ten people, it is possible that some of them may not be affected at all; and if the duties can be interchanged, it means that you can go to a pretty high level, because some of them can take over the functions of others.

DR. FAILLA: That is right. We will agree to that. But to put down in figures just what percentage would show that at 100 r, I don't think anybody knows.

CHAIRMAN DOWDY: I wouldn't hazard a guess as to what percentage. When we say "may," we think it is possible, but not particularly highly probable. I think it depends somewhat on the psychological makeup of your crew, which is the problem which NEPA will have to go into.

DR. FRIEDELL: Let me change my suggestion then. Perhaps we ought to just add a note stating that there is individual variation, and you can expect that some will have no reaction; some will have severe reaction; and some will have moderate reaction in this category.

CHAIRMAN DOWDY: Do you want that note just pertaining to C, or would you like to add a general note?

DR. FRIEDELL: I think it ought to be a general note stating that this is what occurs on the average, but you can expect variations.

CHAIRMAN DOWDY: Would you like to put that in words?

DR. FRIEDELL: Let me write something, and then I can give it a little later.

DR. NIMS: I think also a general note can be added that some of these effects, genetic effects, are dependent upon age.

DR. FAILLA: What effects?

DR. NIMS: A man of 45, the effects are practically nil. I think a general statement of the delayed effects should be put in. We took it for granted that this meant the child-bearing period.

DR. STONE: Forty-five or up?

DR. NIMS: There is no reason why we couldn't. Some of our best civilian pilots are in the age range of 45 to 50.

CHAIRMAN DOWDY: There would be very little danger there, of course.

DR. STONE: A civilian pilot stands the gaff for about three or four hours, doesn't he; and here you are going to get somebody who will have to stand the gaff for up to twenty-four hours, let's say. It would be a difficult problem. You would need somebody who can take it.

CHAIRMAN DOWDY: Is it felt this second suggestion should be in there, or is that covered in our report? We discussed this matter quite thoroughly in San Francisco, and our feeling was that we were specifically speaking in regards to this, the people in the younger periods of life.

DR. FAILLA: I think it is implied.

DR. HOLLAENDER: I am not discussing this as a geneticist, but I have a feeling that not enough attention has been paid to the small effects we may have on the population. I think we should include some statement in here that we do not know enough about these to be conscious of these, and we have to wait until we get results from experimentation. It might help to keep this in mind, at least.

CHAIRMAN DOWDY: That is the reason why I put Dr. Charles' notes in here verbatim. I might say this, that so far as NEPA is concerned, while we did put a fair amount in here on genetics, so far as NEPA is concerned it is a military problem, and genetics is of very little importance.

In the case of war, if an objective is to be gained of sufficient importance, one is not going to stop and consider much what is going to happen to the second and third generation in a relatively small percentage, as compared to the people who are existing, living, at the time.

So, even though the genetic hazards were, we will say, ten or fifteen times as great as we believe them to be, I still don't believe it would hold up the military operation; because if you didn't save the present generation, you certainly wouldn't have to worry about the future.

DR. HOLLAENDER: Couldn't it be stated that we are conscious of the problem?

DR. NEWELL: I think you are right. I think it is the wrong decision, but I think you are right that that will be the decision.

CHAIRMAN DOWDY: Yes, I think it will. We put genetic possibility under each one of these dosage levels, and then I added Dr. Charles' notes verbatim. So I believe we are very well aware of that possibility.

DR. HOLLAENDER: Our feeling is that Dr. Charles represents a school who doesn't believe in the seriousness of the recessive changes that might appear.

CHAIRMAN DOWDY: I think he does.

DR. FAILLA: I think he does.

CHAIRMAN DOWDY: I think he does. Perhaps Dr. Warren and I may have stretched him a little on that. You talked to him and got his results. He is very much aware of that.

DR. STONE: I think, Mr. Chairman, we have got to come back to the idea it is hard for us to get to, that we are dealing here with a relatively small group of people and not with the population as a whole; and we are dealing with a few people that are going to be exposed to greater hazards by a thousandfold or a millionfold than the genetic exposure.

So, in any of these conclusions it is inherent that we are thinking about people that are involved in this particular problem, not about the population as a whole; and this must never be taken out and used as a separate thing for consideration of the population as a whole. It is entirely limited to this group of people that will be involved in flying these particular planes, which will be infinitesimally small compared to the population as a whole.

To lay any great weight on genetic changes for this group, it is unimportant. It should not be taken into consideration. But, as we stated before, we don't want these data to ever get out as any recommendation that is made for anything else than this particular problem, or a similar problem.

DR. ROBLEY EVANS: There isn't any recommendation here, though, Dr. Stone; just facts.

DR. STONE: He wants to change the facts a little bit to state that we may be underestimating the genetic effects.

DR. ROBLEY EVANS: With all these genetic effects put in terms of spontaneous rates, I feel that the recessives would be taken care of.

DR. HOLLAENDER: Charles takes care more of the sex changes than he does of the small physiological changes or the mental changes.

DR. ROBLEY EVANS: But if you do take the recessives into detail, you get the same result.

CHAIRMAN DOWDY: Maybe we can cover what Dr. Hollaender has in mind by adding a note that it is to be strictly understood that these recommendations are for one specific purpose, namely, to NEPA, and do not apply for domestic or commercial use.

DR. NEWELL: May I point out these are not recommendations. These are estimates. Am I mistaken about that? There is no purpose stated in this that I can recollect.

CHAIRMAN DOWDY: We would like a recommendation that we recommend these to NEPA for their consideration.

DR. FAILLA: That is right.

DR. ROBLEY EVANS: That will be the subject of a motion later.

DR. NEWELL: We furnish NEPA with these as the best data we can get.

CHAIRMAN DOWDY: As a guide; that is right.

DR. NEWELL: But these are not recommendations.

CHAIRMAN DOWDY: We don't make a recommendation on what dosage level they will take.

DR. FAILLA: What is said here about genetic effects is in terms of the naturally occurring rate, so that I think it is perfectly fair. Do you object to those statements?

DR. NEWELL: That is the real point. Are these estimates wrong in that respect?

DR. HOLLAENDER: We don't know. There is so little information available on recessive changes; and the statement made that 100 r means an increase of 1 percent in the hazard, this increase of 1 percent is not really a true expression.

DR. FAILLA: It doesn't say 1 percent. Where is that?

DR. STONE: At the bottom of Page 34.

DR. FAILLA: Oh, yes.

DR. ROBLEY EVANS: Those are delayed effects on the recipient of the radiation.

DR. HOLLAENDER: I don't think we have any figures.

DR. ROBLEY EVANS: I think you have a valid objection. One word might be added in the last phrase, if I get it here, objecting to the third line from the bottom on Page 34, which says that "delayed effects * * * not more than 1 percent from all causes" and so forth. Then you go over to the bottom of Page 35:

> "The expression, 'delayed effects,' as used here, refers to any harmful effects attributable to radiation, manifested at any time subsequent to the period when acute reactions may occur."

The subcommittee definitely had in mind the effects of radiation on the recipient of the radiation. This is not stated under the asterisk, but it is what you are really after. That certainly is what we meant.

CHAIRMAN DOWDY: Yes, that is what we meant.

DR. ROBLEY EVANS: If that will satisfy everybody, I would move such an amendment. It refers to any harmful effects on the recipient of the radiation.

CHAIRMAN DOWDY: Is there a second to that?

DR. HOLLAENDER: I second it.

CHAIRMAN DOWDY: The motion has been made and seconded. Do you want to read that?

DR. ROBLEY EVANS: At the bottom of Page 35, three lines from the bottom, to insert after the word "effects" the words, "on the recipient of the radiation."

CHAIRMAN DOWDY: Which page are you talking about? Page 34 or Page 35?

DR. ROBLEY EVANS: The bottom of Page 35. This is just a definition of delayed effects. Delayed effects do not include two thousand years. They include the remaining life of the pilot.

DR. SELLE: Dr. Robley Evans has moved that the terms "to the recipient" be added or inserted.

DR. FAILLA: May I make another suggestion? We could say that "at any time subsequent to the period when acute reaction may occur in the lifetime of the individual." That might make it a little clearer; I don't know.

DR. FRIEDELL: I have something written up here.

CHAIRMAN DOWDY: Do you mean on this, or the one you were referring to?

DR. FRIEDELL: The one I was referring to.

CHAIRMAN DOWDY: We will be back to that in a minute. Let us get this one cleared.

DR. ROBLEY EVANS: After the word "effects," if you put in "on the recipient of the radiation, and attributable to radiation" —

DR. SELLE: Dr. Robley Evans then has moved that we insert the phrase "on the recipient of the radiation, and attributable * *" and so forth.

CHAIRMAN DOWDY: That was seconded by?

DR. SELLE: Seconded by Dr. Failla.

CHAIRMAN DOWDY: Is there any discussion?

(There was no response.)

CHAIRMAN DOWDY: Those in favor of this will say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

DR. NEWELL: Mr. Chairman, I move that paragraph 2 be put before paragraph 1 and renumbered to fit. I think this difficulty arose because you have been talking about effects on the recipient of the radiation, then you begin talking about genetic abnormalities; then you return to the recipient. I want to change the order of paragraph 1 and 2 under D.

CHAIRMAN DOWDY: You would have to change the order of every one of them, then. Those are all put in the same order all the way through. I don't believe that that is what caused the trouble. I don't think that caused the trouble, Dr. Newell.

DR. NEWELL: You don't think so? O. K.

DR. FRIEDELL: I just had another idea.

CHAIRMAN DOWDY: While Dr. Friedell is cogitating, are there any additional corrections?

DR. TITUS EVANS: I am just wondering if this correction is really foolproof. Even if the damage occurs in the second or third generation, it can be traced back to damage taking place during the lifetime of this individual.

CHAIRMAN DOWDY: We are talking about this particular individual, though, when we say delayed effects.

DR. TITUS EVANS: Back to the recipient of the radiation. Well, you can trace this abnormality back to the injury that took place. That is, when the individual's injury took place.

DR. STONE: Is it harmful to that individual?

DR. HOLLAENDER: It depends whether he is interested in his children or grandchildren. If he isn't, then there is nothing harmful.

DR. ROBLEY EVANS: I think you are all right because that is in separate paragraphs. The entire meaning of delayed effects in the minds of the subcommittee was not genetic.

DR. TITUS EVANS: If you restrict this injury to the time of the individual, I want something about half-way between which would bring in both ideas.

DR. ROBLEY EVANS: Don't you have it, for example, under C-1? That paragraph is exclusively about genetic abnormalities; and uncer C-2, on Page 34, the delayed effects are entirely separate.

DR. TITUS EVANS: Yes.

CHAIRMAN DOWDY: We are speaking about entirely different things.

DR. ROBLEY EVANS: They are very clearly kept apart.

CHAIRMAN DOWDY: Is there anything further? Dr. Friedell.

DR. FRIEDELL: I think I have something here, a little verbose perhaps; but I think it includes the general idea:

> "The following is an indication of the general biological effects to be anticipated at various levels. At each level, except at the highest, there may be considerable individual variations so that no demonstrable clinical changes may be observed in a certain proportion, this proportion being rapidly smaller as the dosage is increased.

"Conversely, biological effects may appear at the lowest levels in a small proportion, this proportion becoming rapidly larger as the dosage level is increased."

That would convey this idea of individual variations.

DR. FAILLA: Where do you accept the largest dose?

DR. FRIEDELL: Of course, I was really thinking of doses even larger than 400.

CHAIRMAN DOWDY: It won't be possible for this particular project.

DR. FAILLA: I will have to disapprove of your statement.

DR. FRIEDELL: I am willing to delete that and stop at 400.

CHAIRMAN DOWDY: And your suggestion is that this go in under the caption we have at the top of Page 34, before we start —

DR. FRIEDELL: That is right; so that there will be an indication that there will be individual variations, so that the total individual may not show much, if anything, even though in general this is what you expect.

I will read it again. It is a little wordy.

"The following is an indication of the general biological effects to be anticipated at various dosage levels. At each level there may be considerable individual variations so that no demonstrable clinical changes may be observed in a small proportion, this proportion becoming rapidly smaller as the dosage is increased.

"Conversely, biological effects may appear at the lowest level in a small proportion, this proportion becoming rapidly larger as the dosage level is increased."

DR. STONE: If you put in "smaller," I think you get down to 25 r which we have included here, and you are not going to get any effects on anybody, detectible effects, I mean.

DR. FRIEDELL: I wouldn't say anybody. I think if you did a thousand people, you would find one or two people that would show some effects. I think there would be some small proportion.

I have not indicated here what it is, to avoid this kind of argument; but if Dr. Cantril were here, he would say that 25 r —

CHAIRMAN DOWDY: I will read you Dr. Cantril's remarks. I will read you the second paragraph. This is from Simon P. Cantril, dated March 23:

"In view of my absence at the meeting, I would like to state, however, that in general I would agree with Section IX entitled, 'Acute Exposure.' My only comments concerning this are: one, I would judge that the problem of nausea at 100 r level will be known, and would believe that some amount of fatigue would be more prevalent than nausea. I would not anticipate that the nausea would be sufficiently prevalent to interfere with the coordination.

"Two, I would anticipate that there would be some more sensitive individual who would succumb to a total acute exposure of 200 r penetrated radiation."

DR. FRIEDELL: So he does agree with the idea that there —

CHAIRMAN DOWDY: I might say, in answering that, that I agreed with him, and said that it would be assumed that there would be some supersensitive individual who might succumb at 200 r, and there might be some considerably nauseated at 100 r.

DR. FRIEDELL: I think you might find one individual in 100 or something in that order, a small proportion, who would have nausea at 25 r.

DR. TITUS EVANS: We have leukemia —

DR. STONE: We are not talking about leukemia. We are talking about healthy normal individuals.

DR. FRIEDELL: We are talking about normal healthy individuals. I think you have 1 in 100. I don't know what the proportion is.

CHAIRMAN DOWDY: I think there will be a psychological personality in which, if you lead the member up to a machine and say, "You are going to get radiation," they will be nauseated before you give it. Those are probably the ones who fail in your group.

That is a problem that NEPA will have to consider, psychological evaluation of personnel with respect to this type of danger.

DR. FRIEDELL: I would even leave those out. I think with any noxious acts you find in a very small proportion extreme degrees of sensitivity that we don't understand. Even morphine, for example; very occasionally you get an individual where a sixth of a grain will kill him.

DR. FAILLA: Do you have any such thing in radiation? You have no idiosyncrasies in radiation.

DR. FRIEDELL: I don't know whether it is idiosyncrasy, if that is what you call it, if there is one individual in a greater percentage that reacts that way. I am taking the symptom that is the least biologically important such as, say, nausea.

I think that in a very small proportion some one individual will show unusual sensitivity. The reason for it, I don't know.

I agree in general with this, and I agree also that you have to examine an awful lot of people at 25 roentgens to find one of them; but I think one would exist.

CHAIRMAN DOWDY: How do you feel, so far as this particular project is concerned; that that is an important factor?

DR. FRIEDELL: No, I don't. But I am merely defending a statement which should begin from the very beginning of 25 r. I am only stating that there will be a proportion that will be affected, the proportion increasing as the dosage increases.

What the proportion at 25 r is, I frankly don't know. But if I have to bet, I will bet that if you just examine enough people, you will find one.

DR. ROBLEY EVANS: Do you mean the fractional standard deviation changes as you go up in dosage, and that you can prove that? That is what you said.

DR. FRIEDELL: Yes, that is what I am saying. I think it narrows down as you go higher. I think if you give 2,000 roentgens, you will kill everybody. But there will be one individual who will survive. That means that the standard —

DR. ROBLEY EVANS: That is still all right. That doesn't prove the standard deviation is changed.

DR. FRIEDELL: Yes, I think that if you go high, the standard deviation, the coefficient of variability goes down. Here you have a very broad group, and here I think you have a very narrow group.

DR. ROBLEY EVANS: Would you be happy if the first line on Page 34 had one more word in it: "Estimated Results to Average Humans Exposed?"

That is what you really want, isn't it?

DR. FRIEDELL: That is what I am doing, except the word "average" needs explanation because you talk about the total individual.

The only point I want to make is that with individual units, some of them may not be affected. That is the only point I want to make, because it may be important in calculating what happens with a crew of ten or twenty. When you get numbers in there, then obviously to make it effective, you might, to affect the whole group, have to go to higher levels.

DR. ROBLEY EVANS: Of course, that is in item 1 of the proposed research schedule for this afternoon. That is the very first thing under consideration.

DR. FRIEDELL: If the committee doesn't feel that it is necessary, I will be perfectly happy to withdraw it.

DR. ROBLEY EVANS: I think you have a good point in getting the word "average" in here.

CHAIRMAN DOWDY: Would that satisfy you, "average"?

DR. FRIEDELL: If it satisfies the committee; that is important. My only point is, we ought to recognize there will be individual variations, and it will take on the form of some kind of proportion.

DR. FAILLA: I have a statement that I think is equivalent to Dr. Friedell's, if you would like to hear it:

> "The estimates given below apply to the average individual. It should be borne in mind that there is considerable variation in individuals' susceptibility to radiation."

DR. ROBLEY EVANS: I second it.

DR. FRIEDELL: I think that would do it. I still would like to hold out for this proportion idea.

CHAIRMAN DOWDY: There is no way to arrive at that proportion. It is a fictitious proportion.

DR. FRIEDELL: Whether we know it or not, I think it exists. That is the point.

CHAIRMAN DOWDY: Doesn't this imply that we have taken that into consideration?

DR. FRIEDELL: Yes. All right, I am willing to accept it.

DR. FAILLA: "The estimates given below apply to the average normal individual. It should be borne in mind that there is considerable variation in individuals' susceptibility to radiation."

CHAIRMAN DOWDY: Will you put that in the form of a motion?

DR. FAILLA: Yes.

DR. ROBLEY EVANS: I will second it.

CHAIRMAN DOWDY: The motion has been made and seconded. Is there any discussion?

(There was no response.)

CHAIRMAN DOWDY: If not, those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

Is there any further discussion?

DR. NEWELL: Mr. Chairman, I think there is too small a spread between statements in regard to fatalities at 200 r and 400 r, because when I came to chart them, I found they fell in the same group.

I am talking about Page 35:

> "D. 200 r; at this level, fatalities, 2-6 weeks after exposures, might occur in a small proportion of the irradiated individuals."

Now we will pass to the last two lines of E:

> "E. 400 r; it would be expected that virtually everyone would be immediately incapacitated by such an amount of radiation, and many would never recover completely. Some deaths would occur in 3 to 6 weeks."

The statements are very nearly the same, and yet the doses are 100 percent apart.

CHAIRMAN DOWDY: I wouldn't agree that the statements are the same.

DR. NEWELL: No, I say they are very nearly the same, because I discovered when I tried to chart them, I didn't have any intermediate group to put them in. They had to go in the same group.

CHAIRMAN DOWDY: Doesn't the first one say that there will be a high proportion of nausea, vomiting, and fatigue —

DR. NEWELL: No, I am talking about fatalities.

CHAIRMAN DOWDY: All right — and a small number of fatalities? And then E, we imply that there will be a large number of fatalities.

DR. NEWELL: You said some deaths would occur.

CHAIRMAN DOWDY: At least incapacitation so far as the total group is concerned. Now, NEPA could not afford to use a dosage which would incapacitate 90 percent of their personnel.

DR. NEWELL: Please, Mr. Chairman, I am talking only about the statement in regard to fatalities.

DR. TITUS EVANS: I think the distinction is, it says "might occur" in the upper one.

DR. NEWELL: That is right.

DR. TITUS EVANS: And in the bottom one it says "would occur." The time of death would normally be about two weeks. I mean, that is the crucial time, two to six weeks, regardless of the dose. The difference is in the frequency of occurrence.

You might make that a little stronger.

DR. NEWELL: Might occur means will occur in a certain percentage of cases.

DR. TITUS EVANS: In a small proportion might occur.

