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ATOMIC ENERGY ACT OF 1946

~~SPECIFIC RESTRICTED DATA CLEARANCE REQUIRED~~

NEPA #6 BOARD OF CONSULTANTS MEETING

NEPA 957-BCR-8 (U)

DATE January 28, 1949

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NEPA DIVISION

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NEPA #6 BOARD OF CONSULTANTS MEETING

TIME	9:00 A.M.
DATE	Friday, 28 January 1949
LOCATION	Room 3E-869, The Pentagon, Washington, D. C.

AGENDA

1. Introduction. . . . . .Mr. J. Carlton Ward, Jr.
2. Operations. . . . . .Mr. T. A. Sims
   - (a) The Lexington Project Report
   - (b) Some Remarks about Operations
3. Remarks by the Air Force. . . . . .Colonel M. C. Demler
4. Administration. . . . . .Mr. F. B. Kauffman
   - (a) Status of Facility Construction
   - (b) Some Aspects of Security
5. Technical Presentation of NEPA #6
   - (a) Introduction. . . . . .Mr. Andrew Kalitinsky
   - (b) Reactivity. . . . . .Mr. Kenneth Cooper
   - (c) Discussion
   - (d) Controls. . . . . .Mr. Edward S. Bettis
   - (e) Discussion
6. Open Discussion by Member Companies
7. Closing

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ATOMIC ENERGY ACT 1946
SPECIFIC RESTRICTED DATA CLEARANCE REQUIRED

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ATTENDANCE AT NEPA #6 BOARD OF CONSULTANTS MEETING

Mr. R. A. Anderson, Atomic Energy Commission
Lt. Col. N. C. Appold, Office Deputy Chief of Staff, Materiel
Mr. C. S. Bachle, Continental Aviation and Engineering Corporation
Capt. A. L. Baird, Bureau of Aeronautics
Dr. Charles P. Baker, NEPA Consultant
Mr. Edward S. Bettis, Fairchild - NEPA
Lt. Col. A. W. Betts, Logistics Division, Department of Army
Lt. Commander L. K. Bliss, Bureau of Aeronautics
Capt. C. M. Bolster, Office of Naval Research
Dr. Kenneth A. Browne, NEPA Consultant
Maj. H. F. Bunze, Office Deputy Chief of Staff, Materiel
Capt. William B. Bush, AMC Eng. Field Office, Oak Ridge
Mr. Frank W. Caldwell, United Aircraft Corporation
Mr. J. Frank Coneybear, Fairchild - NEPA
Col. Frank C. Cook, Office Deputy Chief of Staff, Materiel
Mr. Kenneth C. Cooper, Fairchild - NEPA
Mr. C. Coughlin, Fairchild - Washington
Lt. Col. Jay P. Dawley, P&O Division, Department of Army
Col. M. C. Demler, Office Deputy Chief of Staff, Materiel
Mr. Ivan H. Driggs, Bureau of Aeronautics
Dr. C. B. Ellis, Office of Naval Research
Dr. Titus C. Evans, NEPA Consultant
Mr. Fredric Flader, Fredric Flader, Incorporated
Mr. Carl Frederick, Fredric Flader, Incorporated
Col. C. D. Gasser, AMC Eng. Field Office, Oak Ridge
Mr. D. Gerdan, Allison Division - General Motors Corporation
Maj. Donald J. Grant, Air Materiel Command
Dr. L. R. Hafstad, Research & Development Board
Lt. Col. I. J. Harrell, Research & Development Board
Col. V. R. Haugen, Research & Development Board
Col. O. G. Haywood, Office Deputy Chief of Staff, Operations
Capt. W. T. Hines, Bureau of Aeronautics
Com. E. W. Hribar, Military Liaison Committee
Mr. H. M. Hudson, Ace Reporting Company
Capt. J. A. Jordan, Office of Naval Research
Mr. Andrew Kalitinsky, Fairchild - NEPA
Mr. F. B. Kauffman, Fairchild - NEPA
Col. D. J. Keirn, Atomic Energy Commission
Mr. George S. Knopf, Fairchild - NEPA
Dr. Morris Kolodney, NEPA Consultant
Dr. J. A. Kyger, Bureau of Ships
Capt. R. A. Lavender, NEPA Associate Consultant
Mr. Edward G. Littel, Fairchild - NEPA
Mr. R. E. Littell, National Advisory Committee on Aeronautics
Mr. W. G. Lundquist, Wright Aeronautical Corporation
Dr. E. R. Mann, Fairchild - NEPA

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ATTENDANCE AT NEPA #6 BOARD OF CONSULTANTS MEETING

Continued

Dr. W. H. McAdams, NEPA Consultant
Mr. L. C. Merrill, Fairchild - NEPA
Mr. Charles Meyer, Westinghouse Electric Corporation
Dr. A. J. Miller, Fairchild - NEPA
Mr. A. O. Mooneyham, Fairchild - NEPA
Mr. Reeves Morrisson, United Aircraft Corporation
Mr. Lee Ohlinger, Northrop Aircraft Incorporated
Mr. Richard C. Palmer, Fairchild - Washington
Brig. Gen. J. F. Phillips, Research & Development Board
Mr. J. K. Pickard, Military Liaison Committee
Mr. Benjamin Pinkel, National Advisory Committee for Aeronautics
Maj. Gen. E. M. Powers, Office Deputy Chief of Staff, Materiel
Mr. B. R. Prentice, General Electric Company
Brig. Gen. D. L. Putt, Office Deputy Chief of Staff, Materiel
Mr. L. B. Richardson, Fairchild - Washington
Mr. W. C. Rockefeller, NEPA Consultant
Lt. Commander L. H. Roddis, Jr., Bureau of Ships
Maj. Gen. D. M. Schlatter, Military Liaison Committee
Mr. W. B. Seaver, Fairchild - NEPA
Dr. R. F. Selden, National Advisory Committee on Aeronautics
Col. J. W. Sessums, Office Deputy Chief of Staff, Materiel
Com. J. L. Shoenhair, Office of Naval Research
Mr. Carleton Shugg, Atomic Energy Commission
Mr. Gordon Simmons, Jr., Fairchild - NEPA
Mr. T. A. Sims, Fairchild - NEPA
Mr. C. R. Soderberg, NEPA Consultant
Dr. Frank H. Spedding, NEPA Consultant
Maj. J. P. Taylor, Research & Development Board
Lt. Col. H. L. Waesche, Air Provost Marshal
Mr. Charles Wandel, Fairchild - NEPA
Mr. J. Carlton Ward, Fairchild - Washington
Mr. Roger S. Warner, Atomic Energy Commission
Col. Ralph Wassell, Air Materiel Command
Mr. J. W. Webster, Fairchild - NEPA
Dr. G. L. Weil, Atomic Energy Commission
Rear Adm. C. D. Wheelock, Bureau of Ships
Brig. Gen. R. C. Wilson, Military Liaison Committee
Mr. G. B. Woods, Office of the Under Secretary of the AF

P R O C E E D I N G S

CHAIRMAN WARD: Gentlemen, if you will all come to order, we will start the meeting. We have waited about ten minutes hoping that some of the stragglers who have been caught in this bad weather would arrive.

If you can't hear very well in here, I wish you would signal this table, because we have around the table with us members of the Board of Consultants. I think we have two that are absent.

From time to time as questions may be asked and answers given, it may not be too easy to hear in the back of the room. It would be a great favor if you would signal us. I will ask everyone to speak as loudly and clearly as he can and not to speak up at this end of the room, but so to speak that his voice can be heard at the rear. This is our first experience in this room, and I don't know the acoustics too well.

In starting off the meeting, I would like to review a little bit some of the milestones of last year, and in some degree establish the platform on which we are standing at the moment as far as the NEPA Project is concerned.

A number of things happened last year that are quite significant, and I think it might be well first to review our own activities. The best way to do that is to enumerate the meetings that have been held and the objects of those meetings, which brings us really to Report No. 6, the one that you have today.

In January of last year we issued Report No. 5. Those reports, up until Report No. 6, have always been issued in two forms, one form that has been cleared for Board of Consultants membership, and that has never been the complete report.

Report No. 6, therefore, is a milestone because for the first time all of the material will be found in it and all of the consultant member companies now have access to the full information.

We consider this very important, and we have felt that it has been a handicap for the consulting companies not to have access to the full material.

In April we had -- I will call it -- a refresher seminar on the basic science underlying the needs of this project. That was a very successful meeting. It served to bring some new people into the later information that is of concern in this project, and I think it polished up a little bit some of those of us who were in need of another injection.

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In June we had our symposium on materials, particularly for high temperature use. The report on that symposium, I think, contained a great deal of material that is of interest outside of this project and may have been sort of a gathering instrument for putting in one place all of the material on this subject.

In August we had a meeting of the consultant companies, which was not a regular Board of Consultants meeting. It was held in a sense in lieu thereof because we were not able at that time to present Report No. 6. It was not organized in sufficient fashion.

Although our original intention had been to hold a meeting at that time, for reasons that I will touch on in a moment or two it seemed inadvisable, and instead the consultant members were brought to Oak Ridge in place of a meeting here, and were shown the progress of the project.

In other words, they were briefed much as we had briefed the members of the Parker Committee, the Advisory Committee to the Atomic Energy Commission, on industrial uses of atomic energy, and as we briefed the groups from the Lexington Project.

A very interesting attempt was made at that meeting to find out, if we could, what sort of a time scale we were working on for the completion of the project; and each associated company of the Board of Consultant group was asked to form its own opinion of a probable scale of accomplishment in point of time, based on information they were furnished at the meeting.

The study of those estimates is illuminating and interesting. They were presented as a maximum and a minimum, with a sort of a statistical mean of the composite opinions of all the companies.

It is interesting to see those companies that have, shall we say, a relatively dim view; and those companies that had a very bright view of the situation, the spread between them, and then what would be the statistical mean.

It was in a sense a mental exercise in crystal ball gazing, with such experience as each company could contribute from its own attempts to build apparatus on which much scientific information had to be derived as we went along.

In December we had the symposium on radiation effects on material. That, I feel, was also a very interesting contribution and represented, as far as we could get it, all of the up-to-date information on that little-known subject.

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Those were the major formal activities of last year; and while we had only one official Board of Consultants meeting of the character we are having today, as you can see, there were a great many activities, and they take a great deal of preparation.

Along with all these meetings, the NEPA group had some very serious work to do that was not, in a sense, of the normal constructive work of planning and development, which was brought about by the briefing activities that were necessary and reports that were needed for the Lexington Project and for similar activities; although the Lexington Project was outstandingly the one that needed the most thought given to it and the most preparation.

If you will mentally, now, go over this list of meetings, symposia, and the like, you can get some idea of the call on the time, attention, and effort of the NEPA science groups, engineering groups, in order to prepare for this array of meetings.

That brings us to today's meeting as far as that phase of the work is concerned; and I again would like to mention that that Report No. 6 is a complete, unabridged report on the subject presented in it.

The reason I have emphasized that twice is because our Board of Consultant members, these companies, have been operating under a considerable handicap up until now. There are other handicaps besides the one I mentioned. A particular one has been the fact that these Board of Consultant contracts for each particular company have been a long while in negotiation. All of them are now completed but two, and one of those will be signed within a week. The other one, we think, ought to be signed within the next two weeks, let us say.

That one company is having some difficulty on a matter of policy. It is a large company, and I think it is clearing through the various echelons of that large organization.

Except for that one company, or these two, they were not permitted the full and unabridged report. The rest have now had it and had an opportunity to study it. We cannot give that information until the contracts are signed.

Therefore, it is a matter of considerable importance to the project as well as, I would hope, to those companies that this formal signing occur as quickly as they can effect it through their own organizations.

That places the Board of Consultant companies for the first time now in the inner circle. With the signed contracts and the full information, I have only one other thought, and I am really speaking to the consultant companies at the moment.

That is the desirability of their sending full-time working personnel to the project for educational purposes and in order to better integrate themselves into the operation of the project.

It should be obvious to them and to everyone present that it is like all activities of a group nature. You get out of them what you put into them. To read the report is no doubt stimulating and interesting, and to carry out some of the specific subcontracts on the specialized phases of the work that many of these companies have taken on is also interesting. But the only way of really getting into the heart of this subject and throwing their full weight into it is to jump right in and get their feet wet.

The way to do that is to have people working on the project that will ultimately, at their convenience, go back to those companies and carry forward from there.

I would like also in this little brief introduction to mention a few of the accomplishments that are definite enough to be worthy of mention. I have tried to put down only what seemed to us like significant contributions.

I think one of the first ones to be mentioned is our attempt to attack the problem of what happens to lubricants under irradiation. We had surprising results on that subject, as many of you know.

It was very clear that many of the usual lubricants are unsuited to operating in any radiation area. They either jell or semi-solidify or become viscous and have little value as a lubricant.

It could have been a very serious problem. However, we have gone far enough into it, that is, one of our subcontractors, the California Development Corporation, that we now are informed by them that lubricants can be put together that can operate under these conditions and retain their lubrication properties. We think that is a definite contribution.

We also attacked in a fairly early stage the question of what will happen to electronic elements subjected to radiation. While our work is not conclusive in that area, it is rather heartening; and we feel that it also is a contribution.

As far as we know, and as far as our experiments could determine, it is going to be possible to expose electronic elements in radiation areas and have them function properly. I think much more work has to be done on it, but we are not in an area of total ignorance on that subject today.

The next one is, in my opinion, a most important one. Yet you can't call it an accomplishment, and yet you can.

We were naive enough in May 1946 and in subsequent meetings to assume that we could establish a practical working combination of engine and reactor by using a conventional turbo-jet engine which we thought we could simply extract from what was then the state of industry development in the turbo-jet field.

Last year it became extremely clear that the engineering phase of the project is as difficult as, let us say, the nuclear phase, for want of a better word. It is not a strict definition, because there is nuclear engineering just as there is power plant engineering. It seems easier to describe the power plant side as the engineering phase because it is an art that has extended to such a degree from the scientific information that it is largely an applied art.

We now find that the engine portion of this project, the power plant portion of the project, is dictated by the nuclear considerations, and vice versa. The nuclear considerations are dictated by the limitations of the engine.

That becomes readily apparent when you consider the conditions under which an aircraft power plant must operate, the mention of getting the heat out of the nuclear process and into the engine cycle.

The inter-relation will be clearly demonstrated, I think, later in this meeting, when you see some of the studies that have been made on the parameters that dictate the type of nuclear reactors which we shall probably concentrate on.

So, in summarizing, I would say that we now recognize, and our Air Force sponsors recognize, that the problem of engine development is of equal magnitude to the problem of the nuclear reactor development.