DR. NEWELL: Statistically I was unable to put D and E in regard to fatalities in any except the 10 percent death group. As fine as I was able to group them would be 50 percent dead, 90 percent dead, 10 percent dead; and I had to put both 200 r and 400 r in the 10 percent dead group, which was too close together, I thought, for the difference in the dose.

Now, I think that this remark in regard to "might occur in a small proportion at 200 r" came from the Los Alamos experiments.

CHAIRMAN DOWDY: I don't think so.

DR. FAILLA: I think your point is well taken. Instead of saying some under E at the end of the paragraph there, that indicates that very few would die. Actually, 400 is very close to the LD-50. So there would be more than some.

DR. NEWELL: We can go into this estimate afterwards. It is that I would like to spread out the statements more widely.

Let me make a motion then to say that under D, 200 r, instead of saying "fatalities in 2 to 6 weeks after exposures might occur in a small proportion of the irradiated individuals," say "death from this dose would rarely occur."

DR. FAILLA: That is essentially the same thing.

DR. NEWELL: No, I think there is a difference between rarely and some; but there isn't much difference between might and some.

DR. FAILLA: I think if you change anything, you would have to change the "some."

DR. NEWELL: I would prefer to change the 400.

DR. FAILLA: Yes, I think that is misleading.

CHAIRMAN DOWDY: You think the 400 has been a little low on that? I agree with you on the 400 part of it.

DR. NEWELL: Then let's raise the 400 r. What word would you suggest?

DR. FAILLA: Dr. Nims suggested we leave out the word "some," but leave "deaths" plural.

CHAIRMAN DOWDY: "Deaths would occur."

DR. ROBLEY EVANS: That implies everybody. Would you say an important fraction of the irradiated individuals?

DR. FAILLA: Yes, I would say a considerable fraction.

DR. ROBLEY EVANS: A considerable fraction of irradiated individuals.

DR. STONE: I suggest, Mr. Chairman, these two words were put in here after elaborate discussions before; and the minute you get to changing them around much, you get into the whole problem of what would cause death.

I wouldn't agree to "considerable." So I suggested when I wrote back that you underline "might" in the first one, because I think it is very doubtful that any deaths would occur at 200 r. So, if you emphasized the "might" by underlining it, and underline "would" down below it —

CHAIRMAN DOWDY: I don't want to change the opinion of our group, but I know I was and still am of the opinion that 400 r is very close to the LD-50.

DR. FRIEDELL: In what animal?

CHAIRMAN DOWDY: In man.

DR. FRIEDELL: You have come to that opinion from the study of other animals?

CHAIRMAN DOWDY: I certainly haven't studied it on man, but we have some data on man. It isn't entirely that.

DR. FRIEDELL: I brought along a curve. We have been X-raying some rats that coincide with some other work we did. Of course, the number of animals we have done at the lower levels wouldn't be enough to determine statistically whether 1 percent or 2 percent are killed; but here is the character of the curve.

We have done about 20 animals at various points, and it doesn't begin to break until we get to about 425 roentgen.

Of course, this is a rat in which the LD-50 in this particular group is about 550 r, 525 r; and it seems as if the curve, as you can see, starts down pretty rapidly.

That is the experience with all curves. They go along and then they start to go down pretty abruptly. So I think that 300 or 400 r, if that isn't awfully close to the LD-50, you won't get so many deaths in three to six weeks.

If it is close to the LD-50, then that is another matter.

CHAIRMAN DOWDY: I think it is. Would it satisfy everybody if we underline "might" in D under 200, and "would" under E in 400?

DR. ROBLEY EVANS: I so move.

DR. NEWELL: No.

CHAIRMAN DOWDY: I don't think we can give figures. I don't think you can give percentages.

DR. STONE: That is why we arrived at this expression —

CHAIRMAN DOWDY: That is the reason we arrived at this.

DR. NEWELL: I move that the last line under E, 400 roentgens, to read:

"A considerable fraction would die in three to six weeks."

CHAIRMAN DOWDY: While I am personally in agreement with you on this, we spent a lot of time, as Dr. Failla said, trying to get this where we would get a majority of opinion.

Personally, I think that 350 to 400 is the LD-50 for the human. That is my personal opinion. But I wasn't able to inject that into the Executive Committee.

What we are trying to get here is not your opinion or mine, but a consensus of this group, because you and I may be wrong, although we doubt it; but we could be.

DR. NEWELL: I don't doubt it. That is where I differ from you.

DR. STAFFORD WARREN: It is indefinite enough, but it indicates it is a good possibility, and we can't put a figure on it.

CHAIRMAN DOWDY: Until we can do more work with the human to correlate with the lower ranges in the animal, I don't think we can get any closer to it.

DR. STONE: Dr. Newell's suggestion, "a considerable fraction," might leave you just as wide open as this. A considerable fraction might be one-tenth. Somebody else might consider a considerable fraction to be ten-ninths.

DR. NEWELL: But you will have to admit that a considerable fraction means more to anyone than "might occur."

DR. SHIELDS WARREN: Isn't the essence of it that we have already stated that virtually everyone will be immediately incapacitated? That is the thing that is of importance in the control of an airplane.

So just how the number of deaths is worded seems to me more or less immaterial. If they can't operate the plane, then from the practical standpoint, that is all there is to it.

CHAIRMAN DOWDY: That is written for a specific project, and we always have to keep their objectives in mind in our evaluation. I agree with you. I think it is the LD-50, but I think that probably Dr. Failla might be coaxed to come along with us. We want this to be the consensus of opinion, and we spent a great deal of time trying to word this to give the most information to NEPA without irrevocably committing ourselves to something.

DR. NEWELL: I am only trying to be helpful. I made a suggestion. I do think sometime that there is not a sufficient spread between 200 r and 400 r in regard to the statement for fatalities, and I would like to spread it further.

My first suggestion was that we spread it by making the statement less strong for 200 r; but nearly everybody agrees that the statement was not too strong in regard to 200 r.

Therefore I tried to find words to strengthen the statement somewhat in regard to 400 r. I don't know whether anybody seconded my amendment.

DR. ROBLEY EVANS: The only amendment that has been seconded was mine.

CHAIRMAN DOWDY: Do you care to put that in a motion? If so, I will call for a second. Do you care to put that in a motion?

DR. NEWELL: I move that the last line of E be changed to read:

"A considerable fraction would die in three to six weeks."

CHAIRMAN DOWDY: Is there a second to that motion?

(There was no response.)

CHAIRMAN DOWDY: There is no second.

The Chair would entertain a motion for underlining these.

DR. ROBLEY EVANS: That has been moved and seconded.

DR. SELLE: We didn't get a second on that.

DR. STONE: Dr. Dowdy, for a point of information, is this not a misprint, at the 200 level two to six weeks, and at the 400 level, three to six weeks? Weren't they both supposed to be three to six weeks? The second one would be two to six weeks rather than the first one.

CHAIRMAN DOWDY: That could be. But wouldn't your deaths be more at 400? According to the way you think, wouldn't they be apt to occur between three and six weeks?

DR. STONE: Yes, but why leave two to six weeks in the first one, you see. I think in the second one it might be two to six weeks.

CHAIRMAN DOWDY: We could just change those.

DR. STONE: You could leave them both two to six weeks, but I don't think you ought to have one as two to six weeks, and the other one three to six weeks.

CHAIRMAN DOWDY: Let's put them both two to six weeks.

DR. STONE: I will move that, if you want a motion.

CHAIRMAN DOWDY: That is on the change here?

DR. STONE: Yes, two to six weeks on E in the last line.

CHAIRMAN DOWDY: The motion has been made and seconded that "three" in the last line of E on Page 35 be changed to "two."

All those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

We would still entertain a motion if you want to underline.

DR. ROBLEY EVANS: I move that on Page 35 in the second line the word "might" be underlined and on Page 35 in the last line the word "would" be underlined.

CHAIRMAN DOWDY: Any second?

DR. STONE: Yes, I second it.

CHAIRMAN DOWDY: The motion has been made and seconded that "might" in the second line on Page 35 be underlined; and that "would" in the last line under E, Page 35, be underlined.

All those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

DR. STONE: There is one other question I would like to bring up here. I don't think it is academic. Under D in the last sentence it says:

"Temporary sterility in some cases and possibly permanent sterility in rare instances."

Now, I don't know who it was that wanted to get that in under such restricted materials, but I think that if anyone agrees that permanent sterility is likely to occur with that dose, that it ought to be included. But if it isn't likely to occur, I think it ought to be left out, because it has a definite influence on a person. If they even are faced with the thing put up in that way, possibly permanent in rare instances, it is hedging it very much. I would like to see that part deleted.

To get it on the board, I would say let's delete all after "and" and say — the sentence isn't complete — but says:

"Temporary sterility in some cases may be produced."

CHAIRMAN DOWDY: May be produced?

DR. STONE: Yes.

CHAIRMAN DOWDY: Is there a second to that motion?

DR. NEWELL: I second it.

CHAIRMAN DOWDY: The motion has been made and seconded that next to the last line of paragraph 1, Page 35, be corrected to read:

"Temporary sterility in some cases might ensue."

DR. FAILLA: I think the addition of the verb is unnecessary unless we do that on all these other statements. In other words, it is implied.

DR. STONE: We have got "will probably occur."

DR. FAILLA: We have to re-word all of these things, you see.

DR. STONE: Just temporary sterility in some cases, omitting everything after that.

DR. SHIELDS WARREN: I wonder if there are not enough instances of oligospermia in the general population so there might well be a chance for permanent sterility in some individual?

I would agree that in the normal individual with a normal sperm count, it would probably be only a temporary sterility. On the other hand, we have a very appreciable proportion of the population that runs a subnormal or a low count a good share of the time.

Have we any knowledge as to what is going to happen with that group?

DR. STONE: No, but reasoning by analogy with the blood count, I don't think the height of the count when you start has much bearing on the depression that occurs as a person with a 4000 total count doesn't necessarily drop out the bottom of the picture any more than —

CHAIRMAN DOWDY: I think Dr. Shields Warren's point was, though, that if you have a low count to start with, and you drop 50 percent, they are sterile; whereas otherwise they wouldn't be, a 50 percent drop in the normal wouldn't mean sterility.

DR. STONE: I don't think that group would drop 50 percent.

CHAIRMAN DOWDY: You would have to assume that there wouldn't be any more resistance than in the normal individual. Therefore, if you got the same percentage of reduction, you would reach a point of sterility from lack of number sooner than you would in the normal.

I think the point is well taken.

DR. NEWELL: It is awfully close to a quibble. However, you can escape it if you want to say:

"Some normal men will be rendered sterile temporarily."

DR. STONE: Say "temporary sterility in some cases."

DR. FRIEDELL: I would like to sound a little different note. I would suggest we change this as little as possible, because the wording is such to indicate that this is what we are thinking about; and I don't think that any of us here have actually definite specific information good enough to propose changes in here that ought to stay.

Therefore, I would think that we ought to accept this if it in principle agreed with what we are thinking about, rather than making minor changes here.

DR. STONE: I doubt that this is a minor change. That is why I suggested it, because putting in the possibility of permanent sterility changes the influence of this statement.

For instance, if you or I asked for volunteers to do this work and you put in here that there is possible permanent sterility over and above the other, you are likely to run into more psychological resistance than if you left it out.

So, unless we feel that it should be in, I think it ought to be left out.

DR. FRIEDELL: Of course, you haven't fixed again the number of people out of a thousand that will be permanently sterile, and I think it is conceivable that some one individual out of a great many might prove to be sterile, just enough to do this.

So you face the same sort of arguments over again; and since possibly it doesn't have any particular number attached to it, it is probably right. It is possibly right.

CHAIRMAN DOWDY: We can dispose of this. We had a motion that has been seconded. We can put it to a vote.

All those in favor say "aye."

(Response: Aye.)

CHAIRMAN DOWDY: Opposed?

(Response: Aye.)

CHAIRMAN DOWDY: I guess we will have to take a count.

MR. SIMMONS: Would you restate the motion?

CHAIRMAN DOWDY: Read the motion again.

DR. SELLE: Dr. Stone moved that the last sentence in D read:

"Temporary sterility in some cases."

This was seconded by Dr. Newell.

CHAIRMAN DOWDY: Now then, those in favor please hold their right hands up. (Counting) We have four.

Opposed, please raise your right hand. (Counting)

The motion does not carry.

DR. TITUS EVANS: The thing that worries me is protraction. We haven't mentioned how this dose is to be given. If sterility in the male might occur if it were given four times fifty or ten times ten, I don't know --

DR. STONE: This is acute dosage. An acute dosage is three to six hours, or three to twenty-four hours.

DR. TITUS EVANS: I thought it was, but I didn't read it in here.

DR. STONE: This is for people operating an airplane.

MR. SIMMONS: This discussion about the limit of applicability of this scale of exposure risk has worried me a little bit, and it has been stated that this is only applicable to people flying airplanes.

It was my understanding that this was in a sense an attempt to define some actuarial data which would state the risk associated with various exposure levels for man, and that the applicability to aircraft operation came in the selection of the number of r that we would permit the crew to take, which would be selected by looking at the risks that you would encounter from this Article IX.

For example, assume that it is accepted including the statement of risks associated with each dose. It would seem to me that we should know what the objective will be without considering whether the man was going to get the dose in an airplane or in a laboratory. This is what will probably happen to anybody who gets such exposure.

CHAIRMAN DOWDY: We feel that there is no particular immunity because he flies an airplane.

MR. SIMMONS: Because of the importance of the mission and the nature of the group that is carrying it out, you might select a higher dose for that group out of this table than for another group.

CHAIRMAN DOWDY: The thing I was trying to emphasize was, we are charged with the particular problem as far as we were concerned; and if we got off on too many of these equivocal points, we would dissipate our energies and not accomplish our objectives.

If there are no further corrections, the Chair would entertain a motion to the effect that this section on Acute Exposure be recommended to NEPA for their use.

DR. STONE: Following along the suggestion just made, should we have a little more definition of "acute exposure," say, exposure within twenty-four hours, because we definitely don't mean that this could be 25 r once a week built up to 100 r.

DR. TITUS EVANS: But it could be over a two-day flight or a three-day flight. I mean, it is continuous exposure.

DR. STONE: That is what I am saying. Under Acute Exposure, do we want to say:

"Acute exposure within a twenty-four hour period"

or something of that nature?

DR. TITUS EVANS: I think we should.

CHAIRMAN DOWDY: Forty-eight hour. I am afraid that we might pin them down too close if we say twenty-four.

DR. STONE: Anything over twenty-four would be still better.

CHAIRMAN DOWDY: Then, if we reason from that, if this were given in three hours, then anything given longer than that would be better.

DR. STONE: I think so. Acute exposure, three hours or more.

CHAIRMAN DOWDY: Well, I am wondering if we really have to put that in now?

DR. STONE: Acute exposure might mean in terms of a lot of people's interpretation ten minutes. Do we mean ten minutes?

CHAIRMAN DOWDY: Why not "instantaneous"?

DR. NEWELL: May I read the definition of acute exposure which our Committee of Consultants arrived at in December, which was circularized?

The definition of acute exposure was "within a few hours (one to six), excluding instantaneous exposure."

CHAIRMAN DOWDY: I don't think we really have to even exclude instantaneous. That is acute. There couldn't be anything more acute.

DR. STONE: You might run into a lot of discussion about instantaneous exposure.

DR. NEWELL: We excluded instantaneous exposure on the proposition that an instantaneous exposure would be something that the commander could not do anything about, so we weren't collecting any ideas about it at all.

DR. FAILLA: In view of Dr. Newell's statement, with that definition, I think you have to define what you mean by acute here. Otherwise, in the absence of the definition, it may be assumed to be the same as for the other. Six hours is too short a time for this project.

DR. STAFFORD WARREN: What is the longest period you would agree to it being acute that would be less than a week?

CHAIRMAN DOWDY: Why not say "continuous exposure, excluding instantaneous exposure, up to seventy-two hours"?

DR. ROBLEY EVANS: I think we all were figuring under twenty-four hours on this.

DR. STAFFORD WARREN: What is the likely trip? Twenty-four hours?

DR. STONE: If it holds good for twenty-four, it certainly would be better for forty-eight.

CHAIRMAN DOWDY: Just excluding "instantaneous" would be satisfactory? Or do you want to define it more specifically?

MR. KALITINSKY: Isn't that something like saying substantially "continuous single exposure"?

DR. ROBLEY EVANS: In four to twenty-four hours. Wasn't this written for four to twenty-four hours?

CHAIRMAN DOWDY: I think our feeling was within twenty-four hours.

DR. STONE: I think maybe part of this would be covered if the acute exposure were to be taken away from the left-hand side of IX and put in here "Estimated results of exposure of 2000 to 4000 KVP X-rays measured in air within twenty-four hours," something like that.

Your acute exposure is on the wrong side there. It modifies the thing before you get started.

CHAIRMAN DOWDY: If we just put an asterisk up there at "Acute" and then a definition of acute, that would save editing.

MR. SIMMONS: Hasn't everyone been thinking more or less in terms of twenty-four hours?

DR. STONE: Yes.

CHAIRMAN DOWDY: I would say "continuous acute" as defined here means continuous irradiation for a period of twenty-four hours, excluding instantaneous radiation.

DR. FAILLA: For a period up to twenty-four hours.

CHAIRMAN DOWDY: Up to twenty-four hours, excluding instantaneous radiation.

DR. SHIELDS WARREN: Aren't you safe in making it continuous? Wouldn't there be two or three interrupted slugs during the twenty-four hours that would be just as rough as a continuous twenty-four hours?

CHAIRMAN DOWDY: I think it probably would.

DR. FAILLA: I wouldn't limit it to instantaneous. It may be an emergency and a man has to go in and get a shot.

CHAIRMAN DOWDY: Accumulated dose? Within twenty-four hours?

DR. SHIELDS WARREN: Yes.

CHAIRMAN DOWDY: Accumulated dose within twenty-four hours.

DR. FAILLA: That is understood. I think all we have to define is acute. By "acute" we mean during the period of twenty-four hours that radiation has been received.

CHAIRMAN DOWDY: By putting parentheses after "acute," twenty-four hour?

DR. FAILLA: Say up to twenty-four hours.

CHAIRMAN DOWDY: Don't you have to exclude instantaneous?

DR. FAILLA: I wouldn't say anything about it.

DR. ROBLEY EVANS: Do we know enough about it to exclude it?

CHAIRMAN DOWDY: Then why are we worrying about all this?

DR. ROBLEY EVANS: I don't know. I was wondering myself.

DR. STAFFORD WARREN: Why not say "approximately twenty-four hours," because it might be twenty-five.

DR. FAILLA: We are not exactly quibbling because of the other definition received. That is coming out in a report, and then in the absence of any definition of acute here, it may be assumed that —

CHAIRMAN DOWDY: We are conforming.

DR. NEWELL: As a matter of fact, you complained that we don't know anything about continuous exposure. That is the very reason for saying we are not talking about it. That is the very reason for ruling it out as a definition if you don't know anything about it, because you do think you know something about the other.

DR. FAILLA: Why don't we just put an asterisk here and say, "within approximately twenty-four hours"?

DR. STAFFORD WARREN: Couldn't we say just "approximately twenty-four hours"?

CHAIRMAN DOWDY: Put that in the form of a motion.

DR. FAILLA: I move that after the word "acute" in the very first line of Page 34, we put an asterisk for a footnote saying:

> "Within twenty-four hours" or "received within about twenty-four hours."

DR. ROBLEY EVANS: I second it.

CHAIRMAN DOWDY: The motion has been made and seconded than we put an asterisk after "acute" in the first line on Page 34 with a footnote, "within approximately twenty-four hours."

DR. STONE: The IX over on the left-hand side of the acute.

DR. NEWELL: I move an amendment, Mr. Chairman, to add in parentheses "excluding instantaneous exposure."

CHAIRMAN DOWDY: You would have to have a second for that?

DR. NEWELL: I would have to have a second.

CHAIRMAN DOWDY: Does anybody second the amendment?

(There was no response.)

CHAIRMAN DOWDY: No second.

Would you read the motion?

DR. SELLE: It has been moved by Dr. Failla, on Page 34, following the first line, we put an asterisk after the term —.

DR. FAILLA: The very first word you see on that is "acute." Put a footnote for that.

DR. SELLE: The term "acute," we put an asterisk for a footnote —

DR. FAILLA: "Received within twenty-four hours."

CHAIRMAN DOWDY: All those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried. Dr. Stone, that is a typographical error, that statement of yours. We can change that.

Are there any further comments before we proceed?

(There was no response.)

CHAIRMAN DOWDY: The Chair would entertain a motion, then, for acceptance of this.

DR. FAILLA: I move that Section IX be adopted.

DR. ROBLEY EVANS: I second it.

CHAIRMAN DOWDY: Is there any discussion?

DR. HOLLAENDER: I would like to emphasize again, that couldn't some statement be put in that this is specifically for NEPA and should be not considered if it is applied to a larger number exposed? Otherwise, this may be quoted as a recommendation.

CHAIRMAN DOWDY: The facts are the same.

DR. ROBLEY EVANS: And there is no recommendation here. It is a statement of fact.

CHAIRMAN DOWDY: So it wouldn't make any difference whether it was by accident or design.

DR. HOLLAENDER: If it means an exposure of a very large number of men, I think more emphasis would have to be put on possible genetic effects.

MR. SIMMONS: Isn't it true that if a large number of men or a very small number of men are exposed, that the results are comparable? The assumptions are correct.

CHAIRMAN DOWDY: It doesn't make any difference whether it is ten or a thousand. We still think this would happen, whoever is using this.

MR. SIMMONS: You are not recommending any of those exposures —

CHAIRMAN DOWDY: They would have to pick their hazard at a different level. That is up to them. That is not up to us.

All those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

MR. SIMMONS: What are we voting on?

CHAIRMAN DOWDY: The motion has been made and seconded that we adopt Section IX, as corrected. It has been made and seconded.

All those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: Carried. Thank you, gentlemen.

This brings us to the next part of our program.

I would like to call on Dr. Stone, who is Chairman of and who will report on the Committee of Human Radiation Tolerance.

Dr. Stone.

DR. STONE: The problem of radiating humans has been discussed at different times, quite heavily discussed at the meeting of the Executive Committee.

There are two very pertinent articles that have been in the literature recently on this problem, which I will not take your time to go into, unless you wanted to do so.

On February 14, 1948, an article appeared on the "Ethics Governing the Service of Prisoners as Subjects in Medical Experiments," by a committee appointed by Governor Green of Illinois in connection with the human experimentation on prisoners at the prisons of Illinois.

More recently, in July of 1948, Dr. Ivy published an article in SCIENCE on "The History and Ethics of the Use of Human Subjects in Medical Experiments." Both of these articles were taken into consideration in bringing forth a general motion or general report that we would like to bring here.

Our committee has not had a separate meeting, but they are in essential agreement, as I have contacted them individually; and I have this statement to read; which we would like to propose for adoption as a general statement, without going into detail of what might be done.

"The use of human beings as experimental subjects has been countenanced in the past when information was needed that could not be obtained in any other way. The danger of the experiments must be previously investigated by animal experimentation.

"The NEPA Medical Advisory Committee on the Radiation Tolerance of Military Personnel is attempting to establish what would happen to humans when necessarily exposed at infrequent times to certain amounts of radiation that are high relative to the dose set up as the maximum permissible daily or weekly dose for workers constantly working where radiation exposure is possible.

"The doses are, however, relatively low with relation to lethal doses. A review of animal experiments has shown that not only do animals of various species differ in their response to given amounts of radiation, but also that animals of different strains within a given species differ.

"It is therefore impossible to predict with sufficient accuracy what will happen to humans.

"The exposure of sick humans to radiations to the total body with therapy as the objective has provided some indication of how sick people respond, but such therapeutic trials have shown also that the response varies greatly with the clinical condition of the patient.

"A few accidental exposures have provided a little information as to how relatively healthy people respond, but the number of individuals so exposed has been too few to provide statistically significant results and the conditions of exposure are not sufficiently well known.

"The information desired is sufficiently important to the safety of the U.S.A. that we believe the use of humans is justified. It is understood that any such experimentation would be carried on in accordance with the principles laid down by the Judicial Council of the A.M.A. in 1946 as follows.

"First, the voluntary consent of the person on whom the experiment is to be performed must be obtained."

I added in brackets here what is not in the Judicial Council's report, that volunteering exists when a person is able to say Yes or No without fear of being punished or of being deprived of privileges due him in the ordinary course of events.

"Two, the danger of each experiment must have been previously investigated by animal experimentation.

"Three, the experiment must be performed under proper medical protection and management.

"In view of the above we, the members of the NEPA Medical Advisory Committee on Radiation Tolerance of Military Personnel, hereby express our belief that human experimentation is necessary and request the Armed Services to arrange for it."

CHAIRMAN DOWDY: Would you include these two references that you gave, Dr. Stone, in your report?

DR. STONE: We can include them as references at the bottom.

DR. ROBLEY EVANS: Also the reference to the 1946 Judicial Committee.

DR. STONE: Then we could include in the footnotes the reference to the report of the Committee of Governor Green, the reference to the Judicial Committee's report, and the reference to Dr. Ivy's article in SCIENCE.

Mr. Chairman, I move that the Committee as a whole accept this as a general statement.

CHAIRMAN DOWDY: Thank you, Dr. Stone. As Dr. Stone said, in any discussion of this we would like to stay away from any details or any specific experiments. The question open for discussion is the adoption of this as a general principle.

Is there any discussion?

DR. STONE: Dr. Warren, who is a member of the committee, but who was not contacted before this was read, has asked that we add in here a request of the Armed Services to arrange for it, and that the experiments be unclassified.

DR. FAILLA: I think it ought to be more definite than that, since classification doesn't mean very much to the average individual, that the information be made available or be unrestricted.

DR. STONE: The information obtained be unrestricted?

DR. SHIELDS WARREN: I would say not only the information obtained, but the experiments as well. I think it very important in something of this sort that there be no suspicion that anything is being hidden or covered up, that it is all being done openly and straightforwardly.

DR. STONE: I think unclassified.

DR. FAILLA: That is the word, yes.

DR. STONE: And that the experiments be unclassified.

CHAIRMAN DOWDY: Experiments and results.

DR. NEWELL: The technical term is classified data? Is that right?

CHAIRMAN DOWDY: If you put that as data, I mean the data then would be open for distribution, but not necessarily the protocol.

DR. FRIEDELL: Would it be better to say unrestricted rather than unclassified? Classification, as I understand it, is a military term. Is that correct?

CHAIRMAN DOWDY: Not necessarily.

DR. SHIELDS WARREN: We are asking that this be undertaken by the military, so we have to use that phaseology.

DR. FRIEDELL: One other point I would like to make in this regard. I am just wondering whether someone else ought not to hold the bag along with us with regard to making such a recommendation.

Previously in medical experiments the physicians and doctors have made such recommendations because the problem was primarily a medical one. I think this is something larger than that. It is really not a medical problem alone. It has to do with how critical this is with regard to safety to the nation.

Therefore, it would seem to me that the recommendation ought to come not only from this committee but from some other larger organization that has studied and recognized the critical nature of this.

CHAIRMAN DOWDY: If we accept this and pass it on to NEPA, they would have to get recommendations, too, probably from the military personnel.

We have no other body to call on that I know of for this particular purpose.

DR. NILES: Shouldn't there be a recommendation in there along with this that some other agency —

DR. STONE: I don't think that is necessary. I think we are gathered together for the purpose of deciding what we want to do, what we think should be done. If anybody wants to do it, to get other authority or other advice, they can do it.

MR. WARD: I think, to answer that question, NEPA will certainly take that up through the channels of The Surgeon General's Office, so it would be an official Surgeon's recommendation.

DR. FAILLA: The thing that bothers me is this is linked with the safety of the country. I don't think this information should be safeguarded.

DR. STONE: I didn't say safeguard. Sufficient information for the safety of the U.S.A.

DR. ROBLEY EVANS: If he says "security," will that be all right?

CHAIRMAN DOWDY: In a way I think we do, Dr. Failla.

DR. FAILLA: The information we really want we don't get for four or five years.

DR. NEWELL: We can't start any sooner.

DR. TITUS EVANS: I sort of feel if it is vital to our country it shouldn't be laid open to newspaper publicity, like for the Society for the Prevention of Cruelty to Animals, or something.

Maybe we should go a little slow about bringing it too much out in the open if it is that vital.

DR. ROBLEY EVANS: We don't have to advertise it, but at the same time it doesn't want to be concealed, as Dr. Shields Warren has said.

DR. STONE: That particular problem would be a problem for the people actually doing the experiments when it gets there. Our recommendation would be that it be made available.

DR. FAILLA: I think if you said "highly desirable" instead of "necessary," I would be more inclined to go along with it.

DR. STONE: "Sufficiently important for the safety of the U.S.A." is the wording. It doesn't say absolutely necessary.

DR. FAILLA: At the end there you have "necessary."

DR. STONE: The human experimentation is necessary.

DR. FAILLA: Yes. I am not sure it is, for the safety of the country, now.

DR. STONE: I didn't put that there.

DR. FAILLA: But it is in the same statement.

DR. STONE: If you don't think it is necessary for the safety of the country, what do you think it is necessary for?

DR. FAILLA: I don't think it is even necessary. I think it is highly desirable.

CHAIRMAN DOWDY: Well, from the origin of radiology or radiation, humans have been used for everything on testing and various things. We thought it was necessary then.

DR. STAFFORD WARREN: One of the troubles with our deliberations is we don't have data to give firm recommendations to NEPA, and we are in the same bind as you are in the therapy clinic.

Unless you know what your dose is and where you can expect damage and what you can do to avoid it, you can't conduct a logical sequence of treatments. Here you can't conduct a logical military program unless you know.

I think that we are all aware of the difficulty in establishing points. I doubt whether anybody here would want to subject anybody to 400 r, but we ought to know what the symptoms are from small doses. Isn't that in a way necessary for the safety of the country?

We are not only concerned here with NEPA. We are going to have pretty soon a large civil defense program where these same dosage levels will be batted around. Are we going to agree that 25, maybe 50, r exposure is worth doing something for a civilian installation?

DR. FAILLA: I agree that it would perhaps have psychological value, and you could then say, "We did expose 100 people to 200 roentgens, and we found that within two or three years it didn't make any difference." But you know that now.

CHAIRMAN DOWDY: Do we?

DR. FAILLA: Yes, I think we know that now. But then, the question is, what is the period of observation? If the period of observation is one year, I think we know now that there isn't going to be any appreciable effect.

DR. STONE: We had a discussion here this morning —

DR. FAILLA: But the effect in twenty-five years, we don't know. Maybe some of these people will develop symptoms.

CHAIRMAN DOWDY: I don't think we know what will happen to a human at 200 r total body radiation within an hour's time, or a half hour's time. I think it is very vital, when it comes to civilian defense, in knowing how to take care of the casualties.

If 90 percent of the people die at 300 r, or 200 r — we will use that — then it is not worthwhile to spend your medical efforts on that group, and you put your efforts on the ones that you know are going to have a high percentage of them living.

DR. FAILLA: I took 200 r because that would be on the borderline. But, according to the resolution, it says there, for exposures, "small in comparison to the lethal dose."

Well, 200 r would not be a small dose in comparison with the lethal dose. So this work would apply to something like 100 or 150 r. I think we know now what we might expect from such an exposure.

CHAIRMAN DOWDY: What we might expect is an extrapolation partially, and partially on sick people.

DR. FAILLA: Dr. Stone has a radiation of 300 roentgens.

DR. STONE: That is in a month's time. This is acute exposure, 200 r. I may say that we specifically excluded from this report a discussion of just what experiments were necessarily done in the period of observation, to get if we could, an agreement on the idea that human experimentation should go on.

Then, if the group decides that they want to, this afternoon, in discussing specific recommendations, discuss what should be done, that is up to the committee as a whole.

CHAIRMAN DOWDY: Dr. Stone, would it weaken your statement there to alter it so that Dr. Failla —

DR. FAILLA: Say "highly desirable"?

CHAIRMAN DOWDY: Highly desirable. That wouldn't weaken our proposal any, would it?

DR. STONE: They have got to be pretty nearly necessary before you go to giving people —

DR. FRIEDELL: Isn't that necessary there with regard to determining the dosage levels? Isn't that where the "necessary" comes in? You say it is necessary to do human experiments in order to fix these levels, and it is sufficiently important to do human experimentation. Isn't that the wording at present?

DR. STONE: Well, the wording that Dr. Failla is objecting to is in one paragraph here. It says:

> "The information desired is sufficiently important for the safety of the U.S.A. that we believe the use of humans is justified."

In other words, you have got to justify why you think this is necessary. I think it is necessary for the safety of the U.S.A. If the Committee doesn't agree with that, we can amend that out of there.

DR. STAFFORD WARREN: That is a pretty conservative statement. Why don't you read it and see? If this is so important, it would be a good idea to have it unanimous, if we could.

DR. TITUS EVANS: By saying it is "necessary," do you mean all the animal experiments and what we have from patients is not any good at all?

DR. STONE: Oh, no, that isn't stated here. It is stated that we need to get some human experimentation to take us over the gap, is what it amounts to. We have found that animals vary.

DR. TITUS EVANS: But you wouldn't put all the emphasis on that. You wouldn't set the results you got from these human cases alone, would you?

DR. STONE: You would use all your animal experimentations as your basis for doing this work.

DR. TITUS EVANS: But you would take these results, these data, as the standard, and you would put emphasis on that; and aren't you afraid that you might overemphasize that? I mean, we can't measure these things in the field very carefully. There is a possibility of overemphasizing the details of such data.

DR. STONE: Again let's look at what we are looking at here. I agree that you won't be able to study genetic effects on these people at all. So far as that is concerned, that is ruled out.

But for the purposes that we want here, to expose some individuals, you will assume that genetic effects are going to take place. What you want to know is, what is going to happen to these people.

We can't get any agreement here on what is going to happen. Some people want to put in that 200 r will kill some people, and others say we know what will happen at 200 r. That is evidence to me that if we want to be able to talk intelligently about this at all, and if this project to NEPA is to go ahead and they are to ask people to go, you want to be able to say to these people, "We have exposed 100 men to 25 r, and we did it five years ago. They are perfectly all right now. We can't detect anything."

DR. TITUS EVANS: I just have the feeling that we will come to regard that as another experiment, and we will say, "That was the results they got over there, but if we had done it at five places, we would feel better about it."

DR. STONE: Let them do it at five places if they want.

DR. FAILLA: That is immaterial to the question under discussion.

CHAIRMAN DOWDY: We are getting into detail. That is a different problem.

MR. WARD: Mr. Chairman, could we have that read again so that we are sure we all understand what we are discussion?

CHAIRMAN DOWDY: Would you read your note again?

DR. STONE: "The use of human beings as experimental subjects has been countenanced in the past when information was needed that could not be obtained in any other way. The danger of the experiments must be previously investigated by animal experimentation."

That is poor English, but I think that is the idea.

"The NEPA Medical Advisory Committee on the Radiation Tolerance of Military Personnel is attempting to establish what would happen to humans when necessarily exposed at infrequent times to certain amounts

of radiation that are high relative to the dose set up as the maximum permissible daily or weekly dose for workers constantly working where radiation exposure is possible.

"The doses are, however, relatively low with relation to lethal doses. A review of animal experiments has shown that not only do animals of various species differ in their response to given amounts of radiation, but also that animals of different strains within a given species differ.

"It is therefore impossible to predict with sufficient accuracy what will happen to humans.

"The exposure of sick humans to radiations to the total body with therapy as the objective has provided some indication of how sick people respond, but such therapeutic trials have shown also that the response varies greatly with the clinical condition of the patient.

"A few accidental exposures have provided a little information as to how relatively healthy people respond, but the number of individuals so exposed has been too few to provide statistically significant results and the conditions of exposure are not sufficiently well known.

"The information desired is sufficiently important to the safety of the U.S.A. that we believe the use of humans is justified. It is understood that any such experimentation would be carried on in accordance with the principles laid down by the Judicial Council of the A.M.A. in 1946 as follows:

"First, the voluntary consent of the person on whom the experiment is to be performed must be obtained."

I might do well to leave that other sentence out that I read in there because it isn't part of the Judicial Council's ruling. So, if we leave that out:

"The voluntary consent of the person on whom the experiment is to be performed must be obtained.

"Second, the danger of each experiment must have been previously investigated by animal experimentation.

"Third, the experiment must be performed under proper medical protection and management.

"In view of the above we, the members of the NEPA Medical Advisory Committee on Radiation Tolerance of Military Personnel, hereby express our belief that human experimentation is necessary and request the Armed Services to arrange for it, and request that the experiments be unclassified."

We could maybe word that last sentence and recommend that the experiments be unclassified. We won't have any control over that, but we can recommend it.

CHAIRMAN DOWDY: Dr. Stone has moved that this Committee adopt the report of the Committee on Human Radiation Problems.

DR. ROBLEY EVANS: I second it.

CHAIRMAN DOWDY: The motion has been moved and seconded. Is there any further discussion?

DR. NEWELL: Dr. Failla, you don't want to move an amendment to raise the level of experimentation to above that at which it is small compared to the lethal dose?

CHAIRMAN DOWDY: That is something that is specific details, which has nothing to do —

DR. NEWELL: You would put it in this report, that the doses be small in relation to the lethal dose; and both Dr. Failla and I feel it would be too bad if you are not permitted to run the experiment up into the level of 200 or 300 roentgens.

DR. FRIEDELL: What was the consensus of opinion of the members of the subcommittee as to how high you would go, Dr. Stone?

DR. STONE: I only had two definite statements that came back, that were mailed to me. Dr. Friedell stated he thought it ought to go up to 150. Dr. Cantril thought 50 was the limit. So that is why I left out anything except the general statement.

DR. STAFFORD WARREN: I think that could be left to a committee that could discuss and argue this out.

CHAIRMAN DOWDY: I think if we get into the details of it and the dosage —

DR. NEWELL: I don't think that is a detail. I think that that is an unfortunate limitation, if you feel that you have to put it in.

CHAIRMAN DOWDY: It isn't in there.

DR. FAILLA: It is in there. "Small in comparison to the lethal dose."

DR. STONE: "In attempting to establish what would happen to humans when necessarily exposed at infrequent times to certain amounts of radiation that are high relative to the dose set up by the maximum permissible daily or weekly dose for workers constantly exposed. The doses are, however, relatively low with relation to lethal doses."

Now, we can leave that sentence out rather easily, if you want to.

DR. NEWELL: I move an amendment that we leave that sentence out.

DR. FAILLA: I second that. I agree with you.

CHAIRMAN DOWDY: All those in favor of the amendment say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The amendment is carried.

DR. HOLLAENDER: Could we get the statement and read it over individually so that we are sure —

DR. SHIELDS WARREN: I think that is a very good suggestion. It is a pretty important recommendation, and I think that it is important that we not hamstring the actual experimenters themselves.

We ought to leave the statement as broad as possible and in the details of the protocol. I think if we could each see this individually, it would be helpful.

There is one other suggestion I would like to make in relation to the amount of experience which is cited. That is, we have a considerable number of individuals among the Hiroshima and Nagasaki survivors; but the difficulty of estimating what the dose levels they received were is very great. I think that, unless some mention is made of that, it might not make our case as strong as otherwise.

CHAIRMAN DOWDY: We are getting along pretty well on schedule. Why don't we adjourn now —

DR. NEWELL: We haven't acted on this report as a whole yet.

CHAIRMAN DOWDY: We are going to have that typed and bring that up for action.

Also, Dr. Failla brought along some proposals which he had written out and didn't get to me, but brought them with him today. We will pass these around. I don't know whether there are enough to go around or not. Will you also read those during the noon hour.

DR. NEWELL: Do you want to offer this, too?

CHAIRMAN DOWDY: I don't think so. I don't want to get into that.

DR. NEWELL: I wondered if you wanted me to distribute these to those who haven't received them.