I think it fair also to mention one other characteristic, and that is that the jet engine will undoubtedly be required to have, and will have, a much higher compression ratio than has been conceived necessary for chemical-fueled engines. The question of supercharging, therefore, looms very large.

Whether that supercharger will be of conventional types, or whether it will be of supersonic type is yet to be determined. Each one offers a possible solution. It seems wise, therefore, not to cross that bridge today but to attempt to follow through on both of those approaches.

I call that a milestone of accomplishment, because you will see the sharp change in our thinking; and that sharp change was brought about by knowledge as opposed to assumptions and ignorance in 1946.

The next one has to do with the control systems. It is easy to exaggerate; and there is a temptation to understate, to avoid exaggeration.

One of the first problems we could deal with, and hence one of the first ones we could do something about -- and I am referring not to paper studies at the moment; I am referring to actually physically doing something about it -- was the question of a control of a nuclear powered engine for aircraft propulsion.

Those of you who are acquainted with the demands of aeronautical equipment recognize, of course, that power levels have got to be changed with reasonable promptness and over wide ranges, under takeoff conditions and perhaps under certain conditions of flight; that the question of controlling a reactor with its inherent nuclear properties is not a simple one; and mathematical analyses show that there were some very involved requirements in these controls that made it extremely difficult, if not impossible, to use the type of control one would have on an experimental pile, and which also meant that one should not burden the pilot or the nuclear engineer, whatever he may be, with too much drain on his mental capacity to anticipate the peculiarities of nuclear reactions under changing power levels.

Therefore, a simulator was made using electronic elements which was supposed to have those characteristics that a pile would have. That was carefully studied, both for its poisoning effects and for the various delayed neutron families.

It was found that one could have a practical control that was no more difficult to operate than the throttle on a conventional engine and required no knowledge of what was actually happening in this complex process.

Those simulators were built electronically because it didn't take extensive laboratories, which we at that time did not possess. Therefore, that phase of our work went well along, so that we feel today that the control problem is not going to be one of our most severe headaches.

It can be solved, and we think we have studied the characteristics enough to feel quite confident in that statement. So we call that an accomplishment.

Now we have another problem, and it is clear that the thermal lags of the whole system from fission to power for propulsion are considerable. They are considerable in conventional jet engines and were one of the early worries in conventional jet engines.

But when you take the whole system from nuclear energy to propulsion, it is clear that the thermal lags are an appreciable factor.

So, along with them, thermal stresses are an important consideration. They have been under theoretical study, and we have now had certain simulators made, thermal simulators, if you like, that have indicated to us the thermal stress problem does not seem now to be as formidable as it appeared to us on theoretical study.

We are in the process now of constructing additional thermal simulators that we hope will do for us what the electronic control simulators have already accomplished. I have listed that as an accomplishment of last year, although, as you can detect, it is not a completed one.

You will see today the engineering approach to this problem explained to you with graphs and charts. I think we have made a very major contribution in this field of power applications of nuclear energy. I hope you will be struck with the logic of this approach and its simplicity as forcefully as have we.

We did not think there was anything very unusual about it when we started because it is a typical engineering approach, to try to visualize the effect of one parameter on all of the inter-related parameters. The degree of uranium enrichment, for instance, dictates the size of the airplane.

So one might say that it is not feasible in this project to study uranium enrichment in a reactor unless you know exactly what will happen to the size of the airplane and hence its tactical strategic desirability.

I throw that one out because it is so striking. On the one hand you have economy of uranium use, and on the other hand you have the end use of the entire project, the ability to deliver a bomb with the greatest certainty, and in that case a wide difference in the character of the over-all vehicle with its handling problems and its man-hours problems and many other problems that the military would be very quick to recognize.

The charts will explain to you how we can visually represent what happens to a reactor design when you vary any one of these parameters such as uranium enrichment. You will see the effect on size and hence on shielding weights and hence on airplane construction in a very simple fashion.

We hope that this technique will become widely used in the design of reactors in other fields, because it is as applicable to any field as it has been to this rather complex one in which we are working. So I have listed that as a major accomplishment.

[redacted]

The last one I would like to name is not a last year's accomplishment and it isn't an accomplishment in one sense, except probably a realization of the kind of a project we are engaged in and an effort to do something about it.

That has to do with, shall we call it, health physics or radiology or bio-physics. It is all of those fields, as a matter of fact. And, as you know, our contract requires that we also delve into the problem of servicing these aircraft. In other words, the broad problem that the military will be faced with when they attempt, if they attempt, as they attempt, to use airplanes in which there are nuclear reactors.

The health physics problems are complex. We believe that since it is a military use, since the purpose of the project is the security of the nation, that it will force by sheer weight of need a much cleaner realization of the need for greater knowledge in the biological, bio-physical fields in the effect of radiation on humans.

Therefore, what this project will generate will undoubtedly be of the widest interest to all phases of the atomic energy work. Probably the reason we will force it is because our need is among the greatest.

The NEPS Project will have a similar problem, but they will have some advantages we do not possess. They have many of the difficulties that we have, but they will have a little more latitude in meeting them.

In an effort to do something about this problem, we organized an advisory group, which we think contains -- at least we are told it contains -- the most forward thinkers in these broad fields.

I would like to name these gentlemen to you for those of you who may not be familiar with this, to me, extraordinary panel.

Dr. Dowdy of the University of California, Los Angeles, is the chairman, and was, in a sense, shanghaied into that job by Dr. Stafford Warren, who is one of the members of this panel and is his superior in the University. It is a very fine choice because of the work that he had done at Rochester and because of his vigor and knowledge of this field in which he is greatly respected.

Along with him there is Friedell, who comes from the Cleveland University Hospital; Zirkle, who is the head of the Biological and Biophysical Institute of the University of Chicago; Cantril, who comes from the Tumor Hospital in Seattle; Stone, of the University of California at Berkeley; Stafford Warren, whom I have already mentioned; Titus Evans of the University of Iowa; Roblee Evans of M.I.T.; Failla of Columbia; Newell of Stanford University; Hamilton of California; and the latest addition is Donaldson of Seattle.

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As far as we have been able to observe in meeting with these gentlemen, this panel has represented on it a cross-section of practically all of the information or knowledge of where to get all of the information in this very complicated field. They have already accomplished for us a tremendous amount of useful work.

In the very first meeting, that panel examined the tolerances which have to be of necessity set up for the operation of the MED Project, and it was pointed out the military purpose of this project.

Questions were asked, such as, what could be a one shot tolerance for a human being if the necessity occurred for the accomplishment of a military mission? What is the effect of accumulated dosage versus recovery by rest periods between dosages?

Many questions of that type were asked, and from it we were furnished information which simplifies the design of our entire project. We call that one of the accomplishments of last year.

I think then, if I have been at all clear, I brought you through the steps of last year in which we shared what knowledge we had with our consultants and affiliated groups, and those things which we feel have advanced far enough along to not be called paper stages.

Now I would like to talk of developments which have a vast effect upon us as a project, but which are not of our doing directly at all.

There were two that I think should go in the forefront of that. One was the formation of the Lexington Group and the consequent issuance of the Whitman Report. Without attempting to discuss that report, which will come out more later, I think the only mention I should make of it is that it clearly states that after an examination by, as you know, a very fine group of technicians and scientists, the project is feasible.

That seemed to be highly controversial until this group -- maybe it still is, in certain areas -- made its examination; and we feel that their findings, therefore, have a terrific effect upon the project.

The other report which I think should be mentioned is the Parker Report. As I have already said, the Parker Committee was an advisory committee picked by the Atomic Energy Commission to advise with them on that subject of how to bring industry and how to bring industrial uses of atomic energy into being insofar as taking an active part in atomic energy work.

There are many findings in their report which we think parallel our philosophies and therefore strengthen our points of view.

We feel, therefore, that those two happenings of last year have, through no fault of ours, a very important bearing on the project.

[redacted]

I would like also to bring in the thought of the community of thinking and parallel necessity of many steps that each of us have to take between the NEPS Project and the NEPA Project. If you look at it philosophically and analytically, you find that there is a great deal in common.

Each one is a military project dictated by the desirability of a superior solution to a military problem; in other words, the security of the nation in a very direct fashion. Each one is a portable power plant deriving its energy from nuclear fission. In each one men must be intimately associated in a confined space with the power plant. Each one operates under a flotation principle, which means center of gravity; metacenter for a ship and aerodynamic center for an airplane. It has to bear a relation to these very heavy concentrated loads, such as the shielding and the nuclear reactor comprise, and therefore dictates to a certain degree the design of the vehicle, or at least has a very severe impact on the design of the vehicle.

Each of the projects requires a good knowledge of the danger limitations to human beings operating in close confinement with these fission processes under those conditions where gravity is not supporting any given amount of shielding that one might wish to throw around as a safeguard. In other words, there is a penalty for shielding in each of these two cases.

It is a matter of degree in certain cases between them, but there is a strange similarity in these two projects. It is a feeling on the part of NEPA, which I think is shared in by many in NEPS, that each of these projects should yield important information for the other, and that there should be a very parallel tight and close-working relationship and interchange of information and full knowledge on the part of each from what is derived on the strength of the other's activities.

I think I have arrived at the end of my opening comments, and I think we should now go directly to the question of the agenda, which I think you have all been furnished.

Before we take up the formal part of the meeting, and the real part of the meeting, I would like to welcome so many people who have come to this meeting from various military, A.E.C., and science areas; and I would like to repeat again for those who may have come in late, if there is any difficulty in hearing in the rear, I wish you would signal the chair and General Sims or I will endeavor to see that the speakers address themselves to you.

I think it is now your turn, General.

MR. SIMS: Undoubtedly one of the most significant events made by NEPA during the year was the Lexington Project evaluation.

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You will recall in our formal Board of Consultants meeting held in this very room a year ago, Major General Craigie briefed us on the contents of important correspondence between the Air Forces, the Military Liaison Committee, Research and Development Board, and the Atomic Energy Commission.

He read us General Spaatz' letter of 30 December 1947, the letter which set forth the urgent military need of a very strong national effort devoted towards the solution of nuclear-powered aircraft.

In the closing paragraphs of this letter, the Air Force invited the Commission to examine the NEPA program in detail and to develop a plan for the effective employment of national effort and facilities to develop a practical aircraft engine utilizing nuclear fuel.

The Commission gave considerable thought to this evaluation. There were so many technical facets to the over-all problem that it was difficult to assemble in any one group, outside of NEPA itself and its own team of technical consultants, the experts who among them possessed the knowledge not only in the physical sciences, but the practical engineering aspects as well of design of aircraft, engine design, thermodynamics, metallurgy, materials, controls, reactivity, and so forth.

I think the Commission made a fine choice in the selection of M.I.T. to perform this survey. Under the leadership of Dr. Walter G. Whitman, head of their Department of Chemical Engineering, M.I.T., it assembled a group of 45 professional members. Twenty-two of these gentlemen were from M.I.T. itself. The remainder was drawn from various universities, institutes, and commercial organizations outside the boundaries of M.I.T.

They started work early in June, and they visited us on June 3, 4, and 5. I think we had to release two of our consultants for this work.

Their report was submitted to the A.E.C. on 30 September 1948, on the date it was promised. Looking around the room, I see the faces of many people who have read the Lexington Report. Our member companies, however, have just received their copy of it within the past two weeks.

I would like to review, particularly for their benefit, the comments that we had about the report. We consider them preliminary comments, because even at this writing we haven't received all of the various attachments to the report.

For example, there are some 77 LP reports that are attached to the main Lexington Report. I think we have received some 60 of them in our library now. There are 17 more. We also have the appendices to look at and examine, some of the cycle calculations and assumptions we wanted to look at.

[redacted]

So I do consider these comments rather preliminary, preliminary in the sense that we haven't studied the total report in its entirety.

We feel that the Lexington Report is a very comprehensive survey of the factors involved in nuclear-powered flight and contains definite conclusions as to feasibility.

We are pleased to note that the group reached several interesting conclusions which I now quote:

> (a) "Although success cannot be guaranteed, there is a strong probability that some version of nuclear-powered flight can be achieved if adequate resources and competent manpower are put to the development."
>
> (b) "Nuclear-powered aircraft with practically unlimited range capabilities at desired speeds could probably meet the long-range reconnaissance and bombing requirements of the U. S. Air Force better than could chemically-powered aircraft if the missions are to be accomplished with a crew."
>
> (c) "Integration of work on reactors, power plants, materials, and other components is essential for efficient progress towards this goal."
>
> (d) "A vigorous and realistic aircraft reactor development program during the next few years should contribute to, and benefit from, other aspects of the reactor development program of the Atomic Energy Commission."

These conclusions confirm the confidence that NEPA has had and now has in the successful solution of the project; and confirms NEPA's realization of the necessity for coordination and cooperation by the several Government departments and agencies involved.

The report should be of considerable value to the Government now for it contains a detailed study by an independent group of the most complicated and exacting problems in a highly technical field where many factual data are not now known.

The report is timely in that the progress made by your contractor has reached the point where the success of the NEPA Project has become increasingly dependent upon a more extensive use of the facilities of the Atomic Energy Commission. -- Its assistance in the accumulation of technical information in areas not fully explored, -- its assistance in the strengthening of the top scientific leadership of NEPA without disturbing the excellent engineering, metallurgical, electronics and

technical service staffs that have been built up, -- and the construction by A.E.C. of new facilities such as high-flux reactors and their operation to meet some of the research phases of the project, particularly in the field of shielding and materials.

But, as has been so well stated recently by Dr. Bacher, Commissioner to the A.E.C., the development of a nuclear-powered aircraft is primarily a technical and engineering problem, not a scientific one.

The report served to crystallize the immediate problems of the project, problems which should be probed fully in round-table discussion with top policy-making officials of the Air Forces, the Navy, the Atomic Energy Commission, and your contractor.

The report analyzes the major engineering development problems and major research problems of the project and presents certain conclusions. While it is generally realized that application of nuclear energy to the propulsion of aircraft will be difficult to achieve, by the same token the rewards to national security of the successful development will be very great.

NEPA agrees in general with the findings and conclusions of the report, although there are variations in views between the Lexington Group and your contractor regarding emphasis and programming.

While it is not intended to discuss in detail all of these variations, our present comment with respect to several of the more important ones might be in order.