CHAIRMAN DOWDY: You can do that if you want, for information. The first thing we will do is take up Dr. Stone's recommendation and then carry on with the research problems which I have already circulated.

DR. ROBLEY EVANS: Going back to this classification question, the blue document which you stated received the classification because of the inclusion of the Los Alamos data, the value of Pages 34 and 35 to students in training in radiobiology, I think, is very great.

I would like to move that Pages 34 and 35, when retyped with the amendments, be issued unclassified.

CHAIRMAN DOWDY: We had already discussed that, and there are several referred to on Page 3. What we have planned to do is to go through and take out all that information and get a completely declassified document for publication. Then you will have, each one of you, the classified document which is complete.

DR. ROBLEY EVANS: How long will that take? We are in the middle of the term right now, you know. If we could use it once. —

CHAIRMAN DOWDY: We will have it ready for your class next year.

DR. ROBLEY EVANS: I want it tomorrow. I have twenty-three students who want it tomorrow.

CHAIRMAN DOWDY: Do we have a motion for adjournment?

DR. FRIEDELL: I would think that it might be better to save the time at the other end. If there is something we can spend a half hour on and adjourn at a quarter after twelve, it might be a little better.

CHAIRMAN DOWDY: We are going to have to dismiss some of them, at least, to revise this report of Dr. Stone's.

DR. FRIEDELL: Couldn't they do that during the lunch hour?

CHAIRMAN DOWDY: Why don't we say we come back at 1:30? We will gain another fifteen minutes. Can we all make it back at 1:30? Couldn't we say we report back at 1:30 and then we won't interrupt that period for discussion.

If that is agreeable, we will reconvene at 1:30 rather than 2:00.

(Whereupon, at 11:45 a.m., the Committee recessed until 1:30 p.m.)

AFTERNOON SESSION

CHAIRMAN DOWDY: I think we had better come to order and proceed with the business of the afternoon.

I believe you each one have a copy of the report of the Committee on Human Experimentation. I think we will have Dr. Stone re-read that, and you can follow it along and we can dispose of this particular piece of business.

DR. STONE: It has been slightly reworded since I read it this morning, and you all have a copy in your hands.

"The use of human beings as experimental subjects has been countenanced in the past when information was needed that could not be obtained in any other way.

"The NEPA Medical Advisory Committee on the Radiation Tolerance of Military Personnel is attempting to establish what will happen to humans when necessarily exposed at infrequent times to amounts of radiation that are high relative to the dose set up as the maximum permissible daily (or weekly) dose for workers constantly working where radiation exposure is possible."

We have left out the sentence about relation to lethal doses.

"A review of animal experiments has shown that not only do animals of various species differ in their response to given amounts of radiation, but also that animals of different strains within a given species differ.

"It is therefore impossible to predict with sufficient accuracy what will happen to humans.

"The exposure of some sick humans to radiation to the total body with therapy as the objective has provided some indication of how sick people respond; but such therapeutic trials have shown also that the response varies greatly with the clinical condition of the patient."

The next one is slightly altered again.

"A few accidental exposures, and the mass exposures at Hiroshima and Nagasaki, have provided some information as to how relatively healthy people respond; but the conditions of those exposures are not sufficiently well known."

We put brackets around the sentence causing some question. I don't see much difference whether it is in or not, when we come to the information at the end.

"(The information desired is sufficiently important for the safety of the U.S.A. that we believe the use of humans is justified.) ?????"

I think if you take the last paragraph where we recommen the use of humans, you can assume that we must think it is of sufficient importance to do it.

"It is essential that any such experimentation be carried on in accordance with the principles laid down by the Judicial Council of the A.M.A. in 1946 as follows:

1. The voluntary consent of the person on whom the experiment is to be performed must be obtained.

2. The danger of each experiment must have been previously investigated by animal experimentation.

3. The experiment must be performed under proper medical protection and management.

"In view of the above we, the members of NEPA Medical Advisory Committee on the Radiation Tolerance of Military Personnel, hereby recommend that human experimentation be carried out, and advise NEPA to request the Armed Services to arrange for it; and further recommend that the experiments be unclassified."

You will notice a little change at the end there. We recommend that they be carried out and advise NEPA to request the Armed Services to arrange for it; and further recommend that the experiments be unclassified.

If we might withdraw the former motion which was on the floor, I move that this report, with the sentence at the top of Page 2, that is, the one in brackets, be taken as the report of the whole Advisory Committee.

DR. ROBLEY EVANS: If you omit that, you don't get any connection with the bottom of Page 1 and the next sentence on Page 2.

DR. STAFFORD WARREN: If you change that next sentence to say that it is essential that any experimentation on humans be carried on, that would make the connection, wouldn't it?

DR. ROBLEY EVANS: At the top of Page 2 you could leave out "for the safety of the U.S.A." Wouldn't that be all right? "The information desired is sufficiently important that we believe the use of humans is justified."

DR. FRIEDELL: There is another point there, too. Down at the bottom we recommend that it be unclassified. If it is really that important for the safety of the United States, somebody might argue that the publishing of such critical data ought to be classified.

Therefore, I think I would leave out either the phrase "for the safety of the United States" or leave the whole sentence out.

DR. STAFFORD WARREN: "Of humans." If you put that in there, leave out —

DR. STONE: May we get an expression of opinions just informally about the inclusion "for the safety of the U.S.A."?

DR. FRIEDELL: I would move that it be omitted.

DR. SHIELDS WARREN: I second it.

DR. STONE: Can we leave it out as a motion and just alter this according to the opinion, and then see if we can get a unanimous action on it, as altered?

Scratch out the brackets and scratch out "the safety of the U.S.A." Then the top of Page 2 will read:

> "The information desired is sufficiently important that we believe the use of humans is justified."

Scratch out the brackets and the question marks.

CHAIRMAN DOWDY: Is that satisfactory?

(General response in the affirmative.)

CHAIRMAN DOWDY: The other motion, Dr. Stone, the one before lunch, was not seconded. So we can go ahead now with the motion. Would you restate it?

DR. STONE: I move that this report of the Committee on Human Experimentation be accepted as the report and opinion of the entire Advisory Committee.

CHAIRMAN DOWDY: Is there a second to that motion?

DR. ROBLEY EVANS: I second it.

CHAIRMAN DOWDY: Is there any discussion?

(There was no response.)

CHAIRMAN DOWDY: If not, those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Those opposed?

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

Are we ready to move on to our regular afternoon agenda?

DR. STONE: Dr. Evans brings out the point that we left out of here the definition of the word "voluntary." There is a whole discussion in these references down here about what constitutes voluntary, and so on.

I thought we were leaving it out of this to get this as brief and concise as we could, and the references will give you the discussions on the subject, if any of you want to look them up.

CHAIRMAN DOWDY: I think that would be sufficient. You each one have suggested research problems, which are on a mimeographed sheet. If you don't have one, I have a few extra ones I can pass around.

There have been some others come in to me locally, and also Dr. Failla handed me his this morning. I have undertaken to fit these into the outline that I have.

On Page 1 of that there is a correction to be made. In the first statement, "Methods for Detecting the Degree of Sensitivity of Individuals to Radioactive Exposures," knock out the bracket there after Dowdy, and insert Failla's name.

DR. FRIEDELL: What is that?

CHAIRMAN DOWDY: "Suggested Research Problems." It is the copy I mailed to you and asked you to bring to the meeting.

DR. SELLE: We have one more copy left. Does anyone desire that?

CHAIRMAN DOWDY: Under Item II, put Failla's name again.

DR. FAILLA: Do we have to do that? Most of these things are obvious to everybody here. Why bother with the names?

CHAIRMAN DOWDY: I just thought it would indicate who sent them in, and I tried to interpose those on the specific sheet.

DR. FAILLA: As far as my name is concerned, we can save time by not going through this thing.

CHAIRMAN DOWDY: You won't feel slighted, then?

DR. FAILLA: No, let it go.

CHAIRMAN DOWDY: Before we take these problems up, I would like to call on Mr. Simmons to start off this part of the program. Mr. Simmons.

MR. SIMMONS: I just have a few remarks here I would like to bring before this group that have not been mentioned yet.

This report which we are considering today is the answer to the first objective that was set at the Chicago meeting, which was the compilation of approximate exposure ranges versus anticipated damage. This was to be based on the best available data we had at this time.

I think the report we have got meets that requirement, and we have discharged that first objective.

The second big problem that was going to be considered by the Committee is recommendation of additional research, which is required to refine the numbers that we have estimated in the first report. As we get further into the shielding research, certain problems appear to stand out in importance as we are confronted with the problem of predicting the performance requirement for actual shields in airplanes.

We now have what appears to be a reasonable first approximation of exposure standards for the time being, but these values that we have are expressed in equivalent of 200 to 1000 KVP X-rays. When we tried to correlate the physical measurements of shield performance with the biological effectiveness of the shields, we ran into a problem; and a particular example of this problem occurs when we are considering the high energy neutrons that occur in the tail of the fission spectrum.

These neutrons, let's say around the 10 million volt region, may comprise only about 10^{-8} fraction of the whole spectrum; but in our case it becomes very important for us to know, let's say, the number of rems that are carried by this energy group of neutrons, because they have very long mean-free paths. They are very difficult to stop, and the amount of attenuation that must be provided for these neutrons is a very important item in the overall shield weight.

There is also a paradoxical probability that if we do too much degradation of energy on these high energy neutrons, there is some possibility that we might increase their biological effectiveness.

In discussing that further, I have some curves here which I put on the board to show you why this might be a possibility. I don't prove this fact, but there are some indications that this might be true.

If we are considering a monochromatic beam of neutrons, it might be reasonable to say that the biological effectiveness of the beams increases with energy up to a certain point; and that maximum point might be where the mean-free path of the neutron in the body becomes so great that the utilization factor of the body is so small that not many of these neutrons actually release their energy in the body.

(At the board) If we plot the utilization, which we can call the neutrons absorbed, the fraction of neutrons absorbed to the fraction of neutrons at energy E, and we base this just on the mean-free path of the neutron, in tissue for example, we generate a function which looks something like that, this being the energy of the neutron.

The mean-free path in this case has just been considered as proportional to the square root of the energy of the neutron. For instance, we would consider a body as about 25 centimeters thick. It isn't solid tissue, but just using that number, we get for this fraction that would undergo at least one collision in the body, it comes out $1-e^{25/\lambda}$ where lambda is the mean-free path of the neutron.

This number, of course, is quite arbitrary and will have to be set by a physiologist to tell us the equivalent of target thickness in tissue of the human body; but I think that kind of information could be furnished by people qualified to do it.

The lambda is really proportional to the square root of the energy in the range of energies we are thinking about. This relation is not true down in thermal energies.

So we can develop what we might call a utilization factor for neutrons versus the energy of the neutron. If we then want to convert this into the energy utilization rather than the fraction of neutrons utilized, we then have to multiply this function by each energy ordinate. If we do that, we arrive at a function which does something like that, this being the energy absorbed to the energy available in the radiation field.

The next thing we have to consider is the product of this curve with the neutron spectrum — this first curve is for, let's say, a monochromatic beam of neutrons assigned the following energies. Actually I think if we just make some rough checks, this peak may fall far outside of the highest energy group of neutrons in the fission spectrum, when you consider it on the basis that we are considering first collisions only.

But if we apply some of the analytical tools to carry these high energy neutrons on down through their attentuation in the body, we may find that this will bring the peak back into the fission spectrum and we get a more accurate analysis of the question.

So the next consideration is to apply this energy utilization curve to some neutron spectrum, which would in practice be the actual spectrum of neutrons that leak through the shield.

As an example, we just take a fission spectrum — that is, say the fraction of neutrons or energy E to the total number of neutrons — and plot it. I won't draw it as a fission spectrum is classified, but it is some function of that general shape.

We multiply these two curves together. That gives us the energy utilization by a 25 centimeter tissue structure with respect to this particular energy distribution.

When we do that, we get a function which is energy absorbed per fission neutron, and we then come out with a curve of this nature.

The proposal is made that we can at least consider as a first approximation of biological damage that the hazard or that the risk of an individual is not subject just to the intensity of the radiation field, but is directly proportional to the utilization of that radiation in a body.

Under those conditions, the biological damage criteria, or let's say the biological damage risk, would be related to the area under this curve.

I have drawn some curves where I have used some actual valuation. I don't want to put them on the board because that may throw us into security.

I would like to propose that this group consider the possibility of applying some of the analytical techniques that are now being used for shielding calculations, namely, the Monte Carlo or random process method of actually analyzing the behavior of neutrons in an organic body such as a man.

In doing that, I would like to point out that this curve and this curve are really biological in nature. Certain basic decisions will have to be made by the physiologists, the M.D.'s, the biologists, regarding the composition of the body, an equivalent target geometry, which would be typical of a body, which could actually be put into the calculations.

Once that is done, some work can be carried out experimentally by actually measuring mean-free paths of neutrons in a body. I think this could be done without hurting the body.

When we have data sufficient to arrive at reasonable values for these functions, these two are physical functions which could be carried out by theoretical physicists or mathematicians.

I would like to say we do have now analytical techniques that are sufficiently powerful that if we feed the right probabilities for the various nuclear processes involved in this system into the calculations, the accuracy of the method is as accurate as the basic data from which we start.

We can do it without making assumptions and integrating complex functions and things like that by this random process technique of analysis.

I think that is about all I wanted to say, except that it would appear to me that this method would offer certain advantages in trying to correlate the number of rem equivalent of high energy neutrons or high energy gammas with the equivalent X-ray standard that we are working in terms of today.

It boils down to the fact that you are not considering the energy potential of the radiation field, but you are considering the utilization potential of the body as a criteria rather than the incident radiation on the body.

CHAIRMAN DOWDY: Have you tried any of that on animals to see if your predictions would work?

MR. SIMMONS: No, sir. This is absolutely analytical in foundation. It is foreseeable that this type of analysis will lend itself to the calculation of actual energy absorbed from any given radiation spectrum in a body or in a shield.

For instance, we are doing work of this nature on complex geometrics of tungsten, boron, carbon, and hydrogen; and can tell with reasonable accuracy the expected energy released from each kind of radiation in this system.

This is a proposal that, instead of having an inorganic shield, that we run into the same calculations, the equivalent geometry for biological mechanism. Now, that equivalent geometry would have to be arrived at by perhaps physiologists and biologists to tell us, for example, a man's body is equivalent to, let's say, 10 centimeters of water, 2 centimeters of carbon, and give us something that would be equivalent to the elemental composition of the body that could be used as a target for these analysis studies.

Or another way to check this would be to actually make some measurements of the mean-free path of various energy group neutrons in the body as a unit.

DR. FRIEDELL: I would like to know what those units are, again.

MR. SIMMONS: This is the fraction of neutrons actually undergoing collisions in the body, divided by the number of neutrons energy E, the number of neutrons available per collision at a specific energy; and these are the ones that are actually utilized. That is based on this equation 1-e to the X over lambda.

This one is the energy absorbed divided by the total energy available at energy E, which is merely a multiplication. In other words, if we have one 10 million volt neutron absorbed, that is 10 million volts; so we multiply the number of neutrons absorbed or the fraction absorbed by the energy, so it is equivalent to multiplying this function by a 45 degree line drawn on this curve, which then gives us this function.

Now, in order to get some definitive answers, I think all we have to do is to establish one of the accurate probability curves for the nuclear processes involved in the body, which include proton recoil, any other processes that might be pertinent to the physiological damage; and to set up an equivalent target geometry for a body and use that instead of the geometry we are using now in other shields, because the problem is identical.

CHAIRMAN DONDY: What you are doing is just substituting organic material of the body for your shield in this calculation.

MR. SIMMONS: Yes, and using the same calculations that we did in the shield on the body.

DR. FAILLA: There is one thing you didn't take into account, and that is the distribution in the body. This will not give you that, unless you did the work for several different thicknesses. That is important in what you finally get in the individual.

MR. SIMMONS: There may be subtle effects. This is not trying to define the subtle effects that for instance might be associated with density of ion path, a proton versus an alpha particle.

DR. FAILLA: I am talking about the fact that if the radiation comes from that direction, this part of the body (Indicating rear part) would get not as much as this part of the body (Indicating front portion of body).

MR. SIMMONS: True, but that depends upon the energy of the radiation. In other words, if it were high enough energy, the back part away from the source might get more than the front.

DR. FAILLA: That is correct.

MR. SIMMONS: But that would be handled in this kind of a calculation. Remember, what I put on here was just a single one collision analogy.

Now, if you start carrying it on down to second and third collisions and keeping track of where each one of those collisions occur in the media — we are using I.B.M. equipment in which each card represents a neutron; and when we get through and the cards are all sorted out, we know how many hundred volt neutrons collided in this region, how many thousand volt neutrons collided in this region, and we have a much more complete description of the process from the calculating machine than I can put on the board.

DR. FAILLA: The point I want to make is, you have to make that determination before you can estimate what the biological effect is going to be.

MR. SIMMONS: That is right.

DR. FAILLA: If you get the same dose from one direction or two directions, you don't get the same biological effect.

MR. SIMMONS: Yes, that is right. One premise in here that may or may not be acceptable is that the one criteria of damage to a biological organism is the actual quantity of energy that that mechanism absorbs.

Now, there may be subtle effects beyond that; but you say if a man absorbs one kilowatt of energy in whole body radiation and another man actually dissipates in his body 10 kilowatts of energy, that this fellow has got ten times the chance of being hurt that this one has, not saying what the injury is, but at least the danger to him should be some factor above the danger from this exposure.

DR. NIMS: I think another way of saying it, both biologically and physically, the body is anisotropic.

MR. SIMMONS: Correct. That is one point I brought out why the target geometry will have to be set by biologists, physiologists, and M.D.'s. This is something a physicist cannot hope to set up accurately.

The basis for, let's say, in this expression, the value of X, which is the target thickness — I mean, we might have several; X-1, X-2, X-3; we might have a whole family of these expressions. This number will

have to be set up by a group of people such as this in working closely with the theoretical physicist who will actually form the calculations once the basic assumptions are standardized.

I think that is about all I have. If there is any more comment —

CHAIRMAN DOWDY: I might say you have gone quite a bit over my head here. It would take a physicist to understand this.

As I understand it, you hope to find the probability, by adjusting your shield at a certain thickness, you might be able to arrive at a certain energy of neutrons which might be less available, even though they pass through the body, than another group at a lower energy.

MR. SIMMONS: We have some calculations here assuming the mean-free path. Let's take a number here, a mean-free path of a 10 million volt neutron in tissue, at approximately 120 centimeters.

If you say a body, a man's torso or his chest or whatever part, is equivalent to 25 centimeters of tissue, you come out with the value of this function at E equal 10 mev of, I think, approximately 20 percent.

In other words, 20 percent of a 10 million volt group would be utilized by a 25 centimeter body of tissue. Then if you multiply this by the 10 million volts, you see you would have 2 million volts of energy per neutron dissipated in the tissue.

At 2 million volts this mean-free path comes down to about 23 centimeters. This is the assumption we made here just as an illustration.

DR. ROBLEY EVANS: In this calculation you are using one over E?

MR. SIMMONS: In this energy range it is reasonable to use it. Down at lower energies, it would break down. You would have to use the actual cross sections.

DR. ROBLEY EVANS: The principle is all right, though.

MR. SIMMONS: So then, under that condition of 2 million volts, you would have something like 40 percent of the energy available utilized, where your utilization factor would be 40 percent of 2 million volts and 20 percent at 10 million volts.

So, if you had a certain amount of incident energy on the body, let's say you had 100 kilowatts of incident energy, if they were all 10 million volt neutrons, you would actually dissipate less in the body than if they were all 2 million volt neutrons, assuming the same total energy in both fields.

DR. STONE: I would like to bring up a point some physicist here might be able to help me understand.

When we were using so-called 16 mev neutrons to treat patients, we measured the depth of those by ionization measurements and by potentiometers. It decreased way down to about 40 percent of what it is at the surface.

Now, if it has fallen down to 40 percent of that and drops off quite rapidly beyond that, how can we have only 20 percent of them being utilized in there? If it is 120 centimeter mean-free path, why didn't we get more than going right on through the body?