With respect to the capabilities of chemically fueled aircraft, the RAND curve of range versus speed, Figure II-1, is in fact an envelope curve based on theoretical aerodynamic bodies, and is useful only to show trends. A plot of what is possible in practical airplane design would fall considerably below the RAND curve shown, and would be more nearly consistent with the N.A.C.A. figures contained in Table II-1, as amended by somewhat higher estimates for the turbo-props (witness the Boeing estimates on the B-52 as 9000 miles range at speeds up to 500 mph). In LP-63, one of the reference documents to the main Lexington Report, North American is quoted as stating their belief that maximum ranges as dictated by current engineering limitations are of the order of 5/8 of RAND's theoretical estimates. It will be noted that no matter how optimistic the estimates as to expected future performances of long-range aircraft powered by chemical fuels may be, these performances fall considerably short of the desired attainments as to range and speed listed as requirements by the U. S. Air Force and the Bureau of Aeronautics on page II-3 of the Report. It is in the combination of high speed with very long range where atomic propulsion promises to achieve results which appear unobtainable by any other means.

It will be noted also that no matter how optimistic the estimates of what is to be expected in the way of performances of long-range aircraft powered by chemical fuels may be, these performances do fall considerably short of the desired attainments in range and speed listed as requirements by the U. S. Air Force and the Bureau of Aeronautics on another page of that report.

It is in the combination of high speed with very long ranges where atomic propulsion promises to achieve results which appear unattainable by other means.

Conclusion 2, Chapter 1, Page 1 of the report, states that:

> "A nuclear-powered aircraft of extreme range, that is, around-the-world and with a speed approaching the velocity of sound, probably 600 miles an hour, seems achievable in the light of present information; although it is quite conceivable that these characteristics can be improved by developments under way or not yet foreseen."

NEPA agrees with this conclusion. Elsewhere in the report it is stated that supersonic flight is impractical for a long time to come.

The enormously greater tactical advantages of supersonic speed require the conclusion on this point to be reserved until further technical data become available.

We feel that it would be unwise at this stage of the project to limit the thinking to the boundaries of existing aerodynamic knowledge, and no decision should be made to prevent the engine that NEPA is to study and develop from ever being utilized in supersonic aircraft.

The requirements people on the air staff representing the operational point of view definitely list supersonic strategic bombers and reconnaissance aircraft as highly desired attainments, and we believe that a determined effort should therefore be made to give the operational people what they want.

The report emphasizes the possibilities of the tug-tow arrangement. The idea is good. NEPA has had this arrangement under investigation for some time.

The towed aircraft system is essentially an expedient for reducing the weight of a shield required by an inhabited nuclear-powered aircraft, and as such, towing is in a way an evasion of the shielding problem.

An analysis of the advantages and disadvantages of the tug-tow has been carried on at length in the NEPA Project and will be continued. From a long-range viewpoint, NEPA feels that it would be unwise to put too

much reliance on it now, since some good solutions to many of the problems may be missed simply by lack of effort devoted to them. A more desirable single airplane may not materialize as a result.

NEPA feels that the tug-tow arrangement should be kept as a last resort. It should only be relied on in the event it is found impossible to construct an adequately shielded airplane powered by nuclear energy.

It is felt that the advantages of the tug-tow system cannot be determined until a limiting shield weight of the more desirable single airplane has been more definitely established.

Meanwhile it is contemplated that sufficient analysis work will be carried on with respect to the tug-tow system to form a reasonable basis of comparison with the shielded inhabited aircraft. Further studies may indicate the desirability of a flight test program using existing designs of aircraft to obtain an appraisal of the control and other problems.

We understand, from talking to the Lexington people, that Lexington thought if you could adopt the tug-tow system now, you could get it in the air sooner than say the inhabited aircraft. I would agree with that. However, we have been oriented by the point of view that we want to get a test stand sample engine running on the ground first rather than to try to fly it.

I think running one of these things in the air is of lesser importance than running it on the ground first for several thousands of hours. You don't have a practical aircraft engine until it has been operated successfully for several thousand hours on a ground test stand. You just have a drawing and a bag of bolts.

Shielding. NEPA agrees with the Lexington Project that the values which were selected as shielding constants and shielding weights, and hence gross weights of the airplane are proper and logical for the purposes of this report.

On the other hand, it is appropriate to point out that these values are conservative, and with technical development and reasonable success, they should be bettered.

The weight of safe shielding has not been determined with the proper degree of accuracy. The shield design, together with reactor size, are the factors which determine the size of the airplanes and the necessity of remote control, and hence the type and true military value of the end product.

Modification of the present shielding research program should accelerate progress, and that is certainly desirable. In essence it consists of

two phases, the first of which involves bulk shielding tests on composite shield arrangements presently being conducted on the pile facilities of the Oak Ridge National Laboratory. The other phase involves theoretical calculations, and these require certain basic nuclear measurements not now available.

Present indications are that the bulk tests of shielding assemblies will require a much more elaborate experimental setup than is available in the Oak Ridge National Laboratory. It may be necessary to have a source of nuclear radiation of approximate equivalent dimensions and spectrums as the proposed aircraft reactor. Reorientation of the shielding program should involve considerable emphasis on the differential measurements previously described and an improved facility of carrying out bulk testing of shield configurations.

The Lexington Report dealt at great length with the selection and types of cyles, open or closed, and types of power units, and seemed to favor the binary fluid, bismuth, closed cycles, turbo-jet. NEPA has devoted more than two years of directed and concentrated study and analysis of this important phase of the project. It has supplemented its studies by a limited amount of experimental investigation and has come to the conclusion that the open cycle system is the more desirable from the aircraft application and the feasibility standpoints.

In coming to this conclusion, NEPA has had the benefit of continued liaison and consultation with the Air Force and Navy Department and the leading industrial aircraft engine manufacturers. NEPA, therefore, has perhaps given more weight to vulnerability to failure and to attack as a primary factor than has Lexington.

Such a divergence of conclusions as exist may be expected in considering a project such as NEPA. This divergence in conclusions may very well be worked out by a round-the-table discussion of the interested parties.

However, this divergence in conclusions has raised important policy questions, as the Lexington Project Report implies that a paper program of cycle and power unit evaluation should be undertaken for an approximate period of three to five years, with a diffuse materials program is a major experimental activity aside from shielding.

NEPA feels that the time has now come in the interest of economy and progress to lay down an experimental reactor of one of the two types as its primary objective. If an agreement cannot be reached in the immediate future as to the type to be laid down, experimental reactors of both open and closed systems must be carried an appreciable distance into the experimental phase.

A parallel development of all possible avenues of approach would be prohibitive as to cost. It seems imperative that there should be

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adopted as a clearly defined objective the laying down of the most desirable type from the user's point of view and concentrate and work towards that end. A change in that objective may have to be made, and we fully realize it might have to be made, but we feel that faster progress is to be expected from such a procedure.

With regard to the desirability of including further closed cycle studies and some experimentation in the program, it is planned to investigate the possibilities of some of the closed cycle systems referred to in the Lexington Report. These studies will involve the checking of certain basic assumptions regarding the shielding and the heat exchanger, and the thermodynamics of the closed cycle reactor applied to the turbo-jet engine.

With regard to further analysis work, NEPA subscribes fully to the view expressed on Page 2, Chapter 1 of the Lexington Project Report:

> "Insofar as careful analysis and preliminary study can clarify relative merits and probable feasibility of various choices, such studies should be carried out as an important measure of economy."

NEPA would agree with the writers of the Lexington Report that reactor materials development, combined with ingenuity in reactor designs, is a most critical need of the program. NEPA feels, however, that inadequate recognition has been given to the work in these areas that has been conducted directly by the NEPA technical staff and through the medium of contracts with such outstanding research organizations as the Battelle Institute, Fansteel, A. O. Smith, Armour, University of Minnesota, Northrop, Metal Hydrides, University of Michigan, Iowa State, Bureau of Standards, and so on.

The present NEPA materials program is directed towards an exploration of high-fired refractory materials for use in open cycle turbo-jet plants. To experimentally investigate materials suitable for closed cycle systems would involve different reactor moderator matrix combinations, different heat exchanger alloys, and research in many other fields not presently included in the materials program. The present allocation of funds for NEPA will not cover a program of this broader and more diffuse scope.

While we are on the subject of materials, just last week we received one of the classified reports of the Atomic Energy Commission written by Dr. Geller, Chief of the Ceramic Section of the Bureau of Standards. He was one of the consultants to the Lexington Group.

This report contained a comprehensive survey of what had been done and what is being done in the United States in the field of ceramics. I was gratified to find that NEPA received very prominent mention in a

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number of places in that report, not only for the work they have done in the laboratories, but the work our subcontractors have done.

I was particularly attracted to one part of the report. Dr. Geller was describing the early work of the Daniels pile group in the Oak Ridge National Laboratory. He goes on to say that the idea was dropped early in 1947, apparently in favor of fundamental background research. Then may I quote Dr. Geller:

> "The impression was gained that this fundamental work is not supplying a foundation upon which to engineer a pile, but is unearthing more and more reasons for assuming that no presently-known materials are suitable for reactors in high-temperature piles."

Dr. Geller goes on to record the significant progress made in other places, and may I quote him again:

> "It is believed that available information from beryllium oxide, beryllium carbide, and graphite, per se, warrant their use in experimental power piles. They may fail, but it is reasonably certain that their ability or inability to function will be proven only by actual trial. The cost in such experimentation is admittedly great, but the cost of abstract research is also great and is also time consuming."

Returning to the Lexington Report, all of these various aspects of cycle selection, materials, shielding, can profit by close liaison with the work being conducted in Argonne, General Electric, Westinghouse, and elsewhere for the Navy NEPS Project and for stationary power applications. Although the end product is different, these several projects can complement and supplement one another by an interchange of basic knowledge on reactor design, reactor heat transfer, materials, chemistry, bio-physics, and so forth. The very existence of these other projects in this nuclear power field improves the chances of successful application to aircraft.

Estimates in the report as to the time and money have been noted. One brief conclusion stated that it would cost about a billion dollars and take fifteen years. There is no supporting data for that conclusion and no foundation or assumptions upon which to form any basis for comment. Other experts have guessed twelve years and half that sum of money.

Obviously, most cost estimates include major expenditures for facilities which may well be said to automatically become part of the fundamental resources of our nation in exploiting the use of atomic energy in wide fields for this generation and generations to come. Undoubtedly they may have great influence on the economic pattern of our country far

beyond the special military need from which our project stems. These are considerations of the broadest nature and cannot be separated in any simple manner from the necessities of the project itself, but they are extremely large factors in the over-all cost of accomplishment.

One of the important decisions to be made at the present time is the designing and building of high flux reactors and making them available for the needed tests. I am happy to report that the Atomic Energy Commission is preparing to construct the high flux materials research piles from appropriations now under consideration in the Congress.

It is considered most essential that full and effective cooperation and coordination of efforts be made by all the agencies and by all the organizations involved in this important project. The Congressional Aviation Policy Board emphasized the necessity of cooperation between the departments and agencies of the Government in the early solution of nuclear powered flight.

The Finletter Committee stressed the necessity for cooperation and solution of this problem.

General Spaatz in his letter of 30 December stressed the urgent necessity of it from the military point of view.

It is interesting to note that the Lexington Group has so clearly and adequately set forth the same strategic concept that General Spaatz and the other authorities had in mind when they reached these conclusions. They might well be quoted. On Page 2, Chapter 2 of the Lexington Report, it is stated;

> "If war comes, a requirement for strategic aircraft will be to drop strategic bombs on well-protected industrial centers deep in the heart of the Eurasian Continent. These industrial centers are within range of chemically propelled aircraft currently available to the air forces only if the aircraft are operating from bases in Europe, North Africa, or Asia. However, a situation must be assumed where the U. S. Air Force cannot operate from such bases. Thus, the Air Force believes it vital to prepare to operate against Russia from bases within the United States.
>
> "This requires aircraft with extremely long-range capabilities, up to 15,000 statute miles for the round trip, plus the extra range required for emergencies. The last thousand to two thousand miles to and from the target must be flown over enemy territory, thus requiring high speed performance for a considerable length of time."

No matter how large our stockpile of atomic bombs may be, it would be tragic if these bombs remain undelivered over the targets where they would damage the enemy's war-making capacity to the utmost. This could happen if our present limited number of bases outside the country were overrun by the initial overt act of the enemy.

The country that possesses an assured means of delivering the A-bomb from well-protected and easily supplied bases within its own territory to the vital strategic target complex of the enemy, no matter where on the face of the globe these targets may be located, would have little to fear from any potential enemy. Thus, the means of delivering the bomb becomes as important as the atomic bomb itself.

NEPA is deeply conscious of the obligations of modern technology to provide such a weapon, which, if it existed, might effectively restrain serious thought of aggression against our country. The existence of this weapon could thus be a positive force for world peace.

It is submitted that the potentialities of nuclear powered aircraft having, and I quote from the Lexington Report, "an extreme range, e.g., around the world, and capable of operating at extreme high speeds, a speed approaching the velocity of sound, probably 600 miles an hour, although it is quite conceivable that this can be improved by developments now under way or not yet foreseen," are so attractive as to merit a strong national effort towards reaching this goal.

Substantial benefits have accrued to the NEPA Project as the result of cooperation and teamwork between your prime contractor, our consultants and member companies and their subcontractors. Valuable contributions to the success of the project have stemmed from the combined thinking of these individuals and these industrial units.

With a favorable indorsement by the Lexington Group respecting the program for nuclear-powered aircraft, it is contemplated that a more extensive part will now be played by the Atomic Energy Commission. This same teamwork will henceforth be required among the armed services, from participating industrial units, the Atomic Energy Commission, in a common effort to solve the many problems involved in the early accomplishment of an engine for nuclear-powered flight. This concludes my remarks at the present time. Thank you.

CHAIRMAN WARD: This might be a good place to break off for about five or ten minutes and then pick right up where we left off.

(A short recess)

CHAIRMAN WARD: All right, gentlemen. We will pick up and Mr. Sims will continue his remarks. Then if General Putt is not back from on the Hill, Colonel Demler has kindly consented to pinch-hit for him.

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General Sims.

MR. SIMS: While you were gone I put the Brequet range equation on the board. This equation shows that range of aircraft is equal to a constant (depending on the system of units we use, miles per hour, feet per second, or what not) times the L/D of the airplane, which is the aerodynamic efficiency of it, times a factor representative of the over-all efficiency of the propulsive system and times E_f which represents the specific energy of the fuel, times a factor which is representative of the structural efficiency of the airplane.

I put this on to represent the various factors involved. Of course work is under way throughout the entire aircraft industry and the NACA to improve all of these three factors, and significant improvements will materialize some day. Just in the L/D alone, it is very good in the subsonic ranges. You might get 15 to 18 today by clean designs.