Of course, we don't know how many went through the body because we didn't measure what came out the other side.

MR. SIMMONS: Did you know the total energy available in the neutron beam that you had?

DR. STONE: No. We knew the maximum energy.

MR. SIMMONS: In other words, a fraction of the available neutrons at a given energy to the ones actually utilized.

So, what we are measuring is the exposure in terms of utilization factor from the total energy available in the radiation field.

DR. FAILLA: Dr. Stone had a measurement on the surface and a measurement at a depth of 10 centimeters. So, if you assume the effect on the ionization doesn't change much with energy, it changes in a certain way with respect to energy — then his figures are comparable to what —

MR. SIMMONS: Can you assume that, though, that the ionization chamber does not vary with energy? I think that the ionization chamber would suffer from the same problems as the body.

DR. FAILLA: It does vary, but it varies essentially in the same way as the neutron beam.

MR. WARD: From that beam?

DR. FAILLA: That is the point. What you actually use has a very few of the very high energy neutrons. It is way below the 10 million neutrons.

DR. STONE: You would have to work out a whole composition of these curves and fit them all together according to the composition.

DR. FRIEDELL: There is also a correction for the inverse square law. He had a finite distance, a short distance.

DR. FAILLA: Not so bad, however.

CHAIRMAN DOWDY: I don't want to spend too much on this. It is quite interesting; and as I say, 99-99/100 of it is over my head.

Do I understand you would like to enter this type of analysis as a proposal to be considered?

MR. SIMMONS: I want to point out the importance to the project of having a basis of correlation better than the existing basis. In other words, we can now know much more about our shield behavior than we can know about the behavior of the target, which is a human body.

This was a suggestion, at least it was a preliminary method, by which we might be able to utilize some knowledge that had been developed in another field, let's say, removed from the field of biology and medicine.

With the cooperation of the people who are competent to set up the basis for these reactions, and by combining those two fields of knowledge, we might be able to attack the problem more effectively in getting better conversion factors for neutrons.

CHAIRMAN DOWDY: I think all agree that we need better conversion factors, and I think this re-emphasizes the need for the close cooperation of the medical group in knowing what your problems are and how you are approaching them.

MR. WARD: May I ask a question which in a sense tends to clarify a point here in my own mind, at least. I think what this approach brings out is that to merely talk of a neutron beam in terms of total energy, you are going to get discordant results unless the composition of all those beams are identical with respect to the amounts of neutrons at given energy values.

I think this is pointing that up, that unless we make some attempt to find out what the neutron equivalent would be for a given energy value of neutrons, the results will be very hard to correlate and the factors will not be exact.

Is that correct?

MR. SIMMONS: Yes, I think that is a good summary, Mr. Ward. I think we could go a little more in detail, but I don't think it is really worthwhile at this time.

MR. WARD: What I meant to point out was, its importance to the shield designers now, and may save a large weight of shielding mass in an airplane if they know the correlation, the biological correlation, of the different energy values of neutrons. They have so far been submerged.

DR. FAILLA: The point I tried to make was that it was true, provided that the distribution in the body is similar to what it is in the case of ordinary X-rays for which we have this integration.

MR. WARD: That is right.

DR. FAILLA: So that sets a limit at the lower energy end as the neutrons, because the lower energy neutrons will be absorbed only in the same distribution for which we have information available today.

MR. WARD: If I understood his question, he started off — correct me — with some very high energy value neutrons are a difficult thing to shield against. If that energy value was not important, it would simplify his shielding problem.

CHAIRMAN DOWDY: We have not worried about the low values.

MR. WARD: That is right. He was talking about some very difficult high energy neutrons about which apparently he has no biological —

DR. FAILLA: That may bring us to the other extreme, for which we also have no information; and that is where the distribution in the body is perfectly uniform. The method is all right; I have no objection to it.

MR. SIMMONS: This is one method by which you can determine that distribution, theoretically, if you know the basic probability of each nuclear process which you can measure.

DR. FAILLA: I think it can be done, absolutely.

MR. SIMMONS: Then you can do this for both neutrons and high energy gammas. Both are amenable to the same analytical treatment. You can do it with the standard X-rays that you use and set the whole basis of standards on the utilization factor versus energy, or for the standard and for any kind of radiation that you want to consider.

CHAIRMAN DOWDY: If I understand it correctly, it seems to me like it would certainly be a worthwhile thing to pursue, and we as biologists probably would get a good deal of use for information out of it.

MR. SIMMONS: It is impossible to do without the collaboration of a group of highly competent specialists in all of the fields represented here, plus the addition of some very highly qualified theoretical physicists to actually carry out the computing machine calculations, after the basis for the calculations have been established, on something consistent with the geometry of the body and the elements contained in the body.

CHAIRMAN DOWDY: Could I have a motion to the effect that we can accept this approach for recommendations on research.

DR. STONE: I so move.

DR. FAILLA: I will second it.

CHAIRMAN DOWDY: Is there any discussion?

(There was no response.)

CHAIRMAN DOWDY: All those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

Now then, proceeding to the summaries here, if any of you have any ideas on this before we start; I had thought that these logically fell into six different categories. I enumerated these categories on a separate sheet with some little notes of approach under each heading.

Again, as with Dr. Stone's proposal, I don't think it is our function here today to go into details, but merely accept or reject these various approaches; and at a later time, depending upon NEPA's desires and requests, we will definitely set our own proposals and various protocols.

So, if we could start with the first page, which is methods for detecting the degree of sensitivity of individuals to radiation exposures, there are a number of different approaches that could be made on this. One is the mitotic suppression in human epidermis; erythema, the degree of erythema produced by graded exposures near the standard erythema values; and a correlation of the over-all response of patients undergoing irradiation treatment with their erythema response as determined under the heading Erythema.

Also, the histogenetic effects upon bone marrow and their correlation with the peripheral blood picture.

One which Dr. Failla suggested is nausea, comparative relationship of the radiation nausea as against standard emetic drugs; in other words, get the threshold, see if there is a correlation from individual to individual between the threshold for emetic drugs and radiation nausea.

DR. FAILLA: Mr. Chairman, may I say a word in connection with this? It seems to me the first thing to decide in connection with this problem is what effects are we interested in, because the susceptibility may not be the same; I mean, individuals' susceptibility may not be the same for all effects.

In other words, what effects are we interested in in this situation as far as NEPA is concerned?

CHAIRMAN DOWDY: Under I here, if you can find some way of pre-selecting pilots —

DR. FAILLA: With respect to what?

CHAIRMAN DOWDY: To their over-all damage.

DR. FAILLA: First of all, their incapacitation.

CHAIRMAN DOWDY: Immediate nausea, immediate incapacitation, possible short-term effects. I doubt if you could set up one like this to take in long-time effects.

DR. FRIEDELL: I think that must be the crux of the whole problem. This studying mitotic suppression in human epidermis or erythema production must be correlated with what kind of effects you are concerned with.

CHAIRMAN DOWDY: Correlate the erythema with their degrees of nausea or the dosage which brings on nausea.

DR. FRIEDELL: That is right. Therefore, you have to decide what you are going to correlate with.

CHAIRMAN DOWDY: Certainly nausea would be one thing.

DR. TITUS EVANS: I have some suggestions of other things that we might correlate with, irritability and maintenance of equilibrium. Put them on a disk and see how long they can take it — animals — and still walk straight; and then take the amount of food they take after 50 hours or something like that; eating tests, something like that; mating tests — I don't mean mating tests as a test of sterility, but I mean mating tests —

CHAIRMAN DOWDY: For desirability?

DR. TITUS EVANS: Something like that.

DR. FAILLA: The first thing we have to decide is what we are interested in.

DR. NEWELL: This is an engineering project. This isn't just scientific investigation. This is an engineering project. Get your feet on the ground, what are you interested in.

DR. FAILLA: What other things are there? Let's list the things that we are interested in in this connection. I said nausea is one of them, certainly.

CHAIRMAN DOWDY: Ability to perform tasks is another.

DR. ANDERSON: Aren't these really a part of Sections III and V, where certain criteria are believed to be important? This particular thing, it seems to me, should be done in correlation with the criteria that are considered to be most important in the list farther down.

DR. FAILLA: You have got this started with the criteria. The first thing for us to decide is, what criteria are we going to take?

DR. ANDERSON: They are listed under III and V here, as I understand it.

CHAIRMAN DOWDY: The reason I left them this way, this seemed to be something you could do on humans without too much trouble. The ones under III are most apropos, it seems to me, to animal experimentation, of which we have no information on animals or at least very little in that category.

DR. SHIELDS WARREN: One practicable and very easily measurable reaction is the development of diorrhea. I think that will be about as incapacitating as nausea and vomiting will be.

DR. FAILLA: Does that happen within twenty-four hours?

DR. SHIELDS WARREN: In some of the Japanese it apparently turned up very early. It is awfully hard to differentiate there the psychic effect of excitement and your actual direct effect on the g.i. tract.

CHAIRMAN DOWDY: Of course, the nausea and incapacitation from nausea and vomiting certainly come much earlier than any diorrhea. I have seen patients completely knocked out from nausea after one or two treatments without any diorrhea at all.

I think in humans you would have to get up to a higher dosage. You could exclude the psychic factor.

Dr. Stone.

DR. STONE: I was thinking of two things here. If this is a means of picking sensitivity of individuals, you certainly are not going to expose any of these individuals to radiation, their sensitivity. It is like a fellow eating a watermelon to see if he can eat a whole watermelon.

If you have already exposed him, you can't expose him again. So, what we have got to look for, which isn't here, are some other tests which I haven't any to suggest, that might be correlated in a given individual with their sensitivity to radiation; and pick your pilots by some other —

DR. NEWELL: You might irradiate a dime-sized area of the skin to find out how sensitive he is.

DR. FAILLA: As far as the skin is concerned, yes; but you have got to find out if that correlates with anything else.

DR. NEWELL: That is what the project is, to find out if sensitivity as shown in the epithelium correlates with the sensitivity that you were interested in for the gross exposure.

DR. FAILLA: How are you going to find out the other sensitivity?

CHAIRMAN DOWDY: You can very easily find out if erythema correlates —

DR. NEWELL: Suppose we do what we were planning to do in the last motion, use human subjects for total body exposure. If you decide that this is a worthwhile project, you will have a chance to irradiate the skin before you irradiate the body and find out if there is a correlation in sensitivity.

DR. FAILLA: That is very true.

DR. NEWELL: Isn't that what is intended here?

DR. FAILLA: You can do some of those things on patients without having to get at least a lead.

DR. NEWELL: You can do it first in animals.

DR. FAILLA: For instance, you could correlate skin erythema with nausea in the treatment of patients, see if they correlate.

CHAIRMAN DOWDY: You also can correlate with bone marrow on biopsies.

DR. FAILLA: Also see whether nausea obtained by radiation correlates with carsickness or the reaction to an emetic.

DR. NEWELL: What I am impressed by in this proposition, is we are seeking a correlation. The statistical study would probably be something in the nature of a product of a movement correlation collision. Regression equations do not have a high efficiency, unless you have a large standard deviation for the things which are to be correlated.

I think we know something about the standard deviation of these several things, and they do not have a very large standard deviation; so that I do not think we would get a useful efficiency of a regression equation if we should develop one. I don't think it is a promising project.

CHAIRMAN DOWDY: Do we know that? You made a statement there that you don't think there is any very widespread —

DR. NEWELL: These things don't have an enormous standard deviation, do they? Sensitivity of epidermis, erythema? You have done it in your shop.

CHAIRMAN DOWDY: It is fairly widespread for erythema in the individual from area to area, on the same patient.

DR. NEWELL: Of course, on the same area. You have to use the same area. You are going to talk about the same area here. You are not going to do different areas on different people and try to correlate those with total body reaction. You are going to use some standard area, are you not?

CHAIRMAN DOWDY: Yes.

DR. NEWELL: My experience in one standard area is that the scatter among erythema is not great.

CHAIRMAN DOWDY: My impression was that it was pretty great. In fact, I have even seen epilation —

DR. FAILLA: Not more than a factor of 2.

DR. FRIEDELL: I don't think it is even a factor of 2.

DR. TITUS EVANS: A fair-haired, fair-skinned person might give you a bright red one, with the limited experience I have had. The dark-skinned people have a different type of erythema; it goes a different way. And some have hardly any erythema at all. They just get pigment. Don't you find a variation in the area?

DR. STONE: Oh, yes, it is a variation according to the skin type. If you could get some method of detecting the redness other than that — we had, what do you call them? a leukodermic individual. A leukodermic individual is one that has patches of skin without any pigment whatever in it.

There the erythema came up and persisted for very, very long periods of time; and in areas right next to the leukodermic areas where there wasn't any, where you got pigment, the erythema disappeared to your vision within a short period of time because the pigment got so great you couldn't see it.

The erythema persisted for a long, long while in the leukodermic portions of the skin.

I am surprised to hear practicing radiologists here think that they can get any correlation between anything in nausea, because it is such a big psychic factor that there is nothing you can correlate with it; and vomiting is the same. I don't know about diorrhea. I have never carried anybody to that extent except with terrific doses.

So I don't think there is anything you can correlate but the three things you want to get at here, I think vomiting, diorrhea, and fatiguability.

CHAIRMAN DOWDY: Let us restate this another way. This is developing just the way I was afraid it might.

Is it a consensus that we need methods for detecting the degree of sensitivity of individuals to radiation exposure?

DR. FAILLA: Yes.

CHAIRMAN DOWDY: That is all I want to settle here. The thing that you correlate and how you do it is something not to be decided at a meeting like this. It is something that somebody sits down and works out a good protocol with the advice of his confreres, and then presents to a committee to decide whether that is or is not. These are merely examples.

One of the things which I emphasized in Chicago, and I emphasized, I think, every place, is that there is a psychic factor here which NEPA is going to have to take into consideration, and somebody is going to have to set up something to rule out psychic people.

You may be able to get it, for instance, by saying to a patient, "I am going to give you a drug that will cause you to completely empty your stomach in five minutes," and give him saline. If he does, then you had better not use that patient as a pilot for this purpose.

Those are things, when you sit down really to get at this thing, on which you can get some correlation, I believe.

DR. NEWELL: You didn't put that down on this list.

CHAIRMAN DOWDY: As I said before, our purpose is not to go into detail of research problems.

DR. NEWELL: Our purpose is to seek out promising projects —

CHAIRMAN DOWDY: Not at this time, no.

DR. NEWELL: What is this except suggested research problems? Why are we presented with these if it isn't to use our best judgment and pick out which research problems are promising for the purposes of this Committee?

CHAIRMAN DOWDY: These were merely ideas that were submitted, but not to weigh each one of these ideas as to their individual value.

Our problem as I see it — maybe I am wrong — is to pick out major problems that need further exploration; and then if we can decide on those, then individual problems can be worked out by the people who —

DR. NEWELL: You and I are saying the same thing — warrant further exploration. I say, in my estimate this project doesn't warrant further exploration.

CHAIRMAN DOWDY: We had almost a unanimous agreement awhile ago on statement No. I. It is desirable to have methods for detecting the degree of sensitivity of individuals to radiation exposure.

DR. NEWELL: I didn't say it wasn't desirable. I said it wasn't promising.

CHAIRMAN DOWDY: As they are listed here.

DR. FAILLA: Let us agree on what is desirable, then.

DR. NEWELL: What is desirable? Surely, I know what is desirable — the obliteration of the atom bomb.

CHAIRMAN DOWDY: We can't decide that.

DR. NEWELL: I am trying to make it obvious that you can't have what is desirable. Your desirables out-run your possibilities. The function of this Committee is to help choose the researches which might yield something.

Now, there are two ways you can do. You can put down everything you can think of and then set a team to work on everything you think of. That was Edison's idea, and it worked. It is very expensive, but I am sure that we are so inventive that we couldn't possibly find — we couldn't get enough contracts to cover all the projects that we could invent.

It seems to me the thing to do is to pick out the things that have some hope of giving you what you need.

DR. FAILLA: Let us agree on the desirable things, then.

DR. NEWELL: Desirability must be linked with possibility.

DR. FAILLA: Not until you come to actually setting up some projects.

CHAIRMAN DOWDY: I just mentioned one where you can get some idea on a patient's nausea factor correlated with psychic reaction.

Now, that isn't listed here.

DR. NEWELL: I think that might be promising.

CHAIRMAN DOWDY: I think if we can accept in principle that we would like to have somebody to present protocols attempting to give us a method, then we could decide on those protocols.

I wouldn't even agree that all of these that we have listed here are beyond the possibilities of giving some information. That is just a personal opinion.

But then, if we decide it here that these are fundamental problems that should be investigated, then we can decide whether somebody at a later time has presented a protocol which is reasonable of giving us any information.

DR. NEWELL: All right. Thank you.

CHAIRMAN DOWDY: That is my personal impression. So I think we can move on.

DR. STONE: With regard to this first one, methods of detecting degree of sensitivity of individuals to radioactive exposure, I think we come back to what sensitivity are you talking about. If it is just in general, well, of course, it is a nice thing to know; but I don't know that it is connected to NEPA.

The method of detecting degree of sensitivity of individuals to nausea, to fatigue, yes; and what you are going to correlate that with, I don't know. I am thinking largely in terms of the amount of exposure we are thinking of; or I am thinking of here, let's say, 100 r maximum, 25 r probable.

What we want to know is, what is going to happen to people that get 25 r and can we pick out of a crowd some that might be hypersensitive to 25 r? Let's say the figure we have set to shoot at is 25 r, and to find out whether you can pick out whether a person is sensitive to 600 r or not and will show reactions before they did, would be interesting to know, but probably of no practical significance.

DR. FAILLA: I think another problem is this. If you can in the beginning pick the people that are most resistant to radiation in respect to the effects that we decide, in the case of an accident or in case a man has to be over-exposed, he has a much better chance of getting out than if he didn't have that resistance.

So it is important to develop, if possible, means of weeding out those who would be incapacitated if they should be exposed to a larger dose than was planned for that mission.

CHAIRMAN DOWDY: We don't know but what you may have to go up to 100 or 150 r.

DR. FAILLA: You may have to go to 500 r. It depends on what happens in that mission.

CHAIRMAN DOWDY: But I do know that you can expose the same type of individual having the same type of a carcinoma, and one of them will develop a very profound leukopenia and the other one won't. I mean that is pretty well known.

DR. NEWELL: It just happens that I have been through a project like this a few years ago, the prediction of heart size; and the proposition was to predict the normal size of the heart and the measurement of the patient.

We found, and everybody else found, that the efficiency of prediction was about 30 percent; and that you weren't more than 30 percent better off to take your predicted size of the heart and compare the patient with that than you were to take the average for all people and compare the patient with that.

The efficiency of prediction was so low it practically wasn't worth having. That is what I anticipate in these things, is that the spread between the sensitivity of one person and another for your test, which is erythema or mitosis or something of that sort, won't have sufficient spread from the most sensitive to the least sensitive so that your prediction will prove to be of any practical value.

DR. FAILLA: What you are saying is that you don't think it is worthwhile to do a lot of these things.

DR. NEWELL: That is what I mean, yes. If it is not a promising project.

DR. FAILLA: But in the first place we have to decide what we like to have.

DR. NEWELL: That is right.

DR. FAILLA: And then if it is possible to accomplish it.

DR. NEWELL: I think we would all agree that this would be desirable.

DR. FAILLA: I think we have to do that; and then attempt to find something. Maybe there is something worthwhile; I don't know. If it doesn't cost too much in the way of money and time, particularly in personnel, I think it ought to be tried.

DR. NEWELL: I think you have said it very well, if you can find something that is promising.

DR. FAILLA: That is right. Then I would do it. But I think some projects can be set up which will cost very little money.

For instance, the one I suggested about nausea can be done very easily in a hospital, if you can get some radiologist interested in it, because it is also a service to him. He can carry out this investigation at very little extra cost. I think it is worthwhile.

Under those conditions, I think it is worthwhile investigating, even though we don't think it is going to find anything that will mean anything.

DR. FRIEDELL: These statements are not hard to reconcile. I think the point Dr. Newell makes is a pretty good one; that is, that the spread we have observed from biological effect, and I don't care much what they are, from radiation doesn't have a very wide dispersion; and if they don't have a wide dispersion, the results are not likely to be very useful from the point of view of correlation of these things.

DR. ROBLEY EVANS: Just the opposite.

DR. FRIEDELL: It all depends on how critical you want to be. If you decide that you want to know to within one in a thousand, then I recognize that. But if you take some reasonable figure and correct for that —

DR. FAILLA: Ten percent who might be hypersensitive, I think you have a promising lot.

MR. WARD: Mr. Chairman, might I bring out a point, which obviously will be non-medical?

I think the Services will be very interested if, after a proper examination, you told them that there were no tests. I think they would

like an opinion. So I think the investigation of this matter will bring forth useful fruits, irrespective of the answer.

CHAIRMAN DOWDY: For instance, there is a wide spread in mortality. That is a very gross thing. There is a wide spread of the hematopoietic system of a different individual. We know that those are widespread, that there are things that are widespread.

Now then, if we can accept this, then the ones who are interested can sit down and conscientiously work out an approach to this, which can be considered.

So I think we will accept this, as a group. We haven't ruled out any. Right now the consensus of opinion is, these are worth some further effort.

DR. STONE: I think you might add to this list you have here, though, the problem of detecting a degree of sensitivity of individuals to radioactive exposures wherein the test does not involve any exposure to radiation, even to a piece of the skin, if you could get it.

CHAIRMAN DOWDY: There is one of those in the NEPA Project on testing animals. That can be done on animals; and if it correlates there, then we might carry it over on some of the others.

DR. HOLLAENDER: It might be that you would have to have correlated a whole bunch of these tests on these men, like men suited for the submarine duty or airplane duty. It is usually not a single test that tells the whole story.

Maybe you can find sensitivity which would tell you something with regard to radiation, too.

DR. NIMS: I think in all these things we are leading towards the selection of a test. What we gain from them is not with respect to individuals, but statistical ability. We know more about group performance. When you are living with 100,000 men, a 10 percent increase is sometimes worthwhile shooting for.

So there is something then to be said for the number of people you expect to be exposed and worthwhile developing any selection of tests.

DR. ELLINGER: I would like to say this. I am not quite as pessimistic about using the skin, because I have used over 2,000 human beings to study a similar project in ultraviolet. We were able to separate certain groups of healthy individuals, to a point that we could group them at three times the average of the men.

The interesting point is that what I found fifteen years ago correlating to ultraviolet erythema has been carried over to X-ray skin reactions, and very recently also to total mortality under similar conditions.

Referring to one example, thyroid has been found definitely to increase ultraviolet skin sensitivity, that is recognized to rays considerably; and X-rays reaction has also recently been established to raise the mortality percentage of a given dose and total body radiation.

Based on this experience, I think — and as a matter of fact, we have already some joint project at the Naval Hospital and Naval Medical Research under way — you can, by correlating skin to two or three other factors, find certain groups of individuals who are more inclined to show leukopenia, vomiting, and other effects.

I worked on about 3,000 human beings, as far as ultraviolet is concerned, and on a considerable number of experimental animals.

CHAIRMAN DOWDY: Thank you, Dr. Ellinger. We are delighted to have one more supporter here.

The second group was the study of pharmacological studies of radiation reactions. Mainly we are concerned with methods of reducing the percentage and amount of injury from a given dose of radiation; or once they had received radiation, see if we could stop a chain of reactions which is initiated by the radiation. Under this comes the proposal presented by NEPA last June; in fact, June 23.

Studies on those are being done, but I think wherever good proposals come in, they should be supported and recommendations for a continuation of these studies should be made.

I have no real proof or anything that is put down here as of protocol.

DR. NEWELL: There are a few people that have been working in this field.

CHAIRMAN DOWDY: There are a lot of people that have been working in this field. I know of a lot of people working in the field; and I think some interesting things have come out in the last year.

You mentioned, Dr. Ellinger, about thyroid, which is being worked on by several people. Dr. Evans did some, which I think points in that direction on temperature and limitation of blood supply. I think there are large possibilities in this field.

Dr. Ellinger has a published paper on desoxycortiosterone. I think this topic should be included in our recommendation for NEPA for further consideration.

DR. FAILLA: Mr. Chairman, I would like to point out again that we have to decide what deal NEPA is going to operate, first as to the effects that I witnessed; then as to the projects that should be undertaken by NEPA, because there is an awful lot of this work going on and everything that has some connection to the biological effects of radiation which is of interest to NEPA.

Now the question is some division of the field, so there will be no duplication; and what the Atomic Energy Commission should do and what NEPA should do. Those are things I think will have to be decided.

It seems to me that the investigation on fundamental problems does not come under NEPA. This is more of an engineering problem rather than a basic research program.

MR. WARD: May I remark on that, Mr. Chairman? NEPA doesn't desire to do any work that is being done elsewhere. That is one of its fundamental principles, and it has observed that very largely in all of the fields in which it is interested.

It has found, on the other hand, that because of the urgency attached to this project, and because of the peculiar limitations of the project — the two factors — that certain problems which are being left to a low order of priority because of the requirements of other projects become a high order of priority to NEPA.

If NEPA can get someone else to do it, it intends to do so. If it can't, then it will try to do it for itself. I think that is the philosophy.

DR. FAILLA: I agree with that completely, but it seems to me that this group here should more or less decide as soon as possible, so they won't waste an awful lot of time, just what are the things that NEPA wants, and how best to get the information.

If we go through a list of all the effects that we have been studying that would contribute something, so on and so forth, we are going to have a terrific program. Nothing will be done in a short time.

CHAIRMAN DOWDY: I don't think that is quite the way I understand it. NEPA expects us to inform them: one, of programs which as engineers we as biologists should tell them are important to their project, and they should watch out for. When we designate something where they get this information, whether they do it themselves or whether they find out it is being done some place else, is a problem which we are not concerned with at the moment. I don't know whether they have a budget to do this with or not. I don't know whether they expect to have a budget to do it or not, but that will be left up to them.

The other thing is, I think it is perfectly obvious from the meetings that we have had that it is important to them to have a close relationship with a biological group for a tie-in with their work.

If we concede that to be important, then they must have — this is my own opinion again — some sort of people on their project who are interested in biological programs in order to form a close liaison. If that is true, then they will of necessity, to get the type of men they want, probably participate in some program themselves.

As I understand it, after our deliberations today, then they will have something to go on to make their decisions and make their recommendations.

Am I right or am I wrong on that?

DR. FAILLA: We are all right.

MR. WARD: I think everyone is right.

MR. SIMMONS: This may be a little out of the normal procedure and off the subject, but I see the point. I think what we are groping for is an order of importance, practical importance to the NEPA objectives or programs. We could discuss that a little bit.

I think at Chicago we set the number one priority job, which was this. I think the number one priority job is information which will permit the application of this to practical problems that we are confronted with today.

That is, how many roentgens is represented by a thousand 10 mev neutrons which we are going to have to let escape through the shield, so that we can correlate the field results on the outside of our shield with at least the data presented in this document.

Right now we say, "Sure, if we give a man 200 r of X-rays, this does to the best of our knowledge the following things." We are not going to have any 200,000 volt X-rays in this airplane.

I think that is probably the number two priority, the questions we now have, the information to let us use this in practical design considerations.

DR. NEWELL: Could I say something that I think is pertinent to this decision, because you do have to make a decision as to what projects you will go after and what projects you will support.

I would like to draw a distinction between fundamental scientific research and what we could call testing or development, engineering projects. Now, there is no question that engineering projects can be given to a research laboratory oftentimes with magnificent results.

In regard to fundamental research, however, I would like to quote Mr. Jackley of about fifteen years ago. He said, in French, "If you let the scientist choose the problems he will work on, he might be wrong a quarter or a half of the time. If you let the director of the laboratory decide what problem he will work on, he will probably be right about 10 percent of the time. But if you let the board of directors decide, he will always be wrong."

That is in regard to the fundamental scientific projects. I think that this board of directors can direct only in regard to what we can call testing or engineering projects.

DR. FAILLA: Assign certain priorities to this; otherwise, there is so much to be done that nothing will be done.

DR. STAFFORD WARREN: It seems to me one of the most difficult problems we have got before us is the one that is handicapped by time, and that is the long-term effect of these doses that we have signed our names to in this report. That is probably a ten or fifteen year job, and that is a project job that nobody wants to take.

I think that is one that should have first priority in order to get it started, No. I. Those of us in Manhattan tried to start it at the beginning of the war and couldn't because there was a shortage of men and materials. We tried to start it immediately after the war, and it has hung just because nobody would come forth and say "This has got a must."

This project, it seems, has got a must in that direction. We are not going to be able to come up with information from that end very soon. That one ought to be started.

Then I think it divides itself down to four other categories which we can get answers on a little more quickly. Some of this should be done by NEPA and some is already being done by other agencies, chiefly A.E.C.

One very important one is the repair of injury after it occurs. That, I think, has got to be from other organizations. But these next two are the ones we have been haggling around, the identification of sensitivity and the possible reduction of sensitivity.

Those two are being worked on actively by A.E.C. and probably by several other agencies, too. But I think we ought to concentrate some effort in that direction, if for no other reason than to offer competition to other groups that are working in this field.

Another thing I would like to say while I have got the floor, and that is on this long-term effect. At least two large installations ought to take that one on and perhaps small parts done by others with small animals, because of the hazard that you are always faced with in the long-term program of having your colony wiped out by a current infection or some other accidental reason.

Since we would have a stake of ten or fifteen years of observations in just one experiment, it ought to be safeguarded by being duplicated. It is going to be difficult to find a group that will take this on.

I would like to ask Dr. Failla a question. What about the measurement of neutrons of current experimentation? Isn't that one of the difficulties that we face, too, in this kind of experimentation?

DR. FAILLA: Well, the measurement of neutrons in terms of energy absorbed per gram of tissue is at least a solution, and that is available today. Now, the determination of the percentage of the ionization produced by fast neutrons by gamma rays will have to be done. Also the correlation of biological effects when the energy is absorbed from neutrons of different energies has not been made.

So there is a big field there for further study. We have got to have in addition to that the composition of this tissue, what percentage is due to gamma rays; what percentage is due to neutrons and what energy. Those factors come into this biological effectiveness of that tissue in terms of grams.

So there is a big field there. That must be explored before we can predict what is going to happen to the human being exposed in the field of radiation such as exists here.

CHAIRMAN DOWDY: That comes under IV, "Conversion Factors, N to r."

DR. FAILLA: Yes.

DR. FRIEDELL: In your original proposal you said you needed to know certain things before NEPA decided how to correlate the biological program.

I remember the discussion that ensued a year ago in Chicago in which we said there might be two conditions under which you wouldn't need a committee at all, any biological research; and that is, for example, you could build an airplane that would fly with a total exposure of, say, one roentgen; or if it would fly with 100,000 roentgens. There it is perfectly clear that the committee wouldn't be very useful.

We hope that it will fly somewhere in between that range, and if we had some idea as to where we would be, we could maybe decide how important and how critical some of these biological problems become.

CHAIRMAN DOWDY: I admit I am just a little bit confused at this point.

DR. TITUS EVANS: I felt somewhat along that line, and I felt that maybe we just shouldn't do anything from a biological point of view until the engineers could tell us what the mixtures would be. Then we would set some animals up and get some empirical data. If we wait for theoretical calculations —

DR. STONE: They are just asking us what type of mixture we would like.

DR. FAILLA: The design would depend on what importance they had to assign to neutrons and what importance they had to assign to gamma rays.

MR. SIMMONS: That is right. We don't know at this time the spectrum that will emerge from aircraft shields, but we want to be in a position that when we do know the emergence effect, we can evaluate the damage potential of that spectrum and decide whether we have to put another foot of tungsten around the thing, or whether we can take off an inch of boron or something like that.

DR. STAFFORD WARREN: But anticipate certain things, and from a biological standpoint we ought to know enough by now to anticipate what you are going to need, evidence over this dosage range we discussed this morning.

MR. SIMMONS: In other words, a refinement of this data would be bringing these effects down to a unit which would be amenable to the interpretation in neutron fluxes versus energy and high energy gamma radiation.

DR. STAFFORD WARREN: We ought not to be in the position we were during the war, where we had to set up this tolerance dose, and we arbitrarily accepted a certain dose; and at the end of two years of experimentation we find that if we had exposed a large number of our personnel to that tolerance every working day, we might have assumed that we would have found a lot of injuries.

Fortunately, the hypothesis that was used was that we would have no exposure except where it would be necessary. Therefore, we did not have that injury.

We have told you, at least in the Chicago meeting, that it might be likely that 100 roentgens is something to shoot at, and we ought to come up in a couple of years with something about the time you are ready to go with something definite; that 100 is pretty safe or it isn't.

MR. SIMMONS: You are going to tell us what 100 roentgens is.

DR. FRIEDELL: I don't think that is the critical problem.

MR. SIMMONS: In other words, if you tell us 100 rems, then we have got to know how many rems is 100 —

DR. FRIEDELL: What is more critical is what if you are going to be working in levels at 50 or 100 rems or 1000 rems.

DR. ELLINGER: You must start experimentation somewhere and this is really what NEPA would like to have done. Couldn't we now begin a set of experiments where sources are available, to start to get some more basic data? Few are available, or very little.

DR. FAILLA: First of all, I go back again to you have got to decide what effects you are going to study, because this ratio varies with the effect of study. What are we interested in trying to find out?

CHAIRMAN DOWDY: Aren't we interested in the physical fitness of these people to 100 r? Are they going to be fatigued? Are they going to be dull and full of apprehension, lack alertness?

DR. FAILLA: Then you have to plan your experiments to give you that answer.

CHAIRMAN DOWDY: That can be done.

DR. STAFFORD WARREN: You are also going to have to consider the ultimate fate of the individual, too. There is a Veterans Administration insurance gentleman over here interested in that. So are the parents and the individuals.

CHAIRMAN DOWDY: I think the discussion is heading up that we have run into a little difficulty on the pharmalogical material there. Why don't we just pass over that for a moment and leave it and go to the evaluation of the physical fitness of animals to these various dosages, which can be determined by very good scientific methods such as treadmills; swimming tests; Hardy-Wolf pain stimulator; activity cages; voluntary willingness to exert; and mental alertness.

Dr. Donaldson just told me before lunch that he, with his fish, has been able to rather accurately correlate dosage with temperature and indigestion of food relative to nausea and vomiting, and got a very good correlation.

Now, it seems to me that we can accept the evalnation of physical fitness as one of the things that they certainly need. Now, how the experiment is set up will be determined later.

DR. FAILLA: Physical fitness within twenty-four or forty-eight hours or a week, or something like that?

CHAIRMAN DOWDY: Yes.

DR. NEWELL: It looks highly practical.

DR. FAILLA: Yes.

DR. TITUS EVANS: In fact, I think they all do. I think they all are practical.

DR. NEWELL: These they put down here are mostly in the direction of seeing the effect of irradiation on the abilities. One might also consider the correlation of the immediate symptoms with survival if we are going to do these human experiments. That is in the reverse direction, but comes under the same heading.

CHAIRMAN DOWDY: Whoever does human experiments might use some of these. I don't know.

Now, I don't think we are going to need much discussion on IV. That is one of the musts.

DR. ROBLEY EVANS: There is one important point. Maybe you have the answer on IV, Mr. Simmons. Using an ordinary garden variety type of shield which you could set together right now without a whole lot more research; suppose you need this machine six months from now and you have got to start building it tomorrow morning.

Roughly, what is going to be your gamma ray flux in comparison with your neutron flux? In other words, if you take a RBE of 10 plus a minus 8, say, just to be ridiculous, does this really handicap you in the design of the shield. Is the major external radiation going to be gamma rays anyhow, or is the major external radiation going to be neutrons?

This really tells us the importance of No. IV.

MR. SIMMONS: I think that we are going to be somewhere bound to an even break as far as total energy available in the field is concerned, but that we can say this. The shield performs two functions. First, it absorbs the energy and reduces the total energy available. The second thing it does is harden the spectrum; that any spectrum you start with on the inside of the shield is going to be hardened as it emerges. Therefore, we intend to raise whatever total energy we are left with after the shield has performed its function; we have raised that into higher energies, so we are particularly interested in the equivalence at the higher energy ranges.

Let's say we decide we have to have an overall attenuation, say 10^{-8} on the initial radiation emerging from the reactor; and we find that 10^{-8} fraction of the radiation is at 10 million volts or above. So this means we either have to take out everything below 10 million volts, which of course is an impossibility; and then let that go without effect.

That is an overemphasis just to point out the problem, but we know we can not remove all of the low energy radiation; but we can remove a higher percentage of the low quantum radiation than we can of the initial high energy.

What we are actually striving for is the minimum necessary attenuation of the high energy components. This is based on the argument, "Well, if they have such mean-free paths that they penetrate the shield, they are in the same status as far as the cargo is concerned."

We believe that there is a theoretical maximum of effectiveness for a given energy of neutrons. If you plotted the curve out far enough, you would have a maximum neutron effectiveness. This is irrespective of spectrum, just monochromatically. Say the same energy would be less effective at 4 than at 2 million.

DR. ROBLEY EVANS: Let me ask you, of course in a different way and not quite the way Dr. Newell has written behind you, which he is ready to spring on you; if you use the 10^7 on the shield, which would kill the pilot, the gamma rays or the neutrons?

MR. KALITINSKY: I think we can answer that. I think that in any well-designed shield they will be about equal. You can see very easily why that is so, because to really bring radiation down to practically zero, you have to use a very large mass, whether it is gammas or whether it is neutrons.

So that if you tried, for instance, to kill all the gammas and not bother about the neutrons, you would have to have an exorbitant amount of material to stop the gammas. The same is true of the neutrons.

So, quite obviously, the light-as-possible shield will be one that will attenuate the gammas and the neutrons.

DR. ROBLEY EVANS: That is not the point.

MR. SIMMONS: Which will kill the pilot?

DR. MILLER: That is what we want to know.

MR. SIMMONS: Which is the most effective?

DR. ROBLEY EVANS: You don't have to know the RBE. If it is the gamma ray that kills the pilot, you don't have to know the RBE.

MR. SIMMONS: How are we going to find out which kills him?

DR. ROBLEY EVANS: I am asking you.

MR. SIMMONS: It is not a biological problem.

MR. KALITINSKY: We are giving you back the answer.

MR. SIMMONS: Let us assume they are equal.

DR. ROBLEY EVANS: When you say you are assuming they are equal, what RBE have you assumed in your calculation?

MR. KALITINSKY: We have assumed there is about an equal quantity, because if your flux is 10^4 gammas, it will also be 10^4 neutrons.

DR. ROBLEY EVANS: You have certainly made calculations of the external radiation and rems. When you did it, what portion of the rems were due to gamma rays and what portion to neutrons?

DR. FAILLA: According to that, perhaps 80 percent.

MR. SIMMONS: We haven't such a calculation. We have not made one. We have no basis for making one. Well, we could use this factor of 2.5, but we don't believe that.

DR. FAILLA: What is the 2.5?

DR. ROBLEY EVANS: Take any other reactor, then, which you have studied. Take any of the going reactors with their present shields. Which is the most hazardous radiation that emerges from the shield: the gamma rays or the neutrons?

DR. STAFFORD WARREN: They are both hazardous.

MR. SIMMONS: I don't think anybody knows the answer to that question.

DR. FAILLA: Neutrons are ten times more effective than gamma rays, and the neutron beam or the neutron element —

MR. SIMMONS: Correct. But we don't know that the neutrons are ten times more effective, do we?

DR. FAILLA: We know that they are much more effective than gamma rays.

CHAIRMAN DOWDY: We don't know what they will be at that energy.

DR. FAILLA: In terms of ionization.

DR. NEWELL: I am not much of a mathematician, but isn't it true that if the relationship of emergent flux to cost of shield was the power relationship for both gammas and neutrons, and they were independent, then your economy would be to have equal effects coming out, and your economy would only be to have much more of one than the other, if the relationship of one was a considerable power compared to a first power relationship with the other.