It is not very good in the supersonic ranges right now. But that will improve. The NACA gives us forecasts up to 10 or so; and Mr. Bob Woods, who is the chief engineer of Bell Aircraft, maker of the Bell XF-1, predicts that some day designers will be using as high as 12 in the supersonic ranges of L/D.

This factor, propulsive efficiency, doesn't change much from year to year. It is static like interest on a savings account in the bank.

This next factor, E_f, representing the specific energy of the fuel, we can do something about. The fission of uranium, one pound of it, produces something like 35 billion BTU's of heat energy. To put it in more familiar terms, that much heat is equivalent to about 12 million horsepower hours in round numbers. Now one pound of gasoline will run a 6 horsepower motor about an hour. Dividing 12 million by six results in a factor of 2 million.

You can readily see that the avenue is far greater for improvement in this one factor, specific energy of the fuel. The factor of 2 million far outweighs any improvements that might be made in aerodynamic efficiency, propulsive efficiency or structural efficiency, the other items in the equation.

A little bit more about structural efficiency, or shall I say weight of large airplanes. Large airplanes as we know them today have their total weight distributed approximately as follows: 50 percent of the total weight will be in fuel load, and 20 percent in the installed weight of their engines; 22 percent will be devoted to aircraft structural weight: and the remaining 8 percent in personnel, bombs, ammunition, useful load and so forth.

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We have heard of the material, titanium, mentioned in certain correspondence. The writers of this correspondence spoke of it as magic material and gave it wondrous properties.

Actually, information about titanium is rather meager. Certainly some very interesting alloys should be developed from it.

It does seem to have a higher strength at elevated temperatures of four or five hundred degrees, but present information on its strength -- weight ratio, its modulus in tension, in compression, in shear, and in torsion present no valid grounds for forecasting that any real weight savings will result from its use. In fact, my engineers, together with those in the Aluminum Company, won't let me predict any weight saving over aluminum.

But suppose they are wrong. Let us assume now, merely for illustration, that titanium is a wondrous metal and that its use will result in a 10 percent saving in structural weight. This 10 percent is several times higher than anyone would ever use. Now let's apply it to the whole airplane.

Since the basic structure only weighs 22 percent of the total, a 10 percent improvement would save only 2.2 percent of the total gross weight. You can see that this would have a relatively insignificant effect on either range or speed or over-all size of the airplane.

The specific energy of the fuel, now that uranium is with us, still offers by far the most significant chances for improvement in the performance of airplanes where the combination of long range with high speed is needed. So much for the Brequet equations.

In recognition of the fact that my voice is on the ailing side today, Mr. Ward has covered some of the more significant meetings that we held during the year. There is no need for me to review them again.

I would, however, like to point out that these meetings are large affairs. Our seminar in April had 228 people present; and of those 228, 99 of them were visitors from off the area, including guests from the A.E.C., Washington; BuAir; BuShips; Air Forces; Ground Forces, in addition to those attending from the member companies.

The rest of the gentlemen were from NEPA and people from Carbide, X-10, and people on the Oak Ridge area.

At our materials symposium, June 14 to 19, there were 200 people present at the meetings. Ninety-two were from NEPA and sponsor personnel, and 32 were guests from outside organizations.

Representatives were present from Ohio State; GE; Air Materiel Command, Wright Field; Headquarters Air Forces; A. O. Smith; M.I.T.; BuShips, Allison; Battelle; Argonne; A. C. Spark Plug, Champion Spark Plug; Armour Research; Kenametal; A.E.C., Washington; United Aircraft; Bureau of Ordnance; Wright Aeronautical; and A.E.C. installations on the area.

In the last symposium that we had on radiation damage, on December 6, 7, and 8, we had 215 people attending classes for those three days; and of this number, 120 were NEPA and sponsor personnel, and 95 were guests from outside the area.

That symposium was one on radiation damage, and one of the lectures was the effects of radiation on mammalian tissues. We have a lady physicist, and you gentlemen who have been with us at Oak Ridge know that we have a "coke" machine outside the auditorium on the porch. It is customary to recess every once in a while and go out and get a little refreshment.

This young lady was out on the porch during recess, and just as I was about to put a nickel in the "coke" machine, I heard her say, "Yes, and it takes the fuzz off the balls, too." (Laughter)

I wondered what she was talking about; I thought maybe I might be missing something at that point, perhaps even some fuzz. But there were too many visitors present for me to find out politely. But after school was out I inquired into the matter further and determined that everything was all right after all. Our lady physicist was merely talking about the difficulty of playing tennis on the concrete tennis courts.

CHAIRMAN WARD: I don't see General Putt as having returned from the Hill, so, Colonel Demler, would you come up and give us the benefit of your remarks, please.

COLONEL DEMLER: Gentlemen, the purpose of my remarks is to provide the NEPA Board of Consultants with a status report on the project insofar as the RDB, the Military Liaison Committee, and the Atomic Energy Commission are concerned.

At your last meeting, General Craigie outlined the action taken by the Research and Development Board in recommending that the A.E.C. review all work in this field and formulate a plan to effect a single, unified and coordinated program to develop nuclear engines for aircraft.

As you know, the A.E.C. conducted such a review this past summer, and we in Air Force think the Lexington Project did a very excellent job.

On 8 December 1948 the Atomic Energy Commission advised the Military Liaison Committee and the National Military Establishment of the conclusions of the Lexington Project report, and the A.E.C. proposed a

plan to prosecute a joint interim development program on nuclear-powered flight.

The principal conclusions of the Lexington report were outlined by Mr. Sims, and I understand the Board of Consultants has now been furnished a copy of this report.

I should like to read from the A.E.C. letter certain excerpts relative to their proposal for a joint interim program.

Before getting into that, however, I think one of the most important factors the Air Force itself is concerned with, from the A.E.C. letter, is a statement that it is apparent from the Lexington report that a strong development program on nuclear-powered flight would involve large future commitments:

> "The magnitude and complexity of such an undertaking dictates a need for a policy decision at the highest Governmental level as to the effort involving manpower, money, fissionable material, that can be justified by national interests in the prosecution of this program.
>
> "The Commission would appreciate learning the views of the National Military Establishment with respect to the basic conclusions reached by the Lexington report. The Commission desires to obtain the policy decision at the highest level which is based on a thorough evaluation of the respective merits of expending heavily of national wealth for the development of nuclear-powered aircraft, as compared with similar expenditures for development of other means of national defense.
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> "Inasmuch as a decision will aid materially in getting a program under way, the Commission requests views of the National Military Establishment with respect to the manner in which such a decision can best be obtained at an early date.
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> "We would suggest that after your views have been formulated, this matter be made the subject of discussion between the A.E.C. and the Military Liaison Committee.
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> "The decision referred to above is essential before there can be a determination of the scope and emphasis that should be applied to the aircraft program.
>
> "The Lexington report sets forth a number of lines of effort which might be pursued as part of an interim

program. In this connection the Commission will expand its reactor work into those parts of the program involving nuclear aspects such as reactor studies, research and development on materials, with special attention to the materials requirements for aircraft reactors.

"3. Research and development on shielding. As is indicated in the Lexington report, there is a large area of investigation and development in the fields of compressors, turbines, heat exchangers, and aircraft frames, which is predominantly non-nuclear and involves specialties in which the National Advisory Committee for Aeronautics and the National Military Establishment both have great competence and continuing interest for reasons apart from nuclear-powered aircraft.

"Detailed studies of systems using thermal energy from the reactor in actual aircraft will be required. While these activities should be closely integrated with the work outlined above for the Commission's effort, the Commission suggests that the work in the thermal and heat engine area can best be prosecuted by the National Military Establishment and the NACA."

Then, the Commission requests that if these views are generally satisfactory to the Military Liaison Committee and the National Advisory Committee for Aeronautics, that the several parts of the reactor program could be examined and could work out with these agencies suitable procedures for coordinating work on the various parts.

The Military Liaison Committee, for your information, with the coordination of the Air Force, Navy, and Army, advised the A.E.C. that the measures outlined by the A.E.C. were a sound basis for future action and probably as sound as could be devised at this time.

Whereas the views of the entire military establishment could not be given prior to action by the Research and Development Board and the Joint Chiefs of Staff, the service which had the greatest interest in the continuation of the program, namely, the Air Force, has stated its full concurrence in the contents of the A.E.C. letter.

With regard to the Commission's desire to obtain a policy decision on the respective merits of developing nuclear-powered aircraft as compared with the development of other means of national defense, a study has been introduced into the Joint Chiefs of Staff by the Air Force for this purpose.

The Military Liaison Committee went on and replied to the Atomic Energy Commission, to state that the Committee considered the Lexington Project

report a highly competent and independent summation of the factors involved in determining the feasibility of developing nuclear engines for aircraft.

It is believed that the Lexington Project has contributed very materially to a clarification of the scientific and engineering thinking on this subject.

At appropriate times the National Military Establishment will be prepared to examine jointly with the Commission and the NACA the several parts of the reactor program and to work out with these agencies suitable procedures for coordinating the work of the various parts of the over-all program for developing nuclear engines for aircraft.

Accordingly, on 12 January, the Air Force and Navy met with the A.E.C. and NACA to sit down and generally examine several parts of the program. At that meeting the Air Force stated its position generally as follows, and I would like to read from that as pertinent to this subject:

> "The Air Force is pleased to be invited to join with the Commission and the NACA for this purpose, and the Commission's letter of 8 December was accepted as a desirable framework around which the scope and effort on an interim program could be determined.
>
> "It is recognized by the Air Force that a joint technical board is probably the only feasible solution to coordinating the effort of several independent agencies who possess different skills and facilities as required for this type of project.
>
> "The work to be assigned to the present Air Force and Navy sponsored NEPA organization in a joint interim program can be as determined by the agreements reached by the joint technical board."

In order that the Air Force and its industrial contractors may be in a position at an early date to carry out their future responsibilities in this field, the Air Force stated that:

> "It is desired that our personnel be afforded a maximum amount of participation under the technical direction of the agency to be assigned the responsibility for specific areas of work. We feel that such participation by Air Force contractor personnel will promote the integration of the over-all program and provide the best utilization of facilities and personnel available."

As a result of discussions at this first meeting with A.E.C. and NACA, a general agreement was reached that the A.E.C. would initiate action to organize an ad hoc committee of representatives from the A.E.C., NACA, Navy, and Air Force to prepare recommendations regarding the distribution of work and the manner of operating such a joint program.

It is anticipated that such an ad hoc committee will meet perhaps in February. In the meantime, with the assistance of Mr. Ward, Mr. Sims, the NEPA Project generally, the Air Force would like to prepare some recommendations and suggestions for use in setting up a joint coordinating program involving other agencies.

In conclusion, I would like to say that, pending a decision by the Joint Chiefs of Staff in the over-all military importance of this project, the prosecution of an interim program on such a joint basis appears to be a very satisfactory solution to the problems which we were faced with at the time of your last board meeting a year ago.

At this time I would also like to express complete satisfaction of the Air Force with the progress, accomplishments of the NEPA Project during the past year. We are grateful to you, Mr. Ward, for your leadership in this effort; and on the joint interim program, which you folks at Oak Ridge call the "New Look," we hope that the good work and cooperation will be continued.

Thank you.

CHAIRMAN WARD: Thank you very much, Colonel Denler.

Now we are getting toward the close of the general policy considerations that have to do with this project, and I think you might be interested to be brought up to date on the facilities that have been constructed at Oak Ridge.

On that, we would like to call on the Deputy Administrator, Mr. Frederick Kauffman.

MR. KAUFFMAN: Gentlemen, we are pleased to report that our first real facilities for getting our hands dirty are completed. The materials laboratory is completed and in operation. The equipment is not all hooked up and shaken down, but it is operating about 75 percent.

The chemical laboratory which we have had under modernization and extension is also completed and the chemists are busy in there with their pipes, and so forth.

These two laboratory buildings, we think, are very finely equipped and will enable us to do the control work necessary, in addition to the work being done by contractors and subcontractors. The investment in the two buildings was in the neighborhood of $575,000, less equipment.

Out of our budget, we have spent about $500,000 for new equipment for these two laboratories. In addition to that, we were able to get a good deal of equipment from War Assets, and the A.M.C. at Wright Field were extremely helpful in assisting us in running down a lot of equipment. We were able to get a lot of excess equipment directly from the U. S. Air Force.

All told, we have estimated about 1 1/2 million dollars worth of equipment in these two laboratories; that is, at replacement cost.

I might give you some idea of the major pieces of equipment in the materials lab. We have a dilatometer; a compression furnace, 2600°; 200-ton press for hot and cold pressing of metal powders and ceramics; a 50-ton press for hot and cold pressing of metals and ceramics; a carbon resistor furnace for the testing of materials up to 4500° F.; electron diffraction unit; a Phillips X-ray spectrometer; a Picker X-ray diffraction unit; a Hayes X-ray diffraction unit; a Bausch and Lomb Mettalograph; one petrographic microscope; a Fagergren flotation machine; 50 KW carbon resistor furnace, maximum temperature 5000° F.; and so forth.

So the materials lab is pretty well equipped. We have not received all the apparatus which we want to put in the laboratory. Some of it is on order, but it is coming in in pretty good shape.

We have split up the chemical laboratories into five sections: Physical Chemistry; Analytical Chemistry; Radiation & Tracer Chemistry; Spectroscopy; and Inorganic Chemistry.

We have the usual pieces of equipment in those laboratories that you will find in any well-equipped laboratory.

I have a sort of kindergarten chart here to give you an idea of what is happening. Most of you are familiar with the buildings in the S-50 area. They have all been turned over to us by the Atomic Energy Commission.

F-10, as you will remember, is the materials laboratory; and the space here, F-08, is the new chemistry laboratory. Those colors in orange are shops and laboratory facilities; and the black color indicates office space, drafting rooms, and so forth.

We have had an engineer go over the area and give us some costs and estimations and specifications for rehabilitating these old buildings. Most of them, of course, were temporary, and in pretty bad condition.

We expect next week to issue an invitation for bids to rehabilitate some of these buildings. Those outlined in green will require rehabilitation. We are very short of storage space, and we are putting up a new temporary building for a warehouse. That is outlined in red.

As a matter of fact, we were looking for excess material and found some steel beams in AMC custody. We wrote for the drawings, and instead, they sent us these terrific long steel girders. They arrived and we stacked them up in a pile and scratched our heads to find out what we could do with them.

We found out that we could pin them together, and we hope to have a new temporary storage building which would give us about 16,000 square feet of space. We checked very carefully and found out that by using these steel beams it was considerably cheaper than putting up a wooden building.

The only building we are not actually occupying at this moment is building F-18, which was really in horrible shape. But in our plans for the bids next week, we intend to put an extension on this end of the building for a new engineering drafting room.