Am I wrong about that?

MR. SIMMONS: I don't think that is wrong. The only assumption that we have been making — we may be wrong on this, and this is where we need correction from this group, if it is wrong — we are working under the assumption that the damage potential — we are not saying what that damage is; it might be leukemia or running fits — but the damage potential from a given radiation field is some function of the amount of energy that the body absorbs from that field, the integral of the utilized energy.

DR. ROBLEY EVANS: It is for the gamma rays, but it certainly isn't for the neutrons.

MR. SIMMONS: Of course, the ridiculous case in here would be neutrinos. Let's say the Hanford pile puts out a few thousand kilowatts of neutrinos, and you have a fairly intense radiation field. It isn't utilized. As you increase the energy of a neutron, it approaches the neutrino in its utilization factor.

So we are going on the rough premise that we can calculate — I haven't got the figures here — the energy potential of any given field of monochromatic gammas or monochromatic neutrons, and we can interpret that into a distribution of these groups and say, "Well, so much energy will be absorbed by a hunk of meat."

DR. ROBLEY EVANS: All right, if you just use a concrete shield or something of that crude sort, and then in a side of beef there is just as much gamma ray energy absorbed as neutron energy?

MR. SIMMONS: It depends on the spectrum. I would think that in a hunk of beef that there would be more neutrons absorbed than gammas.

DR. ROBLEY EVANS: Do you mean more energy or more particles?

MR. SIMMONS: I don't know. I don't have a numerical answer to that question, but I say it can be calculated.

DR. ROBLEY EVANS: It can be calculated, and it is pure physics, and you don't have to ask a biological question if the gamma rays are the important radiation.

MR. SIMMONS: What we are asking is, is this a reasonable assumption, that the biological hazard is a very close relation to the energy utilized?

DR. ROBLEY EVANS: I think not, because of the RBE.

DR. STONE: Again it depends, if you are up high enough in energy so that the energy absorption through the body is fairly evenly distributed, then you are getting up where it is; but if you are in an area where it is distributed all near the surface, then it isn't.

DR. FAILLA: On one occasion it is protons; on another occasion it is electrons.

DR. STONE: If you get up to the point where your neutrons and your gamma rays are both going relatively far into the body, then we already know the RBE for a great many factors.

DR. FAILLA: I see what you mean; yes.

DR. STONE: Therefore, if we know that they will all be neutrons of, let's say, 5 mev or above, the variation from 5 to 10 isn't going to be too great.

But if you get down much below that, below, say, to 2 or below, then the variation begins to get greater; but the depth to which they go gets less so that you will have to work on that factor as well. They won't be evenly distributed in the body, and you have got a balance of two factors, whether they are evenly distributed or whether they are absorbed near the particular surface that happens to be exposed.

CHAIRMAN DOWDY: I think this is a question that the physicists will have to sit down and —

DR. ROBLEY EVANS: I think there is a great deal that can be done by the calculations that Mr. Simmons is already using. For example, in our first meeting in Chicago, you gave us the results in draft form of the large number of calculations on whether the aircraft could fly in terms of the weight of the shield.

Now, a similar set of computations can be given as to whether or not accurate knowledge of RBE is important. You can make a series of assumptions that it is 2, it is 5, it is 10, or it is 40. And it may be that your dosages to the pilot will almost be independent of RBE because you made your radiation to gamma rays.

On the other hand, it may be the reverse. If it is the reverse, then you are absolutely right. Number IV is your important project of top priority. If it is gamma rays, then No. IV is not the project of top priority.

MR. SIMMONS: I don't think there are any indications to show that we would have any reason to say it is gamma rays.

DR. ROBLEY EVANS: My point is, from the standpoint of physics you can calculate —

MR. SIMMONS: Well, you can calculate how much energy is absorbed, and you can say where it is absorbed in the target. I mean you can have your cards keep track of actually where each reaction takes place in the body.

But then, to interpret this as to damage potential is a problem for the biological people.

DR. ROBLEY EVANS: You can run your Monte Carlo calculation through several times with different RBE's for the high energy neutrons, and you can see whether there is any difference in that overall effectiveness, if you want to measure your rems at 30 feet or so, an arbitrary thing.

It may turn out that this is all primarily gamma radiation and neutron capture gamma radiation originating in your shield. If that turns out to be true, then Project IV is not number one priority.

You have got to know whether rems depend strongly on RBE or not. By rem, I mean the sum of your gamma ray and neutron effects. Depending on the importance of the gamma radiation emitted from the shield and the captured gamma rays produced in the shield, it could be, just like your graphs there. It could be. You are going to go through Monte Carlo about three or four times with an RBE of several valuations, 2, 5, 10, or 40.

MR. SIMMONS: We have no indication to lead either way on the question.

DR. ROBLEY EVANS: When you get the answer, you will know whether Number IV is right. You are going to be the one who has to decide whether Number IV has top priority, not us. It depends on how it enters your calculation.

Maybe you don't care about RBE, and any value between 2 and 40 gives you the same shield.

MR. SIMMONS: Another thing, in running these Monte Carlo calculations we need to have intelligent targets. In order to set up a target of geometry for the human body, we have got to have guidance from somebody who can do that for us.

We are not capable of saying what the density of hydrogen, oxygen, carbon, sulphur, phosphorous, and all the other ingredients should be in this mass where we use the Carlo technique.

DR. ROBLEY EVANS: You want a standard man —

MR. SIMMONS: A chemical man to plug into the Monte Carlo.

DR. ROBLEY EVANS: Somebody is working on that. It seems to me we can't decide the priority on the IV until you have a lot more arithmetic done.

MR. SIMMONS: I think this is a part of IV; that this type of work would come under IV.

DR. ROBLEY EVANS: Then that is top. But the determination biologically of RBE for the lens of the eye and for the base of the fingernail or other things may be unimportant to you.

CHAIRMAN DOWDY: If he includes that in IV, then we could give No. IV number one.

DR. ROBLEY EVANS: That would give IV number one. That is IV-A. Then IV-A is number one. Then IV-B may never come up.

DR. NEWELL: Are you sure that we are not suffering here from something that we have suffered from a great deal, and that is the necessity for security, the necessity for secrecy? Are you sure that you are permitted to be frank with us here?

I would suggest that the thing to do is to get Dr. Evans and Dr. Simmons together on the quiet and let Dr. Simmons tell Dr. Evans all about what he has done in regard to these shields so that Dr. Evans can satisfy himself whether, in fact, there will not actually in practice be so much of a positive correlation between neutron absorption and gamma absorption that you do not come out with an even balance at the best thickness of a shield, that you come out with a balance of —

MR. SIMMONS: We are still working with these calculations. They have not been completed. All these things are just tentative opinions at the moment based on the work we have done so far.

DR. NEWELL: To my mind it would be very remarkable if the most efficient shield would be one which produced even quantities of neutron flux and gamma flux leaking out. It would be an astonishing coincidence because I think that the absorption of neutron flux and gamma flux do have a correlation.

MR. SIMMONS: The final criteria on this thing that tells you the effectiveness is the human body. We are using fission counters and other mechanical instruments now in trying to interpret what happens to those in terms of what this means if the counter was a man.

DR. NEWELL: The difficulty with the calculations is, the calculations are in terms of energy absorption. The radiologists have escaped lots of that difficulty by dealing not with energy absorption but with the significant quantity which is capacity for ionization and the unit that they do all their absorptions with and all their protection problem with is the roentgen, which is not a matter of how much energy was absorbed in going through the protective factor, but how much the

danger was decreased in going through the protective factor, and ignore everything about the energy absorption. They have cut through it by jumping clear over to the significant quantity.

It might be that you could solve some of your difficulties by jumping over with an empirical absorption law, paying attention to nothing except either the ionization going through or the biological effect.

MR. SIMMONS: I am wondering whether it is wise to skip over this energy absorption thing, because that is the real field that is operating.

DR. NEWELL: You like energy absorption because you can calculate it. But the radiologist has got rid of his difficulty in that regard by paying no attention to the energy absorption. He is measuring a purely pragmatic quantity —

MR. SIMMONS: Which works as well until you get into neutrons and radiation of that type which just doesn't fit. I mean, that is why we have cases where one man gets 1500 rem and lives twenty years and wears out three wives; and another man gets 150 r and dies in ten days, and they are supposed to be the same units.

CHAIRMAN DOWDY: We have tentatively here agreed on priority number one for No. IV.

DR. ROBLEY EVANS: Not the biological part; the physical part. The part that is not written here is the part that has top priority.

CHAIRMAN DOWDY: How about No. III? Can we assign that a priority? That is the evaluation of the physical fitness.

DR. NIMS: I would recommend that be the second in order of priority because their immediate problem is military effectiveness under the conditions which the plane has to operate.

CHAIRMAN DOWDY: Priority No. 2 on that? Is that agreeable?

DR. TITUS EVANS: How about this compilation of available data? Isn't that very important?

CHAIRMAN DOWDY: We will come to that.

DR. TITUS EVANS: We are going to consider all of them together?

CHAIRMAN DOWDY: We are going to consider all of them. No. VI is the continuation of compilation of available data as initiated by NEPA Advisory Committee along the lines of this blue book. I think that should be continued.

DR. NIMS: Isn't it also being done in connection with the others, setting tolerances in general?

CHAIRMAN DOWDY: I don't know.

DR. HOLLAENDER: The National Research Council Committee is doing something.

CHAIRMAN DOWDY: They have been sending out abstracts, but totally uncorrelated and unrelated.

DR. NIMS: It is not duplication, but there should be some coordination on these compiling groups. There are quite a number of them.

DR. HOLLAENDER: Dr. Perry, who was the Secretary of this —

CHAIRMAN DOWDY: That is something else. That is entirely different.

DR. FAILLA: What does this mean? Continuing what has been done before?

CHAIRMAN DOWDY: Making this more complete and keeping it up to date.

DR. FAILLA: I don't think there is any question about that. I think it is desirable.

CHAIRMAN DOWDY: That is what I thought. If anybody else is doing it, I don't know. Had we known it, we would have tried to have gotten that information.

DR. NEWELL: I think it should be continued, surely.

CHAIRMAN DOWDY: What sort of a priority would you give that?

DR. NEWELL: The first priority would be the human experiments, would it not?

CHAIRMAN DOWDY: You can have more than one priority number one, can't you?

DR. NEWELL: Human experiments would be priority one; and No. III would be priority two; and No. VI would be priority three.

DR. TITUS EVANS: By saying "priority three," you don't mean it should be stopped until the others were done? Why not make it priority one, also? We are going to keep that going.

DR. NEWELL: I thought you were trying to order them.

CHAIRMAN DOWDY: No.

DR. STAFFORD WARREN: This whole list is priority one, isn't it?

DR. ROBLEY EVANS: If we are going to try to arrange these, wouldn't it be simpler to have category VII, which is your Monte Carlo and has first priority? It really isn't the same as IV here, when you put IV in the category of I, II, III, V and VI. Monte Carlo is just as separate as this is.

CHAIRMAN DOWDY: Monte Carlo, priority one.

DR. ROBLEY EVANS: Let's not call it IV-A any more. Call it VII.

CHAIRMAN DOWDY: I have got it VII.

DR. FRIEDELL: We have been belaboring these research problems for a long time, and I don't think we have come to a very good solution.

Would it be appropriate to suggest that a subcommittee be appointed who will work very closely with the NEPA people and who will make a report as to how the priority ought to be arranged and as to what looks to be the critical problems?

CHAIRMAN DOWDY: I suggested that we do this, and have a meeting of that committee prior to this so we could do this first part.

The Committee at that time didn't think it would profit very much.

CHAIRMAN DOWDY: What I would like to do, these topics should be recommended to NEPA to pay attention to; then we can appoint a subcommittee after they look them over and decide wherein they feel they fit or what they want to do with them; and then set up a subcommittee to go over these in detail with specific recommendations.

DR. FRIEDELL: I think it might even be better to give the subcommittee a free hand and let them work it out and make a report to this Committee.

In that way they would be quite cognizant of all the problems because of the discussion that has gone on, and we can ask them questions which they could answer more intelligently than we can do now.

I just feel that we are foundering a little bit and it might be the best solution. I would be willing to make that as a motion because I feel rather strongly about it.

CHAIRMAN DOWDY: I think if we are going to consider detailed approaches to these fundamental problems, which I never intended that we should do, I think there are six or seven primary problems here. We could recommend their importance, and then they can decide whether they want to continue the medical committee or not, or ask us to set up subcommittees to study particular problems, with recommendations.

Wouldn't we get just as far and just as fast that way, or not? Is there a second to Dr. Friedell's motion?

DR. FAILLA: Did he make a motion?

DR. FRIEDELL: I moved that a subcommittee be appointed to study the priority of the research program in conjunction with the NEPA representatives.

DR. STAFFORD WARREN: I will second that.

DR. SELLE: As I understand it, Dr. Friedell, you moved that a subcommittee be appointed to study the priority of the research program in conjunction with the NEPA representatives. Is that right?

DR. FRIEDELL: The character and priority of the specific research problems.

DR. ROBLEY EVANS: Biological research problems.

CHAIRMAN DOWDY: The motion has been made by Dr. Friedell and seconded by Dr. Warren.

All those in favor say "aye."

(General response: Aye.)

CHAIRMAN DOWDY: Contrary, the same.

(There was no opposition.)

CHAIRMAN DOWDY: The motion is carried.

DR. STONE: I think we are getting somewhere with this discussion. There is some advantage in having a large group like this, even though you don't get very far, throw in their ideas. That is the idea of having a lot of people in. We are here now. Why throw this over until another time? I think you might go on with a discussion of these things as you have been doing.

CHAIRMAN DOWDY: Because this committee would eventually have been set up anyway, regardless of our discussion. So I am perfectly willing to continue.

The subcommittee is inevitable, as I see it, and always have seen it.

DR. STONE: I would like to ask a question. Mr. Simmons seems to assume that there is very little data on the conversion factor of N to r. I think there is a lot of data on the conversion factor of N to r, but you have to know what problem you are dealing with.

Dr. Friedell and Dr. Evans during the wartime project got a lot of information on that. The only thing that we are lacking in is just what range to work in.

You were working with a maximum of 9 mev?

DR. FAILLA: Heterogeneous beams.

DR. STONE: Aren't we always going to have to work with heterogeneous beams? You can't get monochromatic beams. You have got work in Berkeley that can be backed down a little bit before the war that gives you something on the 16 mev, and some on the 8 mev that they did before the war.

The human work that we did before the war, there we have a relation insofar as the small end of the r is concerned; and we know something of the factors now so that you get very close approximation. But when you go from one biological reaction to another, they vary considerably. So you have got to know which particular one you want.

We know they vary, and there is nothing going to solve the problem. It is there.

The thing that interests me is the range we want to know about, which is unexplored. Now, Gray in England has been working on the 3 mev range largely, isn't it? He has got a lot of data over there on correlation of energy absorption.

CHAIRMAN DOWDY: We worked with 12 mev. Another thing is the chronicity of this type radiation.

MR. SIMMONS: Perhaps one of the important things, then, is to get this data together.

DR. STONE: I should think Dr. Failla and Dr. Evans could give you — they went over this whole field very thoroughly.

DR. STONE: N to r relationship.

DR. FAILLA: The thing that is being used by the committee setting up a permissible limit of exposure is a factor of 10, 10 reps of gamma rays equal to 1 rem of neutrons.

CHAIRMAN DOWDY: It goes all the way from 4 to 16. That is just an average.

MR. SIMMONS: This doesn't give any indication of biological effectiveness.

DR. FAILLA: Yes, that is what it does.

DR. FRIEDELL: In other words, one rep of gamma rays is ten rems of neutrons; whereas ten rems of neutrons is one rep of gamma.

DR. FAILLA: The other way around. Let's leave out the rem from this thing. For the same amount of energy absorbed per gram of tissue, you can use — let's put it the other way around. For the same biological effect, you have to use ten times more energy absorbed per gram of tissue if the radiation is gamma radiation than if it is fast neutrons.

MR. KALITINSKY: There is one point I would like to clarify in some of our thinking here. On the shield calculations we have done so far, we have used biological equivalents; and there was some question about whether gammas or neutrons are predominant.

We have one specific case in which we simply assumed them to be quite separate. We tried to figure out how much do we have to attenuate the

gammas so that the gammas alone could use 1 r, rem in this case, per hour. Then we took the fast neutrons and figured out how much we have to attennate them to produce 1 rem per hour, because we figured that from then on, if you add them, you would get twice the tolerance theoretically; but it is easy enough to cut both of them in two from then on.

DR. FAILLA: The biological factor in that?

MR. KALITINSKY: Yes.

DR. FAILLA: What?

MR. KALITINSKY: Ten. The attenuation for the fast neutrons came out in that particular case 10^8, I think; 1.6 times 10^8 r; and for the gammas came out six point something times 10^7. So, for all practical purposes, they are the same.

I think that point should be clarified. The neutrons are slightly more difficult than the gammas.

DR. FRIEDELL: Biologically?

MR. KALITINSKY: Yes. But that was made on a gross assumption as far as the effectiveness of fast neutrons. That is, the flux predominant is 2 mev neutrons.

What we would like to know, to be sure, are we making a mistake? Are we being over-optimistic here by neglecting, let's say, the 8 mev neutrons, which come through the shield in a higher percentage than 2 mev?

DR. FAILLA: I don't think so. I don't think there is much of a difference. The thing that comes in at higher energies is that the heavier recoils contribute more energy, say the oxygen recoil. I think we are making calculations on the basis of the cross-section values that we could get hold of.

At the higher energies some 25 percent of the energy is in the form of a heavy recoil and is absorbed, that is, heavy recoil; whereas at the lower energies it is only about 10 percent.

If the biological effectiveness of the heavy recoil is very much greater than the regular one attained, it might be a considerable factor.

MR. KALITINSKY: Here is another question. Could we get some opinions as to how good this factor of 10^1 is?

DR. FAILLA: That factor of 10^1 is not very good for the simple reason that there is no equivalence between gamma rays and fast neutrons for all effects.

Now, this factor of 10 has been taken as representing an average condition.

DR. ROBLEY EVANS: Conservatively.

DR. FAILLA: Conservatively in some respects, and not in others; and also including the fact that the exposure is over a longer period of time, because that factor varies with the time of exposure.

It is lower for the acute exposure than it is for the chronic exposure. So you see, that factor is an average which presumably applies to long exposures rather than short exposures, and you are more interested in the short exposure.

CHAIRMAN DOWDY: Which would be somewhat lower than the 10.

DR. FAILLA: Yes.

DR. STONE: A factor of 10 applies to N as measured, rather than to rep.

DR. FAILLA: No, it applies to rep.

DR. STONE: You are making it 25, then?

DR. FAILLA: Twenty.

DR. STONE: That is a very conservative factor.

DR. NEWELL: You were experienced with your fast neutron of radiation for —

DR. STONE: No, that was down closer between 6 and 10 of n to r, and you had to divide that by 2.5. So it came to a factor of 4. We used 2.5

DR. ROBLEY EVANS: You mean capital "N" when you say N. Dr. Stone means small "n" when he says n.

CHAIRMAN DOWDY: Isn't that awfully high?

DR. STONE: I thought n to r was 10.

DR. ROBLEY EVANS: It always was; but Failla is putting in an extra factor of 2, or 2-1/2, for chronic effects.

DR. FAILLA: No, I am not putting anything in. The confusion has been between calling the thing reps and calling it N. The ratio of X to N as given ordinarily is for the 100 r, and that is a factor of 8; but when we have talked about permissible limits, we are talking in terms of reps; and even in the old Manhattan District recommendation, it wasn't in terms of rep.

At that time the factor was 1 N equals 5 reps to give 1 rem for each.

Now, then, we doubled that factor for the reason that we found that some of these effects were produced more readily by neutrons than by X-rays; so we doubled the factor. That made it 1 to 10. If you want to express it in other terms with the Victorine chamber, then it would be in the range of 1 to 20 or 1 to something else, depending on which chamber you use.