The first phase of this rehabilitation, including this new building and the extension of this building here, will run in the neighborhood of $225,000; and we will have a second phase of it which will run something on the order of another $200,000. We hope to include some space for some sort of a quick lunch cafeteria. Our personnel have been very patient in putting up with the poor quality and appearance of the food now furnished us in the Carbide Canteen. So much for the buildings.

We have been informed we will be unable to put any more permanent-type buildings in this area. So we are rehabilitating these as fast as we can. Of course, the major effort is on the facilities. In addition, we have recently placed an order for a Univac electronic computer, which will probably take some year and a half for delivery. We hope to be one of the first six to receive one of the machines from the Mauchley-Ekert Corporation. So much for the facilities.

I guess I would be remiss if I didn't bring up something about this old bugaboo of security. I would like to inform the Board of Consultants that the Number 6 Report was completed in September, and we sent it in for a security check prior to distribution. We were told we couldn't distribute it to the member companies.

So we had some meetings and wrote a lot of letters and went back into the historical parts of the project and endeavored to see what we had to do to change the policy, so that we could give the necessary information to our Board of Consultants.

Well, we ran into the problem of which came first, the hen or the egg. How do we get the interest of our member companies? How do we get more interest? Do we give them information and thereby get more active participation? Or do we get their participation and then give them information?

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There is no answer so far, and apparently we will have to go through the same process in sending information to you gentlemen that we have gone through on this.

Incidentally, out of this discussion came the so-called idea of a principal and an alternate; and I am afraid that when our invitations went out, we were not sufficiently specific. We meant that we would like to have both the principal and the alternate attend these Board of Consultants meetings, if possible.

We grasp at stray bits of sunshine on personnel clearances, but the clouds get darker and thicker, and we still have more trouble trying to get people cleared for this project than I think we have any reason to expect.

At Oak Ridge there has been some curtailment of activity in the A.E.C. The people working at the Clinton Laboratories are in the process of being laid off. They have been working with restricted data and they have A.E.C. "Q" clearances. We would like to take them on, but we can't take them with "Q" clearances. We have to send them on through and get a U.S.A.F. and NEPA clearance. That takes anywhere from two to five months.

We have now 91 people waiting who would like to come to work, who had previously been cleared as far as we know by the Atomic Energy Commission, i.e., have worked with restricted data. We are still waiting on them. We have had one waiting since August; one waiting since September; two since October; fourteen since November; sixty-two since December; and eleven in January.

For new personnel who have never been mixed up in this restricted data racket, we have 163 people awaiting clearance; 23 since September; 63 have been waiting since October; 19, November; 36, December; 19 in January.

You know, sometimes we feel we don't live right down there in NEPA. The other day we had an application from a man to come in as chauffeur. I guess this bird is probably as well known in the Manhattan District and by the A.E.C. as probably any other individual.

He was until very recently the number one driver of the number one car for the number one man at Oak Ridge. I think he drove for quite a while.

So, being such a good driver and knowing his way around, we decided we could probably use him. But then we looked into the security angle, of course; and we find -- lo and behold -- the A.E.C. wouldn't give him a "Q" clearance. He has been in all the plants, driving in all the Manhattan District, driving generals around, Mr. Franklin, Manager

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of Oak Ridge, in and out of all these places. They wouldn't give him a "Q" clearance, so I don't know what we are going to do. We haven't dropped that in the U. S. Air Forces' lap yet.

In regard to our personnel, you may be interested to know that we now have about 475 employees. We had 106 in January 1947; 283 in January 1948; and one of our biggest problems down there in personnel is the question of houses.

We had 151 houses in January 1948. We now have 173 for 475 employees. There doesn't seem to be much we can do about it. We get a few houses now and then. I remember last summer General Sims wrote three letters, at least three; and we made numerous calls trying to get a house. We finally got one, too; six weeks and many hours on the telephone and many letters were required to get a house.

Oak Ridge is building up down there. The A.E.C. is building some new apartments and some new houses. The Commission is endeavoring to open Oak Ridge and make it a normal city. So housing is going to remain one of our major problems.

Thank you.

CHAIRMAN WARD: Now we have finished the policy, and I guess I had better add gripe, section of the program. We are down now to the technical angles.

We will open the technical section with general remarks by the chief engineer, Mr. Kalitinsky.

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MR. KALITINSKY: Gentlemen, the technical part of this meeting is going to be devoted to the presentation of this report, that the member companies have now received, NEPA Report No. 6 on criticality and control. It is a very imposing looking report. Actually, the report itself is fairly short, and the rest of it is the appendix.

I would like to explain first the relationship of this report to the previous ones, in particular to NEPA No. 4 and NEPA No. 5.

We have started out in a very new field here, and therefore we had to build a certain foundation from which to work. The first block in that foundation was Report No. 4, which attempted to cover the whole field and set down certain fundamentals from which the work could proceed, and make those available to all the people who would be working or consulting with us.

However, if you recall No. 4, you will remember that there were some significant gaps in it. Although the subjects were treated, they were treated in a very sketchy manner.

One of these gaps was materials. Now that gap has been filled out by NEPA No. 5.

Again I want to emphasize, we don't say that NEPA No. 5 contained any new materials information. Hardly any was in it. It was mostly a compilation of available data on which to build, and an outline of a program by which new data could be obtained and new materials could be developed.

The other significant gap in NEPA No. 4 was on what you might call the reactivity and controls. That is the nucleonics of the reactor and the dynamics of the reactor.

Now, NEPA No. 6, this report here, is an attempt to fill that gap and provide a basic block from which we can work. There is still a third gap in NEPA No. 4, which I will just mention in passing, and that is shielding. I don't think that we will have a cornerstone, because there is just no available information on shielding to speak of, and all of it has to be developed from scratch. So NEPA No. 7 will not be a shielding report. The information on shielding will be reported as it is developed.

This Report No. 6 as we have it now here is a little bit outdated. As Mr. Ward mentioned, it was originally scheduled to be prepared and presented sometime this summer, in July, I believe. Its preparation was delayed by an unusual amount of activity in presenting data, preparing new data, preparing presentations and reports for the Lexington Project, and some of the activities that went with it.

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However, the report was completed in September, and it was actually issued on October 1. Since then, as you have also heard, we have had to work rather hard to get this report to you.

So it is a little outdated, but I believe that in the detailed technical discussion by Mr. Cooper and Mr. Bettis, we will try to bring you up to date. This is not too serious, since I have mentioned before that this report is a foundation and is not an attempt to bring you actual current developments.

Mr. Cooper and Mr. Bettis will also try to show you which way we are proceeding, in which direction we are working, what our plans are, and what some of the things are which we have actually learned since we wrote this report.

I would like to say a few words about the importance of the things that are discussed in the report, and their position in the program. First, there is the item of criticality. In a few words, that means just how much uranium does it take to make a reactor actually become a chain reacting system and actually produce power.

We know pretty well that we can make practically any reactor that is satisfactory from a heat transfer point of view critical. It is not a feasibility question. It is a question mostly of economics. We want to know how much uranium it will take, of what enrichment, to make this reactor go, in any number of possible configurations, and how will that investment of uranium be affected by all sorts of variables.

This type of consideration is extremely important in what might be one of the most central and most important items in the whole project, namely, the establishment of specifications for the reactor and the other components such as the engines, let's say, the turbo-jets, the airplane itself and the shield. And the establishment of a set of specifications for all these components which are compatible between themselves, and which are not only that, but which actually represent the optimum compromise between the conflicting requirements of these components and of the operating requirements of each one of them, because we will have to compromise; like in all technical endeavors we are forced to compromise. It is very important to get a good compromise, an optimum compromise. So this criticality work is extremely important in just that respect.

I believe also it is quite important to mention that we have had to pioneer, in a way, on the methods used to calculate and present the results of such an analysis of a reactor. In the past, the approach has generally been simplified by the fact that the objective of a reactor was clearly defined from a nuclear point of view. It was the production of an explosion, or of plutonium, and there weren't these conflicting requirements, at least not to this extent.

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So it was fairly easy to pick one design point and simply design a chain reacting system for that particular point. Here we have so many conflicting requirements that it is very important to cover a wide range of variables, and to have a very clear picture of the effects of each independent variable, alone and in combination.

Therefore, you will see that we have approached this from the standard analytical side and we have been able to apply the well-known methods of engineering analysis to the calculation of reactors.

However, I want to point out that we have not made any new discoveries. We are simply providing the basis for some actual design work on reactors.

I would like to point out again, and I think this point is going to be repeated, that it is necessary to compromise between some requirements that you might call nucleonic requirements, and performance requirements, engineering requirements of the airplane and the power plant.

There are three things that you can pick out as being of major importance. I would like to mention them.

First, there is the gross weight of the airplane. Second is the thermodynamic requirements of the power plant. As an example, I might mention the compressor pressure ratio of a turbo-jet. Third is the uranium economy, the investment in uranium, which in this case replaces to a great extent the specific fuel consumption which is so important in a chemically-fueled airplane.

You will see, for instance, that it is possible to design an airplane for a minimum gross weight, but only if you are prepared to allow a fairly large investment in uranium and are prepared to go to a high pressure ratio.

On the other hand, again speaking rather generally, if you want the minimum pressure ratio, you can achieve that also, but again at the expense of, let's say, medium uranium investment and maximum gross weight in your airplane.

Again, if you want minimum uranium investment, you can achieve that, but at the expense of a very heavy airplane, and again maybe medium compressor pressure ratio.

In a case like that, of course, you have to make a decision as to what you want to do, as what is the relative importance of these considerations. I mean, how important is it to keep the gross weight of the airplane down to some value; how important is it to keep the investment of uranium down, and so on.

For this type of work, we need some good information on the amount of uranium it takes for various reactor configurations, various materials, and so on. Some of it is given in this report.

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Then we come to the question of controls. Again it is pretty obvious that you have to have a power plant of this type well under control. It is a little more obvious than it is in a chemically-fueled engine, because chemically-fueled engines cannot produce more power than is contained in the gasoline that is flowing into them from a pipe.

However, a reactor has a tremendous amount of energy stored right in it, so that it can easily get out of control. So your control system has to be adequate to control it at all times, even though unlimited power is potentially available.

Then, as was mentioned before, the operator of the engines might be the pilot himself or a flight engineer, and he cannot be too involved in figuring out the details of just what is going to happen, and how much he must adjust the controls under certain conditions. So there is a very definite need for an automatic type of control.

In the third place, we have a rather unusual combination of variables and effects, with which we haven't had too much practical experience. That is, the actual mechanism by which the energy is released in a nuclear process has certain dynamic characteristics.

You have all heard about reproduction factors and delayed neutrons, poisoning, and so on. The fission energy heats up a fairly large mass which has a thermal inertia. That mass in turn heats a flow of air which has certain lags in it, and that flow of air is produced by and drives a turbo component, let us say, which also has certain inertia factors and is basically a boot strap arrangement.

We have really no experience with the physical properties of such an arrangement. They can be analyzed, but analysis is always dangerous to a certain extent, because one might very easily miss some of the important physical processes, or misjudge the importance of the various individual factors or of the various constants in a differential equation.

So we were faced with the necessity of investigating this control problem, and again here we have what is not a proposal for a control system, but some basic information partially obtained by analytical means, and partially by what you might call semi-experimental means, such as electronic analogues. This information is to form the basis for our future work. I think Mr. Bettis will tell you about some of the work that we have done since this report has been written, in proceeding onward from that basis.

Again here I would like to emphasize the very intimate interaction between the so-called nuclear phases and the so-called thermodynamic or engineering phases of a nuclear power plant.

For example, if you want fast response, it looks as though one of your main difficulties is actually the time it takes the reactor to heat up, just to heat the mass of the reactor; thermal consideration, not nuclear consideration.

Now, one possible way of getting around that is to operate the reactor always at more or less a constant temperature and simply vary the air flow and the rate power generation accordingly; then you avoid that thermal lag.

However, that immediately effects your nuclear considerations because the reactors that we have been talking about here generally have very high temperature coefficients. That is, their reactivity is affected by temperature to a very high extent.

It also affects the operation of your power plant, because possibly in order to achieve this type of operation at constant temperature, you might not want to control it in a normal fashion; but you might want to bypass some of the air around the reactor to a greater or lesser extent, depending on your power.

So you can see how all of these things are intimately tied together. The control system is composed of several loops which take into account engine speed, turbine inlet temperature, reactor temperature, rate of fission, altitude, airplane speed, and so on, of course.

All these loops naturally have to operate as one unit, and they must be stable. So what you will hear here is some of the basic work on which we can base our reactor investigations.

In concluding, I would like to say that, like all reports that are written at a certain time, it is not complete. It does not cover certain types of reactors. It covers the ones in which we are most interested at the present time. It does not make use of the most exact methods of calculating criticality; however, we believe that it makes use of the best methods for its purpose, because what we want is to cover wide ranges and show trends. However, if we were actually predicting a critical mass for an experiment, we wouldn't pick it right off the curves you see here. We would recalculate it.

The calculations in this report are only as good as the data on which they are based, and some of that data is not very good. This also makes use of too complicated calculation methods not worthwhile in some instances.

Of course, in quite a number of cases here, we are outside the range of practical experience with critical masses; and therefore it would be highly desirable if some of the points on these curves were confirmed by some critical experimentation. The same to some extent applies to the controls. The control calculations are based on certain data with

respect to delayed neutrons, xenon cross-sections, and so on. Some of that data is not very good. It would be very useful if some of the calculations could be confirmed by experimental work.

I believe that more details will come out in both presentations, and I hope in the discussions that will follow them.

CHAIRMAN WARD: I think before Mr. Cooper takes the program, we ought to have a five-minute recess.

(A short recess)

MR. KALITINSKY: Gentlemen, I would like to introduce Mr. Cooper now, who will talk on the reactivity part of this report. He is the head of our Analysis Section, and as such, he handles not only the nucleonic analysis, but also the thermodynamics, thermal stress, heat transfer, and so on. From what I have said previously, you will understand why it is absolutely essential to combine both the nucleonic, the thermodynamic, and stress analysis in a single working group.

I might point out that we have in some ways achieved something new here. I think we have provided a group of what you might describe as nucleonic engineers. I don't know of many others in that field right now. It is obviously something that will have to be done on a fairly large scale if we are to have practical use of atomic energy. I believe we have done this, and it is also a very important step to alleviate some of the shortage of specialized and competent manpower that exists in this field.

The only way that that shortage can really be alleviated is not so much by people attending lectures, as by people actually getting in and having to do the work.

Mr. Cooper.

MR. COOPER: Gentlemen, the title of NEPA No. 6 Report is "Criticality and Control." Criticality is the study of a chain reacting system that is going along at a steady rate of heat production. Control is the study of this same chain reacting system when you want to change the rate of heat production, or sometimes when it wants to change the rate of heat production and you don't.

So I would first like to discuss some of the bases of criticality. When an atom of uranium-235 fissions, we know that a neutron has hit the bulls eye. We also know that on the average two and a half more neutrons come out during this little explosion.

Right now I would like to shift over to 100 fissions at a time, because I have difficulty with these half neutrons. When we have these 100 fissions, and these take place in a critical pile, this means that 100

of these 250 neutrons have to be reserved to cause new fissions so that we can continue the chain reaction.

So the basic studies in criticality are, what happens to the other 150 neutrons. Two things can happen. They can be absorbed in the pile without causing fission; or they can escape from the pile completely.

On this chart I have shown where the neutrons go in three different types of reactors. First is the natural uranium reactor, such as the Hanford one. These natural uranium reactors are the ones we hear most about, and the total length of the chart here represents the 250 neutrons per 100 fissions. The red portion represents the 100 neutrons that are reserved to continue the chain reaction that have to cause more fissions.

The white part represents the number of neutrons that are absorbed in the pile without causing fission, and the blue part represents the neutrons that leak out into the wild blue yonder.

As you know, in a natural uranium pile, we have one atom of uranium-235 which is fissionable, to 139 atoms of uranium-238, which is relatively non-fissionable. The uranium-238 isn't just sitting there. It is grabbing some of the neutrons; and as far as continuing the chain reaction at the time is concerned, we call that absorption, and it is non-productive from that standpoint; although from the Hanford standpoint it is quite productive since most of it is forming plutonium.

One hundred neutrons of the 250 now are going to continue the chain reaction. One hundred and forty are being absorbed in the reactor. Ten escape or leak outside the pile and are gone forever.

Let's take another extreme. Suppose that we wanted to set up a chain reaction in a mixture of pure uranium-235 and a moderator, graphite. In this case we would find of the 100 fissions, again 250 neutrons produced; 100 reserved to continue the chain reaction; 25 being absorbed, mostly in the uranium-235 without causing fission; and 125 escaping.

In NEPA No. 6 we present a design study of a reactor which could possibly be considered as being a practical type of reactor; that is, it is not one extreme or the other. It is a reasonable size and it has reasonable materials in it.

When you put reasonable materials in a reactor from the construction point of view, they seem unreasonable to the neutrons; and some of the neutrons are absorbed. So we find in this case, 250 neutrons again per 100 fissions, 100 reserve to continue the chain reaction, 65 absorbed in the pile without causing fission, 85 escaping.

What is the significance of all this? As you know, this leakage is necessary in a reactor. It is not possible to put some kind of a

barrier or membrane around the outside of the reactor and prevent all the neutrons from leaking. The neutrons always see some holes between the atoms in there, and if one of the neutrons happens to see a hole that leads to the outside of the pile, it makes a bee-line for it and gets out.

We can put a reflector around the pile and try to prevent the leakage, but we can only prevent some of the leakage. There will always be some neutrons that leak.

So we only have this little margin of ten neutrons out of 250 that we can allow to leak in the case of the Hanford pile. The construction of the pile dictates basically that 140 will have to be absorbed.

So, how do we achieve this? How can we keep the leakage down to only 10 out of 250? We have to cut down on the ratio of the surface through which leakage occurs, to the volume in which the neutrons rattle around. To keep this ratio down, we have to have a large pile, and these are large. That is, I am talking of something 20 feet, of that order.

In the enriched reactor, when we get rid of the uranium-238 or most of it, we can have as much as 85 of the 250 neutrons leak. This means we don't have to be nearly so careful of holding down the ratio of the area through which it leaks to the volume in which the neutrons rattle around. So we can have a practical size. That is, by practical, I don't mean this. I mean something like this; something like that, several feet, a size that you can consider would be capable of powering an aircraft. So that is the first consideration.

If we are stuck with natural uranium for our pile, it has to be a big one and it cannot fly. When we can have enriched material, we can have it small, all because of this leakage.

Another thing about that. Suppose it is found necessary to put in more materials in the pile for coating a structure, or something of the sort. That would tend to absorb more neutrons.

Let's take the NEPA study. Here we have 65 neutrons of the 250 being absorbed. Suppose that we had to change the specifications of this pile and said 75 had to be absorbed. That would mean, then, that 75 instead of 85 would be left to leak. We could take care of this by putting in more uranium.

Now, what would happen to the Hanford people if somebody said, "You have to put in some more absorber to the extent of 10 neutrons out of 250"? That would leave none to leak whatsoever; since we have said that we can't prevent leakage altogether, you just could not make this pile critical.

So it is nice to know that in one sphere of activity, the achievement of criticality, the problems of NEPA are easier than the problems of some other reactor people.

Now I would like to get into the work that we have done on criticality. First a sketch of the type of reactor that we have studied. These dimensions should not be scaled. Working figures are given in the later charts.

This pink portion is the core of the reactor. The core contains the fissionable material, uranium-235, and moderator. I think you all know the purpose of moderator. Moderator is in this type of reactor to slow down the neutrons.

Neutrons come out of fission at an average speed of about 50 million miles per hour. When they get slowed down as best as they can by the moderator, they are crawling along at only about 5,000 miles per hour. The reason we want to slow them down is that a uranium atom looks bigger to a neutron which is crawling along at the 5,000 miles per hour than it does to a neutron which is zipping along at 50 million miles per hour, or anywhere in between.

If the uranium atom looks big to the neutron, the neutron has a better chance of smashing into it and causing fission. If it looks small, the neutron can zip right past it and has a better chance of escaping completely out of this system.

The way we have to compensate then, if too many neutrons are going too fast, we just have to put more atoms of uranium in there so that the sum total looks big enough to the neutron.

So here the core contains fissionable material and the moderator. Around the core is a reflector, which in our studies we have assumed to be the same material as the moderator. Through this reflector and core, we have channels for the coolant to go in and out. I will put this chart over here, and if any are interested, they can refer to it later on.

Mr. Kalitinsky mentioned some of the practical considerations that we have to keep in mind in our studies. Shielding is probably the biggest problem. That is, if we are going to have a shield light enough to be flown, the reactor and the reflector have to be small, as small as possible. Let's say the smaller the reflector outside diameter, the lighter the shield. So we are interested in the range of small reactors in this case for shielding.

The thermodynamic problems of pressure drop and heat transfer bring up the question of the coolant holes. We have to have holes to get the coolant in and out, and we have to have area inside the pile to transfer

the heat. The more coolant we want to put through, the bigger the reactor wants to be. Shielding now wants a small reactor; the thermodynamic considerations of pressure drop and heat transfer want a big reactor.

Uranium investment is really what we are solving for, but in these studies we aren't just interested in finding out what amount of uranium is required in a given type of reactor. We want to investigate the regions that will satisfy the shielding and thermodynamic requirements as well as possible and still have no more uranium present than is necessary.

The weight of the reactor itself I put at the last, because any reactor which is shielded has a weight which is a small fraction of the shield weight.

These design considerations lead us to investigate the criticality requirements of reactors over quite a range of variables. Here I have listed these variables. Basically there are three of them: The core size. The core, remember, contains fissionable material and moderator, so we have to have a range of core size. Reflector thickness. The reflector around the outside of the core tends to bat back any neutrons that are trying to escape and enables us to get away with less uranium investment. Coolant flow area. That is the area through which the coolant has to get in and out of the core.

So, in making these studies, first we had to take variations of core size, reflector thickness, and coolant flow area. We did this for a simplified system of uranium-235 and moderator.

This chart shows how the weight of uranium necessary for criticality varies with the core diameter, the diameter of the portion of the reactor containing the fissionable material and moderator.

There are three different moderators shown on here, the three which are discussed in NEPA No. 6, and which we have worked with: Graphite; beryllium oxide; and beryllium carbide. You can see that in all of these we start out with a small diameter, that is, a small size core. This core, by the way, has no reflector around it, no coolant holes to simplify the discussion. We start out with this small core, and we have a high amount of uranium required.

We make the core bigger and the amount of uranium required goes down. Why? It comes back to this question of the disposition of the 250 neutrons. One hundred have to maintain the fission chain reaction. The other 150 are going to split it between leaking and absorption.

In this case where we had pure uranium-235 and moderator, about 25 of the neutrons were absorbed and 125 leaked.

If we have a small diameter core here, there is not enough moderator there to do a very good job of slowing down these 50 million-mile-per-hour neutrons, so that some of them will slow down pretty well. Some of them will not slow down very much.

The ones that do not slow down very much have a good chance of escaping. To keep the fission process going, the chain reaction, we have to put in more atoms of uranium-235; so that as far as the neutron is concerned, if it cannot see big atoms, at least it can see a number of small atoms of uranium-235.

Then as we go out in diameter here, we put in more moderator; the neutrons are able to slow down better. The uranium atom looks bigger, and less total uranium is required.

In the case of carbon, you can see that going from perhaps 3 1/2 feet, at 200 pounds of uranium, to 4 1/2 feet at about 50 pounds, is quite a substantial saving in the uranium investment by going up in size.

As for beryllium oxide and beryllium carbide, they are better moderators than graphite. They do a better job of slowing the neutrons down. Remember that the neutrons slow down by giving a kick to the atoms of the moderator, and you can kick a small atom better than you can kick a heavier atom. The beryllium atom is lighter than the carbon. Therefore, the neutron will lose more energy per collision, and we say that the beryllium compounds are better moderators. They do their job better than the graphite. So throughout this range the beryllium compound moderators enable us to have less uranium for a given size reactor.

Incidentally, you can carry this argument too far. That is, within reason, as we increase the amount of moderator, the uranium investment goes down. But after a certain point, we are doing a pretty good job of slowing down the neutrons. If we try to make it any bigger than that, we just cannot slow them down any more than to thermal equilibrium of the moderator. So the benefits stop after some diameter.

Next the reflector thickness. We have taken a graphite reactor which has a four-foot core and a free flow ratio of .3. I will have to define that.

Remember we have coolant channels in the core, and if we take the area, the total cross-sectional area, of the coolant channels and divide that by the cross-sectional area of the core, we get this so-called free flow ratio; and when we have .3, that means 30 percent of the cross-sectional area of the core is taken up by coolant holes.

This was a graphite moderated reactor. You see, if we try to get away with no reflector at all, zero thickness, we would have eight or nine hundred pounds of uranium-235 required. If we go up to about a foot in thickness, we get down to about 12 pounds. So, from the standpoint of reducing the amount of uranium-235 required, you can see it is quite desirable to have a reflector.

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Now, here again we start to lose out on the percentage gained if we go too far. That is, around a foot or so we have done a pretty fair job of keeping in a good percentage of the neutrons that are trying to get away from the pile. If we double that thickness, we keep a few more in, but it does not save us much in uranium.

As one report put it also, "From economic considerations, an infinite reflector is impractical." That was not our report.

The third variable we studied was the free flow ratio. A solid core is quite desirable to calculate, but you cannot get much heat out of it. So we want to study the effect on uranium weight of changing the ratio of flow area in the core to the cross-sectional area of the core, or the free-flow ratio.

If we have this solid reactor, which is four feet in diameter, the core, and has a one-foot reflector around it, we find that if there are no coolant holes at all, we need perhaps seven pounds of uranium. If half of it is coolant holes, then we need maybe 70 pounds of uranium.

What is the reason for this? We will look at the neutron again. If we have coolant holes in the core, we do not have moderator where the holes are. Whatever moderator we do not have there does not help in slowing down the neutrons. Faster neutrons do not get as good a view of the uranium. That is, the uranium does not look as big to them. They have a better chance of escaping.

We just have to put in more atoms of uranium, therefore, to compensate for that.

These curves so far have been plotted with the ordinate uranium weight. For use by the other engineering groups considering the thermodynamics and the aircraft part of the picture, it is frequently desirable to make cross plots.

This curve has the core diameter plotted to the right. In all of these studies I should have mentioned before the core length is assumed equal to the diameter. The core diameter is plotted to the right and the reflector or diameter plotted up. That is, the reflector diameter equals the core diameter, plus twice the reflector thickness.

This happened to be done for a beryllium oxide moderator at a free flow ratio of .3 again. These curves are discussed more at length in NEPA No. 6, so I will not comment on the particular shapes of them here.

Since preparing NEPA No. 6, we have experimented with other ways of plotting this information. Here is one such way, again for beryllium oxide moderator. I will go all over this and then explain what the items mean.

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We plot optimum free flow ratio against the free flow area, the coolant flow area, with lines of core diameter and uranium weight on here.

Now, free flow area is just the cross-sectional area that the coolant has to go through. We remember free flow ratio is the ratio of the coolant area to the cross-sectional area of the core. Optimum we have defined in this case as being the free flow ratio for a given uranium investment, uranium-235 weight, which gives us the smallest total volume of core and reflector to shield. This curve, then, was prepared from the shielding point of view.

Suppose someone said, "Not one pound more than 50 for uranium-235." Okay; we are going to have 50 pounds of uranium-235. The thermodynamics people say, "We have to have six square feet of flow area."

Then from this we can find out what the optimum free flow ratio is to make the shielding problem as easy as possible; that is, to have the minimum volume to be shielded.

So we look along this curve and we find out that it is perhaps .4. Then, by chasing down some other curves, we will find out what reflector thickness this requires.

Incidentally, the way it turned out in this case, if we have these problems, we are stuck with a given amount of uranium weight that we can't exceed, and we are stuck with a given flow area for part of the range here, it is found desirable to have a bare reactor, a reactor with no reflector at all. For some of the rest of it we need varying thicknesses of reflector.

The portion to the right was the bare reactor, and down here we got up to perhaps six inches thickness of reflector.

These lines are constant core diameter lines, the dotted lines; and the solid lines are uranium weight. So this, in conjunction with some simple geometrical considerations, would give us the core thickness, the reflector thickness, for a minimum total volume to be shielded, if the other engineering groups specify the coolant flow area and the uranium weight.

There are all sorts of ways of plotting this type of information, all sorts of parameters. General Electric had a report that came out last October in which they have considered heat transfer and thermal stress requirements in with the uranium weight. So, for particular purposes, you can get all sorts of cross plots.

These studies and most of the reactor studies that have been made so far at other places, as well as NEPA, were made with pretty simplified systems, as in our case we just took pure uranium-235, pure moderator.