MR. SIMMONS: Dr. Failla or Dr. Stone, I would like to ask this question. These factors we are talking about, what correlation with energy do we have? It is certainly dependent on energy. It is an energy dependent function.

DR. STONE: Yes, but once you get above, let's say, 2 or 3 mev, the change is not too great, dependent on energy. Now, the distribution in the body changes there because it goes to greater depths and you affect deeper organs.

If you are talking about a specific effect like erythema dose or effect on some cells that you have outside, or something like that, where it doesn't enter in, it is a question of the absorption at the point that you are interested in.

We had worked with the pile down at Oak Ridge, and that was with the fission neutrons; and worked at Chicago with about 8 mev; and Dr. Failla's work, and then the work at Rochester and the work at the other places; and the factors all came out fairly close together, somewhere around the 5 that we talked about, or 10, whichever you want to call it; 10, if you are comparing it on a straight basis.

There was a little variation, but there was more variation from reaction to reaction studied than there was energy to energy.

Would you agree from about 3 up to 10 there is a slight fall-off as you go up and measure —

MR. SIMMONS: That is where the physical analysis of the situation indicates that as you go up in energy, more and more neutrons are not utilized.

DR. FAILLA: I'll tell you where the trouble is. You are basing your reasoning on something else. The only thing that is taken into account in this situation is this specific ionization, which is the number of ions produced per centimeter, i.e., path of the charged particles.

That does not change much with energy until you get way up; whereas the protons will have a terrific effect. Therefore, there wouldn't be very much difference in the biological effectiveness of high energy neutrons with respect to energy because that factor does not change much.

MR. SIMMONS: Yes. But from the other point of view, you could say that at 10 million volts, 25 centimeters of water, 8 percent of 10 million volt neutrons will pass through without any reaction whatsoever.

DR. FAILLA: Then you have no energy absorbed.

MR. SIMMONS: Only 60 percent, neutrons. We have got a difference there —

DR. FAILLA: All right. You have to make your calculations in terms of energy absorbed per gram of water or whatever you choose. Then you can make the comparison, not in terms of the energy that —

DR. STONE: We don't think in terms of the energy of the beam.

DR. FAILLA: Energy absorbed.

MR. SIMMONS: We start out with energy available.

DR. FAILLA: That is right.

MR. SIMMONS: Then we have a utilization factor which gives us the energy —

DR. FAILLA: The energy absorbed is a big mass.

MR. SIMMONS: That is what we are doing, practically. We have just one centimeter of tissue there as a part. That would be entirely different than if you had a body, a massive body.

DR. FAILLA: You may be somewhat in error by doing that, because if we consider energies for which the absorption would be very irregular, not uniform —

MR. SIMMONS: Isn't this the difference between local and whole body radiation that we are talking about?

DR. STONE: You would be getting somewhat the difference between the gram roentgen and the roentgen.

DR. FAILLA: That is what I said in the very beginning when they brought up that discussion. You have got to have the conditions so that the distribution of energy absorbed per gram of tissue is similar to what we are familiar with.

MR. SIMMONS: That isn't uniform, in other words. If you consider energy absorbed per gram, that is a local condition, which isn't an index particularly of the total energy absorbed by the object, which is the thing that is hurting.

In other words, at one point you might have too much energy per gram, and another point you might have very little.

DR. FAILLA: That is right.

MR. SIMMONS: We have got to integrate this thing with a standard man.

DR. FAILLA: You can't be sure that the individual body is correlated with the effect. That is the thing I pointed out in the very beginning.

MR. SIMMONS: But then, if you start from this basis to analyze the effect of this radiation biologically, it was my idea that you might have a sounder starting point.

DR. FAILLA: That has been shown to be a fairly good approximation.

MR. SIMMONS: Then you still have to interpret this biologically the same way that you now interpret based on the gram roentgen energy per cc.

DR. FAILLA: Let's say that you have made a calculation, that the distribution in the body is such that at the surface it is 100 percent, and that the other side of the body is, say 35 percent —

MR. SIMMONS: And this is with respect to energy?

DR. FAILLA: Yes. And I will say those are the conditions essentially under which we have some information about X-ray effects. Therefore, if you assume a factor of 10 for the biological effectiveness of that dose in terms of energy absorbed per gram, you won't be very far from the truth in predicting what is going to happen to those people.

CHAIRMAN DOWDY: Could I get us back here just a moment?

On this subcommittee as approved, are they to start from scratch on research problems, or should they consider them under these seven headings that we have here?

DR. STONE: They can use this to start from.

DR. FRIEDELL: I think this sort of a discussion is one of the things that ought to go on, because they have been making calculations from the integrated dose and information on the different kind of distributions, doses of absorption.

I think just such things would develop which would give us an idea of which things ought to be first. So I would say let's give them a free hand and let them start from the beginning.

DR. FAILLA: I think the subcommittee should report to this main body so that these things can be discussed by a larger group of individuals.

DR. FRIEDELL: You mean report back to this committee?

DR. NIMS: I think there could be something done here, and that is, set the purpose of what biological research they want to do. In all of these particular things, the first thing NEPA is interested in is whether the mission can be accomplished or not. The second thing they are interested in is the cost of that mission with respect to short and long-term effects.

I think we could make a recommendation right now that the thing they should begin work on very soon is these high level radiation effects that have been proposed here; supposed effects with given dosages where the accuracy is not large or not great.

I think they would be very happy to know that a mission could be accomplished with 200 r. Also, they would be happy to know that if the mission could be accomplished with 100 r, the biological cost would probably be reduced to one-tenth the cost of the 200 r exposure.

If we take it from that point of view on the thing, these problems of selection and the problems of treatment become secondary problems.

DR. TITUS EVANS: The r to N ratio is of secondary priority?

DR. NIMS: The r to N ratio is important on this because you need that to essay the top.

DR. STONE: I think what we need here on the r to N ratio is for Dr. Failla and Dr. Evans both to get together with you two and —

MR. SIMMONS: I think that would be very helpful.

DR. STONE: I think you are approaching this from another angle than what we have the information on.

MR. SIMMONS: That is true.

DR. STONE: For instance, we found out that the absorption of energy from a 200 KV beam and from what we thought was 16 mev neutron beam was almost identical as measured both with the ionization chamber and with the induction of activity in silver, I think it was. I forget the exact material.

But we measured the neutrons getting in there in proportion to the neutrons on the surface. A 200 KV X-ray beam and a 16 mev neutron beam gave about the same effect of ionization as it went down through the body.

Now, that doesn't mean that they produce the same biological effect, because they did not; but the percentage that got to the various parts of the body was the same. That is really what is important, whether a lot is absorbed on one surface or the other surface.

It isn't altogether what goes all the way through the body. If you radiate the bones very heavily, then you get a maximum effect on your bone marrow and that becomes your predominating effect; whereas if you radiate the skin very heavily and the tissues just under the skin, you may have not very much effect on your blood, for instance.

MR. SIMMONS: In these things we have no hope of trying to evaluate these factors that you are mentioning. That is a field that you gentlemen are guiding us in.

All we were trying to do was to arrive at a basis of energy level to start interpreting from; in other words, an amendment by which you would have a fairly accurate knowledge of the actual distribution in the body of these reactions. Then, the physiological results of the reaction is something that can only be coped with by the biologists and the physiologists and the M.D.'s.

DR. STONE: Of course, that is why I say if you were to get together with Dr. Failla, he has been attempting all his life to interpret physical data to us and biological data to the physicists, I think you can get much farther ahead than you can by open discussion.

MR. SIMMONS: It may be that there is a great deal of work that has already been done in this direction that Dr. Failla can tell us about.

CHAIRMAN DOWDY: I think we are getting some place here. I think that Dr. Nims had a very good point. I was hoping that out of this we could have certain categories of problems that were of urgency as far as NEPA was concerned.

Once they were set in a general discussion like this, turn them over to the subcommittee for thorough thrashing and crystallization and bring them back to this committee.

I think that his point which he made of several different things that should be done are the ones that perhaps the subcommittee should consider. I would like a little more discussion along those lines, if we could.

DR. STONE: We perhaps need two subcommittees. Maybe you need a subcommittee on this No. IV, the conversion factor, a separate subcommittee from anything else.

CHAIRMAN DOWDY: I just scratched out here, while we were talking, one committee on physical relationships and another one on research. I think maybe we ought to discuss it with NEPA, whether we need more than two.

DR. FRIEDELL: Why don't you just combine the two? Put members on that would fit in either one of those committees. I think they are closely intertwined.

CHAIRMAN DOWDY: They won't be separated entirely, but will have a cross membership.

DR. FRIEDELL: I think it would be much simpler to make one subcommittee. Put on it people like Failla and Dr. Evans who could do these things. It would be much simpler. They could integrate this thing very easily at their own meetings.

I really feel that the proposal by Dr. Nims as to the broad general over-all approach, what are the levels we are interested in; what are the costs going to be; weighing it against the probability and feasibility of flying the airplane, is a broad general over-all thing, anyway.

I don't think you could even instruct anybody to decide about those things unless these people already have rather concrete data on what is going to happen with the various kinds of shields.

DR. STAFFORD WARREN: If you get the work done, you have got to start with a small group.

DR. FRIEDELL: I feel that these things ought to be presented to this large committee by a subcommittee composing the elements which we have discussed, which will outline to us what the critical problems are and how they are related to these various things, and present them for approval here.

I think that if you make more than one committee or one subcommittee, you are going to get into difficulty.

DR. TITUS EVANS: May I say just about three sentences in a summary of a question of this r to N factor?

CHAIRMAN DOWDY: Yes, sir.

DR. TITUS EVANS: I would like the whole group to realize the complexity of the problem in a general way, at least. That is, it seems that we have found that the r to N factor will vary with the particular tissue that we are most interested in. We will have to determine which is most critical.

It also seems to vary with the duration of the exposure; the time of the exposure, that is, acute or chronic.

Then, apparently, it seems to vary with the energy of the neutron. So that brings up the question of activity.

Can we add a certain dose of neutrons and a certain dose of X-rays? That brings up the problems from the physics side of measuring them accurately and also calculating them so that we can convert ionization measurements into terms of neutrons.

When we get to where we can understand each other in those terms, then I think we will be able to collect your data.

CHAIRMAN DOWDY: Is there any further discussion?

DR. ELLINGER: Are you planning discussions with Dr. Gray, who is coming down to Oak Ridge next week?

CHAIRMAN DOWDY: The NEPA people will probably hold discussions with him.

DR. STONE: If Dr. Gray is going to be down there, you certainly should get hold of him to discuss this problem.

DR. STAFFORD WARREN: How are we going to get out of the bind of getting proposals to some point where they can be acted upon, because time is passing pretty rapidly. We have now been nine months, and this long-term thing, which I am much interested in of course, that is time during which the projects could have been tooled up and exposures already made. I hate to see another six months go by and another and another.

DR. TITUS EVANS: Are you suggesting that we empower this committee to consult with other groups and try to get something started?

DR. STAFFORD WARREN: I would like to see proposals turned in to somebody with authority to recommend them to NEPA as a start.

CHAIRMAN DOWDY: The subcommittee will thrash them over.

DR. STAFFORD WARREN: That is probably the simplest way, and then have that subcommittee report to this body. We probably can't meet again within a month or so. Can we come to this committee with some definite proposals within a month, the subcommittee, along the lines that are in these seven topics?

CHAIRMAN DOWDY: That depends on how quickly the subcommittee can act. That is a pretty short time, after looking back over how much trouble it took to get this far.

DR. STAFFORD WARREN: Some of the group have pretty well crystallized ideas about programs.

DR. STONE: You didn't get what you asked for, did you?

CHAIRMAN DOWDY: No. As a matter of fact, as much as we did get hasn't met with very favorable reaction.

DR. NIMS: I think in a general sense couldn't we recommend this evaluation of physical fitness following total body irradiation at 100 r level be done immediately?

DR. FAILLA: On what man?

DR. NIMS: Monkeys, orangutans, chimpanzees.

DR. TITUS EVANS: I might say in that connection that this committee of the National Research Council was interested in radiation of neutrons; that at least one of the members of the committee wanted to start work immediately on monkeys and so on.

We might be able to correlate some of our efforts with those, because monkeys and apes are expensive and hard to take care of, and there are only a few colonies in the country.

So I think that this committee should work with other groups to see if they can't get some data from their work and help them get started

and so on. They are interested in the same thing we are, except the long-term effects of perhaps multiple exposure, but not daily exposures, but accumulative effects on the eye.

DR. ROBLEY EVANS: And not necessarily involving whole body. That is the difference between these two viewpoints, which is quite a point.

DR. STAFFORD WARREN: Failla has quite a bit of work on the eye.

CHAIRMAN DOWDY: The long-term program has been batted around all over the country for the last year and a half, two years, or even further than that; and we have never had any discussion on it at all.

DR. FAILLA: I think you and Dowdy are referring to the same program. Is that right?

DR. STAFFORD WARREN: That is right.

DR. FAILLA: I think the thing to do with that program is for some of us to get together and decide what are the essentials.

CHAIRMAN DOWDY: The way it is set up is a group should investigate it and carefully study the thing.

DR. FAILLA: It is certainly set up in such general terms that you could — it would take a hundred years to really get an answer, and a lot of people working.

CHAIRMAN DOWDY: I think you mentioned that program, Dr. Failla. It was recommended that it be accepted in principle. Once it was accepted in principle, then a group be appointed to work out the details and essentials.

But it never even got that far.

DR. FAILLA: I think they are taking it up now, because I just got a copy of it for an opinion.

CHAIRMAN DOWDY: You read it once before, because you had seen it before.

DR. FAILLA: I saw it a year ago.

CHAIRMAN DOWDY: I have a copy of it here, and it was set up to accept in principle. Once it was accepted in principle, then a group be appointed to define its extent and method of carrying out and what tests would be looked for.

It seems to me we can go no further here until we appoint a subcommittee which would be representative and which would meet as quickly and as fast as possible.

If they are going to wait for us to circularize this committee and then send them proposals, I can tell you it won't be very satisfactory because there are only about four of this committee who sent in any proposals at all.

I would assume that any one of you would be willing to serve on this subcommittee if you were appointed.

DR. FAILLA: It depends on what is involved.

CHAIRMAN DOWDY: Unless there is no further discussion, that winds us up.

DR. STONE: There is one other question I would like to have you discuss. Does this committee wish to make any further recommendations regarding what should be done in human experiments, or leave that entirely to be worked out?

CHAIRMAN DOWDY: I think we would leave that entirely to be worked out. We would get into more trouble with that than we have with this.

DR. STONE: You can't go up to 600 r exposures there. So that would limit you a little bit.

CHAIRMAN DOWDY: I would like to call on Mr. Ward before we disband today for any comments that he might have.

MR. WARD: It is the end of a long day. It is no time for speeches. But I certainly want to compliment the Chairman for the way that this meeting has held to schedule. It is only a quarter after four now, and it was a very difficult schedule, and it has been accomplished.

I think it was quite a remarkable exhibition. I think the contribution of the preparatory work has had a lot to do with the speed with which the committee has had to do today.

I want to mention one or two broad matters. NEPA, at its earliest conception, in defining its relative problems and the difficulties of the relative problems, put a No. 1 on the biological aspects.

It has always been very conscious of the fact that because of the nature of its mission, the biological implications and the proper solution were very grave and would involve additional emphasis in the fields that are already under way in developing experimental background data.

It is again a matter of reiteration that NEPA does not wish to do any work that is competently done anywhere. That is a selfish reason, if no other; because, whereas our funds are in millions, the A.E.C.'s funds are hundreds of millions.

However, we feel, and I don't want to be misinterpreted in this, that perhaps in the A.E.C. itself, as we see it from the outside in, there

has been less done proportionately in these fields than perhaps some of the other fields that A.E.C. is interested in. That is, I am sure, a very amateurish criticism and it is only from the outside looking in.

But if that be the case, and there are some elements of a proper research program that have not been covered, NEPA hopes that it can contribute its funds and its efforts along those particular lines within the limitation of its budget.

It also recognizes, as has been brought out here today, that some of these subjects are so broad that one could see the vista of years stretching on and an almost unlimited number of workers in the field; and NEPA can't wait for that.

Therefore, there is going to have to be some elements of judgment and proper assumptions based upon the best existing information that will have to direct NEPA's own activities even though they may in the later knowledge be changed more or less. NEPA will have to change its work wherever it is based on such assumptions, because, as you have seen here today from the very point brought out by Mr. Simmons, the assumptions that are made by this group are going to actually dictate maybe the size of the airplane, which is the fundamental of the project.

What you gave us at your first meeting was a ray of light in a very dark room, and gave us the courage to make some early assumptions, without which we would have been in greater difficulty than we have; and we have been in great difficulty from time to time.

Now, one more remark, and I think I have covered all that I would like to say, except to agair express appreciation for everything that has been done and for the fact that so much talent has been willing to sit down on these problems, as evidenced by these meetings and the subcommittee meetings; and that is, that while NEPA's mission so far has been purely that of a power plant, some evidences are beginning to accrue that it may have some other problems thrown in with those which make the emphasis on the biological data even more important than we at first assumed.

So I would like to close my remarks on the thought that to us this is a number one phase of the problem.

CHAIRMAN DOWDY: Thank you very much, Mr. Ward.

I would like to ask one other question here in reference to this committee. If this executive committee did end up by having Dr. Failla and Dr. Titus Evans, would it be agreeable that we have this executive committee be the subcommittee and in the meantime have Dr. Robley Evans and Dr. Failla and Dr. Titus Evans hold a conference with the NEPA people, however long a duration it seems necessary prior to the meeting of this committee, so that they will be fully conversant with their problems; and then convene the executive committee?

Would that seem a reasonable approach to those of you here?

MR. SIMMONS: Who will set the date and the time and the place for this get-together?

CHAIRMAN DOWDY: I think that would be something for you people to work out with the two Evans and Dr. Failla. Then, as soon as you have done that, we can set a date for the executive committee; and to that will be officially added Dr. Failla and Dr. Titus Evans.

DR. ROBLEY EVANS: What is it this first meeting is to do?

CHAIRMAN DOWDY: You and Dr. Failla and Dr. Titus Evans have a discussion relative to the physical factors involved with the NEPA people, preferably at their place.

MR. SIMMONS: That is the most convenient. I would also like to have Dr. Anderson. He is one of our people at NEPA.

CHAIRMAN DOWDY: Whatever members of your NEPA group that you want, so that you can agree on the preliminary factors involved. They are all just a part of the same committee, but you people are more conversant with this particular phase of it than the rest of us.

You could thrash out this physical problem that has been bothering us; then convene with the executive committee to formulate research plans.

MR. SIMMONS: Could I suggest that Dr. Failla and the two Drs. Evans agree on a convenient time and date and then notify us when they would like to meet.

DR. FAILLA: How about doing it right now, this afternoon?

MR. SIMMONS: All right.

CHAIRMAN DOWDY: You can decide that. But I thought maybe you might want more time.

MR. SIMMONS: I think we would be a little pressed this afternoon, and I think it is something we ought to be deliberate about.

DR. ROBLEY EVANS: He means get together and arrange the date.

DR. FAILLA: We might get together this afternoon. Some of you fellows might want to get some more information before we leave here.

CHAIRMAN DOWDY: Before we close the general meeting, I would like to ask Admiral Sims if he has any remarks he wants to make.

MR. SIMS: Nothing, Dr. Dowdy, except to express my respect for this committee and its individual members, and to emphasize the words of Mr. Ward, that I think the committee has been very wise in selecting Dr. Dowdy as Chairman.

I hope the next time you select a time and place for a meeting it is not on Sunday.

CHAIRMAN DOWDY: I personally want to thank each and every one of you for the cooperation that you have given us, and we will attempt to arrange a meeting for the executive committee as soon as possible; following this, call a meeting of the general committee.

Is there a motion for adjournment?

DR. FAILLA: I so move.

DR. ROBLEY EVANS: I second it.

CHAIRMAN DOWDY: The motion for adjournment has been made and seconded. The committee is adjourned.

(Whereupon, at 4:20 p.m., the committee meeting adjourned.)