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When it comes to a practical design study, you have to consider some other variables; first, temperature. In these studies presented so far, it was assumed that the reactor was at room temperature. When we heat the reactor up — that is, a requirement of any power reactor is to run pretty hot — the moderator atoms are rattling around a lot faster than they are in a cold reactor.

That means the neutrons that slow down to the thermal equilibrium with the moderator are rattling around faster, and the uranium does not look as big to them.

So when we consider elevated temperature, we are then faced with the problem of putting in more uranium to compensate.

Coatings and structure. In the design study that was reported in NEPA No. 6, the materials people specified a possible coating material, silicon, which would protect the moderator, graphite in this case, from oxidation. In addition to that, it would protect the coolant stream from the fission products, the radioactive portions of the uranium atoms that have split.

This silicon and the other structures involved in this study unfortunately absorbed some neutrons.

Depletion. Although it is possible to get an enormous amount of power out of a little bit of uranium, in a power reactor the uranium used up is not inconsiderable. To get 100,000 kilowatts of heat energy for an hour, we are burning up about a hundredth of a pound of uranium; and for an aircraft reactor which may be several hundred thousand kilowatts, operating for upward of ten hours per mission and a number of missions, there will be an appreciable amount of uranium consumed on the order of some pounds or some fraction of a pound.

More important from the criticality standpoint is the fission products, some of which absorb neutrons very strongly.

We have to put in uranium to compensate for temperature, coatings and structure, depletion, and fission products so far. The coolant will also absorb some more neutrons.

If we want to iron out the power distribution to be constant in some particular direction, we also have to put in some more uranium. I will discuss that in the next chart.

Enrichment. We are not going to have pure uranium-235. There will be some uranium-238 mixed in with it. Just how much, that is an economic question mainly; but we have to know the effect of it.

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Now for this power distribution. I think you have all been acquainted with the principle that in most reactors there is a high concentration of neutrons banging around in the middle of the reactor; and as we go out toward the side, there are fewer and fewer neutrons.

Generally, the uranium is distributed evenly throughout the reactor, and this means that if there are fewer neutrons to cause fission at the outside and the same amount of uranium in a given space, there will be fewer fissions occurring in a given volume, and the power will also fall off as we go away from the center.

An interesting effect of the type of reactor such as the design study we made in NEPA No. 6, in this case a good percentage of the neutrons causing fission will not come down to thermal energy. They were still going along pretty fast.

As I pointed out before, a fast neutron has a better chance of escaping; so in this case quite a few of these neutrons will escape out of the core represented by this space here into the reflector. The reflector was made of moderating material, so that some of these faster neutrons that escaped the core would be slowed down in the reflector and bounced back again.

When they came back slower than they left it, that meant that the uranium atoms looked bigger, and they had a better chance of causing fission. So we have an actual upsurge of power production out near the boundary of the core and the reflector.

This is quite a break, if we want to level out the power distribution across the reactor; and we do in a radial direction, at least as far as the thermodynamic requirements are concerned.

This means that if we want to re-shuffle the uranium in here and add some more so that we will have constant power production across the radius and have the reactor remain critical, we can do it in this type of reactor with a somewhat reasonable additional increment of uranium in the order of 10 percent; whereas if we had a reactor which was completely thermal, it would require much more uranium to try to approximate a constant power level. We would not have the advantage of this self-levelling effect.

There are a number of further studies that we intend to get into in criticality. Some of the more important ones are listed here.

Full effect maps are shown as number one. Now, what we did in these maps that I showed you in the early part of this talk was to assume that the reactor was composed only of the fissionable material and the moderator. When the thermodynamics people and the aircraft design people took those maps, they took in other considerations into account, arrived at some design point which is discussed in NEPA No. 6. That is a design point for a particular study.

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This design point was so big, it had so much uranium in it, it had a certain free flow ratio, but it did not consider these other practical considerations of poisons, depletion, temperature, and so on.

So what we did then was to choose a design point based on the simplified maps and then go back and recheck this design point for the effects of the additional considerations, and we arrived at a new weight of uranium. In this case, ten pounds of uranium were required by our initial studies; and when we have rechecked and used a better method of calculating, it was found that 38 pounds of uranium were required, a factor of about four to one.

In addition to that, we found out that the design point may have been different. The trends were different when we considered these other effects such as poisons.

In illustration of that, if we have no poisons in a pile, no extraneous poisons, the bigger the pile, the better off we are in general. We can make some savings in uranium by making a little bigger pile, more moderator, less escape area to volume ratio. But when we start putting some poisons in such as the coating materials required to keep the moderator from oxidizing and keep the fission products from the reactor coolant; when we start putting these in, then we find that after a certain point, making the reactor bigger actually costs us more uranium.

So maps prepared with actual design considerations may give you quite a different optimum overall design point from the simple maps. We want to make some maps of varying size, free flow ratio, reflector thickness, considering these operating effects.

Fast reactors. It is possible to have a chain reaction without slowing down the neutrons, and this would be of particular value in the case of liquid metal-cooled reactors where they can be made smaller. So we may get into some studies of fast reactors.

The geometry of the reactors studied so far has been this square cylinder with the length equal to the diameter, and a reflector around it. Sometimes there are demands for criticality studies on different geometries. For example, some people are kicking around the idea of bringing the coolant into the center of the reactor and pushing it out to both ends. This involves cutting out a portion of the core of the reactor right where it will do you the most good to keep it in. So studies of different geometries will be required.

Different reflectors also. In these studies so far we have assumed that the reflector is the same material as the moderator. There are some good reasons for considering different materials for reflectors. One reason would be if we are forced to use a moderator like graphite, which is not the most desirable one, in the core, if we are forced to do this because

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of temperature considerations, for example, we might be able to get away with a better moderator in the reflector which could be run at a lower temperature, a better moderator, such as a beryllium compound.

In addition to that, there is a possibility of doing some shielding in the reflector by using some material such as bismuth.

As you know, we have made control studies of the control element required to maintain or change the reactivity of the pile. This is reported in NEPA No. 6. It happens that the control problem is basically more complicated than the problem of solving the criticality requirements. For a given amount of work, you can get more precise information from the criticality studies than you can from the studies of the control elements. So we will have to get into more involved studies of the control elements to try to even up the picture, to have the control elements predicted with somewhere near the accuracy we can predict the criticality.

Incidentally, as far as this accuracy is concerned, a check was made by some people on the Lexington Project of our design study — they calculated a uranium weight of 8 percent different from ours, which we consider pretty good at this stage of the game. Calculating uranium weight by different methods gives you somewhat different results, but they are within reasonable range of each other.

When NEPA gets around to performing critical experiments, there will have to be a lot of analysis work done on the correlation of the theory with the actual test results.

I have not discussed the actual methods of calculating the reactors yet. This is the best part of the talk coming up right now, the best part being that I am not going to discuss them.

This is just a sample of the type of mathematics you get into, some of the simpler mathematics, in the calculation of critical size for reactors.

The calculations made to date at NEPA have been based on the so-called diffusion theory, which is borrowed from gases. That is, the behavior of the individual neutrons is examined statistically, and then some fancy equations are derived from mathematics which treats the behavior of the whole crowd of neutrons.

This is pretty satisfactory for simple conditions, but in case of unusual geometries or control elements, and in shielding work which has nothing to do with the criticality calculation, the boundary conditions are so involved that we cannot get good results in some cases by using the diffusion theory.

In the past two years a new method has been developing. It started at Los Alamos. It considers individual neutrons, chases them around and

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finds out what a whole group of them are doing, and then averages them out, instead of averaging the behavior first and putting it into mathematics.

The way that would work is, suppose that a fission is taking place at a certain part of the pile. We want to know how many neutrons are coming out. You know the answer to that, 2 1/2 neutrons.

You all realize that that is just an average figure. There is no such thing as half a neutron; in any actual fission there is either going to be one, two, three, or some whole number of neutrons produced.

In this case we find out the probabilities of these different numbers of neutrons, put them around on a roulette wheel, spin the roulette wheel, and then the roulette wheel says three neutrons. All right, we have got three. What are they doing? Which direction are they traveling in?

We will set up the probabilities again, spin the roulette wheel, chase them down. How fast are they going? Based on the probability of going at different velocities, we will find that out again. We will choose a given velocity, and the same with the distance they travel and the event that takes place.

That is, once they have traveled that distance, do they cause fission, or are they absorbed, or do they bounce off a moderator, or are they out of the pile altogether?

The probabilities for these events can be set up around this roulette wheel, and we can spin it. That sounds like a lot of fun until we start thinking of the number of processes involved. We can't just chase down one fission. We have to chase down perhaps one hundred fissions, perhaps a thousand throughout the pile in order to get representative results.

We also are involved with a great many collisions for the neutrons that are being moderated. In some cases there may be a hundred different collisions of the neutrons before they are hooked by the uranium. So it requires a great many calculations to do this.

This method, as I say, originated at Los Alamos, and it is being pursued quite actively in a number of atomic energy installations. You would expect it to have a pretty scientific name to it, and it does have a very descriptive name — the Monte Carlo Method.

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When it comes to actually spinning the roulette wheel, we find when we list all these events that we have to consider that we will have to use something not quite as naive as an actual roulette wheel. It involves some kind of calculations along with some kind of assumptions.

It has been estimated that if we wanted to solve a rather complicated reactor, that is solve the criticality requirements of a complicated reactor by this method, it would require 100 men working for about two weeks to do this. Each man has a desk calculator. Of course, if we use girls they could do it in about one week.

This is the magnitude of the task that is involved in using this Monte Carlo Method. One hundred calculators out of 275 NEPA people -- it gets to be out of this world.

Fortunately, this method was evolved for use on the electronic calculating machines such as the Eniac, and there are more modern electronic computing machines such as the Univac that you have heard about. This one machine will solve in a few hours what it would have taken these hundred people to solve in weeks. We do not show the half a dozen or so mathematicians working to keep this thing busy, neither do we show all the other people involved to keep this hundred men or girls busy and happy.

Finally, I would like to discuss some of the activities in which our people are participating at other sites than NEPA. That is, I do not want to give the impression that we are just a bunch of engineers, physicists, and mathematicians down there merging our identities with one another and with a reflector or stone wall or something around us.

Actually the building up of this group of engineers, physicists, and mathematicians to do this type of criticality work has only been possible because of the contact with other people in the atomic energy field, people who got there first, who have done the pioneer work. Much of this contact has been through reports, and much of it involved personal contact. We have had the utmost cooperation on the working level with these people who were ahead of us, who knew how to do some things that we are interested in.

In addition to learning the methods from these people, the simpler methods, the more fundamental method of calculating criticality, there are a number of activities that were of interest to us in other A.E.C. installations that we wanted to help contribute to and that we wanted to learn about so we could use them ourselves.

Critical experiments is one such. We have had people working along with other A.E.C. installations on critical experiments, helping to run them and being trained.

Shielding really doesn't belong in a criticality discussion, but I keep getting back to it because it is such a fundamental problem to NEPA. Our personnel from the reactivity department have contributed to the shielding problem, both in the test experiments run at X-10 and in the theoretical work.

One of our men spent a month over at X-10 helping to compile a report on critical masses; that is, a report of all the activity that had been done throughout the various A.E.C. installations in calculating and testing for critical masses.

This, of course, was of interest to us in being able to check our methods and could only be performed at X-10, where they had all these reports.

The xenon cross-section, as you will hear from Mr. Bettis, is of extreme interest to us. There has recently been some valuable work going on at X-10. In order to help push this work through and help get the information for ourselves, we had a man over there.

The Monte Carlo Method is only two years old and not highly developed yet. There has been some valuable work going on at Argonne recently. So we sent two of our men up for several months to help on that work and to be trained in it.

Incidentally, right now one of our mathematicians is out at Northrop doing some shielding calculations, using the Monte Carlo Method.

I have discussed so far the fundamentals of criticality; the work that we have done; and the work that we intend to do. I have passed over the problem of how we have actually calculated it. I want to dwell again on this cooperation problem, that we have obtained the utmost cooperation with the working people of other sites. This type of cooperation on the working level is the only way that the art of criticality studies, let's say, can be advanced efficiently.

That is, people of General Electric will develop a new way to calculate reactors or improvement of old ways. We want to get in on that, and we did get in on it. In our design studies, we used a modification of the so-called General Electric method, and the same with these other installations.

I expect in the future that we will continue to profit from contact with the other people. I think more and more the other installations will profit by contact with us.

MR. SIMS: We would like to hold a discussion period now on what has been said by Mr. Cooper, prior to going in to lunch. We will go to lunch following this discussion and before hearing Mr. Bettis.

We have a recorder over here. We are trying to record faithfully the minutes of this meeting. Prior to making any remark, I would like to request that you gentlemen rise and state your names so that the recorder can get it down. The names of the gentlemen who make the remarks are of vital interest to us, too.

I would like to ask Mr. Kalitinsky here to moderate some discussion on what we have heard on reactivity.

MR. KALITINSKY: We mentioned the name of General Electric a number of times. I would like to ask Mr. Prentice if he would like to comment on NEPA No. 6 and maybe tell us about some of the work that has been going on at Schenectady.

I would like to emphasize that in all these presentations, we have to avoid going into the technical details and refinements. I think if there is any interest in them, this is the time to bring them out.

MR. PRENTICE: We had very few comments on your report. I think most of them, if any, on this phase are detailed. The results that you get are about the same as results that are gotten, I believe, at Oak Ridge for the Daniels pile, which is about the same size.

It seems to us, perhaps, that these criticality studies are somewhat preliminary inasmuch as you do not include in them any statement of precisely what the fuel elements and the heat transfer are going to be, which I think you doubtless recognize effects the size of the reactor just as much as any criticality considerations.

Perhaps the purpose of this work was educational, to explore all the variables preliminary to a design study which would incorporate the actual fuel elements and the heat transfer fluid and the canning material and its effects.

I think it was mentioned in one case there that some G. E. studies have included thermal stresses, heat transfer, in the criticality studies. We feel that is the essential part of a design study.

MR. KALITINSKY: I believe that one study, which does include fuel element shapes, the working fluid, and the coatings and all the effects that are of importance, has been made and is very briefly described in NEPA No. 6.

MR. PRENTICE: Our people had one comment on the spectrum of neutron which you appeared to be using. I do not believe that you said in the report that you were using a thermal reactor as such. They thought that perhaps you were. I do not know.

Is that what you are working on, a purely thermal reactor as close to thermal as you can get?

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MR. KALITINSKY: We started out without deciding on a thermal or other kind of reactor. What we tried to do is get the optimum one and let it fall where it may. You may have noticed that there are two sets of calculations, a two-group and a four-group set.

MR. PRENTICE: The comment our people made along that line was this. Because of the xenon effect, our people often find that you get a lower or a smaller amount of fissionable material than what they call the epithermal region, which is just slightly above thermal.

MR. COOPER: This design study reactor included in NEPA No. 6 had about 60 percent of the fissions caused by thermal neutrons and the rest were above. That would probably be more thermal than your reactor.

MR. PRENTICE: I am not talking about the reactor which we are working on at Schenectady, but we are analyzing other reactors and other applications, too, very near the thermal range where one of the requirements is a minimum inventory of fissionable material.

We do find that in the range that you call epithermal is the range in which we get the minimum fissionable material inventory. That is covered by a report.

I presume you are able now to get our reports, our technical reports?

MR. KALITINSKY: Yes.

MR. PRENTICE: That is covered by a report. Actually, it is A.M.A. No. 1.

MR. KALITINSKY: That checks pretty well with the results that we have obtained. As a matter of fact, as Mr. Cooper mentioned, on this particular design study a point was selected on the basis of the simplified reactors and then a check was made, the check with all effects included.

The check was not only made at that specific point but in the surrounding region as well. That check definitely showed that with the contaminated and poisoned reactor, the minimum investment lay towards the faster region than with a non-contaminated reactor.

MR. PRENTICE: One other thing. On the question of temperature coefficient, your report appears to assume that the temperature coefficient will always be negative.

Is that correct? There has been some investigation at our site of the possibility of a positive temperature coefficient which would make a reactor autocatalytic existing at very low temperatures in the reactor.

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We have not completely analyzed that problem, but in an aircraft reactor where you are likely to be under site conditions which are at very low temperature, you may wish to investigate that possibility.

MR. KALITINSKY: Do you want to comment on that? We checked some of those.

MR. CONEYBEAR: The point that Mr. Prentice makes is with regard to a more heterogenous reactor than these studies are for. The effect came in because of the thickness of uranium; whereas in these studies, the uranium was considered to be uniformly diffused. You do not have the same effect in this study.

MR. PRENTICE: The uranium was uniformly diffused?

MR. CONEYBEAR: For this study the uranium was fairly uniformly diffused. That is correct.

MR. PRENTICE: I mean, do you think it actually will be in an actual reactor?

MR. CONEYBEAR: Do you want to comment on that, Mr. Cooper?

MR. COOPER: How about you, Mr. Kalitinsky?

MR. KALITINSKY: We cannot be sure of that, but that is our preferred design right now: that is, where the size of the uranium particles will be small compared to the mean-free path of the neutrons.

MR. PRENTICE: You are aware of that possibility?

MR. KALITINSKY: Yes. As a matter of fact, we did check for the Doppler effect on stability, and for the resonance instability; but in the reactors that we are considering now, they appeared to be faint, insignificant compared to the thermal basis shift effect.

DR. SPEDDING: I would like to bring up a question. I am wondering if any consideration has been given from the theoretical viewpoint to the effect on control when you accelerate and decelerate the airplane as the neutrons shift in their inertia in the reactor? This may be negligible or it might be appreciable. I think it ought to be looked into, but it has not been looked into. It may be inappreciable, but I think it should be looked at because we do know these things do have inertia, and as you accelerate you may have neutrons tending to concentrate a little more on one side of the reactor than another. This will change your control problems.

MR. KALITINSKY: We have not considered that, Dr. Spedding.

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MR. MERRILL: You may get an idea of what that effect may be by remembering that five-thousand miles an hour is the thermal velocity of neutrons.

DR. SPEDDING: It will not make a big difference in concentration of the neutrons, but it might make a difference in the sensitivity of your controls because you are right on the sensitive part.

MR. MERRILL: I just wanted to bring that up, that the thermal neutrons are going so fast and travel such short distances between collisions, that acceleration of the reactor would have little effect.

DR. SPEDDING: I do not expect it would be a big effect. I just do not know. I think the theoretical people ought to look into the matter.

MR. MOONEYHAM: At most we would consider this by second or third approximations. I believe in the state they are in, this would be a second or third order effect.

MR. COOPER: There is one comment I would like to have about this low temperature. In the aircraft application, we do not expect the reactor at any time to be under very low temperature. That is, at the start it is at room temperature. When you are producing power, it will be at a pretty high temperature. The coolant going in is always at a high temperature.

MR. PRENTICE: The only question I had was at the start, on the site, when the reactor had not previously been started before, any auto-catalytic tendency is important whether the reactor has ever been run or not.

MR. MOONEYHAM: Would you mind quoting the temperatures at which you have this auto-catalytic tendency?

MR. PRENTICE: I am not familiar with those details personally. Some of our technical people who analyzed this report just wanted to call that to the attention of the NEPA people, and apparently they are aware of that possibility. So I think that is all we need to say here.

MR. MOONEYHAM: I think it is worth emphasizing again that this effect is found where the fuel elements are heterogeneous; and to date, and in the near future, it seems that we will be working with homogeneous fuel elements.

MR. KALITINSKY: I would like to emphasize one point here, that the whole method of approach that we have used is predicated on considering uranium economy as one of the paramount necessities in the design and selection of such a power plant.

However, there is a distinction that has to be made there between getting the absolute minimum of uranium, no matter what the cost in terms of airplane gross weight, airplane performance and the complication of the rest of the power plant; and arriving at a compromise between the two.

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So, at some point, we will have to have from somebody a fairly good indication as to just how important uranium economy is in the terms of numbers. I mean, is a saving of 10 percent in uranium investment per reactor worth a gross weight increase of 25 percent in the airplane? That is quite an important point, and it will be certainly one of the critical points in the selection of a power plant.

I would like to have some comments on that. Mr. Prentice, would you like to comment?

MR. PRENTICE: No.

MR. KALITINSKY: Dr. Spedding, would you like to comment on that question?

DR. SPEDDING: I am really not in position to tell you on that. I think it depends on so many factors that I have no knowledge of.

I think there is one thing for natural uranium, and another thing if we could get allocated a certain percentage of the amount of fissionable material that is being made in the present piles, and for which there is going to be quite a struggle because that is definitely limited and there are a great many military uses which can be made of this material.

I do not believe in any of your piles you are contemplating using natural uranium, are you?

MR. KALITINSKY: No.

DR. SPEDDING: So the real problem is the allocation of this fissionable material. I am in danger of sticking my neck out again, but I want to make another suggestion, and I wonder if anything has been done about it.

There is another alternative on this shielding problem. It is just on the edge of whether it might be useful or not.

That is, in the piles developed in this country, it is possible that it will get up to a total capacity of maybe 50 million kilowatts. In the piles when they are operating one isotope of strontium generates heat at about 1/4000 of this total. This is a beta emitter. By taking all the isotopes which are very weak gammas or beta emitters, we might be able to get enough to operate one plane which could be much smaller now because the shielding problem becomes enormously simplified. You do not have the neutrons or the hard gammas. You have some gammas because of the stopping. Electrons will cause them.

This has not been studied very carefully. The people at our laboratory have talked about it. I am wondering if it shouldn't be looked into very carefully to see whether it wouldn't be possible to use this waste material to operate a plane of a smaller design with a continuous source of heat. This might be an economy.

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I think it is on the edge of whether it is feasible or not. It would require a lot more capacity in piles than we have at present, but I think it would be worth looking into by a theoretical group.

MR. KALITINSKY: Yes. The same suggestion has been made on using polonium.

DR. SPEDDING: I think the problem is different there, though, because of the quantities you can get.

MR. KALITINSKY: That is right. But both problems really have two major question marks attached to them. The first one is the supply you mentioned. You would have to have a total pile capacity of the order of 50 or 100 million kilowatts.

DR. SPEDDING: That is correct.

MR. KALITINSKY: In order to supply just one or two airplanes. The second problem is, of course, control. If you are talking about a high-speed airplane --

DR. SPEDDING: A continuous source of energy.

MR. KALITINSKY: You are talking of the order of tens or hundreds of thousands of kilowatts heat energy release.

DR. SPEDDING: We made our calculation on 50,000.

MR. KALITINSKY: And the problem of disposing of that heat while you are not operating at the high speed is very difficult. But it certainly should be kept in mind.

DR. SPEDDING: This could be hooked into a plant when you got back with the plane, you see. I think it should be looked into. I think it is a possibility. I think the problems of getting a plane to operate with this are much simpler because you do not have the shielding problem. I think that is the worst problem of all that you have to face.

MR. PRENTICE: You asked a question of me a minute ago about what my opinion was of whether or not the minimum amount of fissionable material was absolutely necessary. It does not seem to me that neither I nor anyone else in civilian status is qualified to answer that question. That is primarily a policy question, it seems to me, of the military establishment; and before going to answer such a question, you have to know how many airplanes you are talking about, and when; what is the time scale.

You people have made no statements as to time scale, nor statements of how many aircraft you are expected to use at any one time, this fifteen-year figure given in this report, or what you are shooting for.

To answer that question, you would have to look very far into the future to determine what the supply of fissionable material is. So I do not feel there is any answer to be given to that question; and certainly, in the final analysis, it comes back to the matter of policy of the Government, of how much is this application worth in fissionable material.

MR. KALITINSKY: I quite agree with you that an answer cannot be given definitely. The only trouble is, we have to make a guess or an assumption as a working assumption.

MR. PRENTICE: I should think you would have to ask the A.E.C. and the military establishment.

MR. OPLINGER: Mr. Prentice has somewhat anticipated me, because it is inevitable that the amount of uranium you use will be tied up in the anticipated or extended application of nuclear energy to the propulsion of aircraft; and unless you have some indication of that, you cannot say whether achieving the ultimate minimum in uranium is going to be a desirable goal, or whether you can afford to, let's say, expend another 10 percent.

From present indications, my own personal opinion would be that the minimum uranium, while it is desirable in view of the total extent of uranium readily available, is still not going to be the criterion, because I cannot see a very wide-spread or extended application to aircraft, at least from a military standpoint, for some time.

Now, I should not speak as a civilian, but nevertheless that would be my reaction, that we certainly should not try to shoot for the very minimum and yet not achieve too high. You should allow a few percent over.

If there are no other comments on this subject, I would like to interpose a question and ask Mr. Cooper, if I may, to elucidate on the comparison between the machine and the manpower in one respect. It is a very interesting comparison, but was unfortunately so grossly made, let's say, that I think it overlooks an important point in the problem.

The few hours machine time represents what may be weeks or months of preparatory work in setting up the problem that is solved in a few hours on the machine.

Are you making the same comparison when you set it up for 100 men working several weeks? Does that include the preparation of the problem, or is that assuming that the same amount of time has been expended in preparing a problem for that manually conceived solution?

MR. COOPER: Mr. Merrill, would you care to comment on this?

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MR. MERRILL: What were your figures for the preparation time again?

MR. OHLINGER: I was pointing out that whereas a few hours on the machine might represent the solution time, it may be anywhere from weeks to months. I do not know.

MR. MERRILL: That is entirely correct. It would be in weeks.

MR. OHLINGER: Now, in order to set the problem up for the machine, it might take weeks of manpower, manual preparation for the machine solution. In order to set the problem up for manual solution, you have an entirely different approach; and the time consumed may be less or more.

Therefore, I think in giving your comparison —

MR. MERRILL: The time consumed, not man-hours?

MR. OHLINGER: Man-hours or labor consumed, let's say; in giving a comparison you should include the preparation as well as the solution of the problem. I would like a comparison on that basis.

MR. MERRILL: I see. The preparation is in the order of weeks, certainly, but it is not weeks for 100 men. It is weeks for two or three men. If there are any more than two or three men working on the problem, it becomes highly inefficient.

So you still are working with low man-hours. It is probably very true that the ratio of time consumed irrespective of the number of men working on the problem won't be as large as Mr. Cooper would have indicated.

MR. OHLINGER: How would you estimate the manpower for preparing the machine problem compared to preparing the problem for human calculators?

MR. MERRILL: I would say a hundred men — for how many-weeks?

MR. COOPER: Two hundred man-weeks.

MR. MERRILL: Two hundred man-weeks, compared to about twelve man-weeks to set the problem up for the machine.

MR. OHLINGER: And how many to set it up for the manual solution?

MR. MERRILL: It is of the same order of magnitude for that. When coding for the machine, it is as if you were coding for computers. It is a little easier in that you do not have to tell the computers as much. The machines are extremely ignorant on how to compute actually, and you do have to spend more time coding for the machine than you do for the computers.

But it would still take no more than a few man-weeks.

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MR. OHLINGER: Our own interest in the Monte Carlo Method and of course the rather unique facilities there have made us keenly cognizant of this as a means of solution, and so far results have been very encouraging.

But this comparison, I think, does not get the whole picture. That is why I tried to bring it out.

MR. MERRILL: It is not a fair comparison. There has to be some effort on the part of the mathematicians.

MR. MOONEYHAM: There is one additional point I would like to make here. That is, when you code a problem for an automatic computing machine such as the Eniac or the Univac, there is a certain amount of initial effort required. After this, you can run off a huge number of problems just by changing a small amount of the input data. You do not change the formula. You just change your data or the value of some of the variables.

Hence, for a certain investment on these automatic machines, you can get a large amount of results; whereas for a staff of computers, the amount of work is a large sum. Each time you change the data, the work has to be performed.

In other words, what I am saying is, the actual computing time after you set it up is so much shorter with automatic computing machines and the net time for several problems runs to quite a saving on the automatic machines.

MR. OHLINGER: I am not trying to dispute the advantages of machines. We are entirely sold on the idea.

I am merely trying to get the complete picture, because I think the preparatory work is also an important factor.

MR. MERRILL: In the case of a large number of solutions of the same problem, then Mr. Cooper's ratio applies. When you do it once, then it is not a fair comparison. That is the way it works.

MR. WEBSTER: I would just like to add to that the problem that the Argonne group put on the Eniac in getting flux distributions for their pile took about two weeks on the Eniac.

MR. OHLINGER: For solution?

MR. WEBSTER: Yes.

MR. OHLINGER: How long for preparation?

MR. WEBSTER: It is hard to say, because most of it was development of the method, and it went on for a year, I suppose.

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DR. ELLIS: In the calculation of these reactivities, a part of your problem is certainly highly mathematical on the machines, all your integrals and everything. You come out with reactivities in certain fundamental arrangements and parts.

But then, don't you have following a lot of somewhat more elementary calculations leading those various engineering charts you have there, interrelations of all kinds of factors?

Would it ever become possible to put some of those simpler calculations into a machine, analogies, either electronically or mechanically, so that you might be able by using some fundamental results from the mathematical calculations to make some engineering adjustments for various practical features a little bit easier to see all at one time?

MR. COOPER: Mr. Coneybear, you have given that some consideration, the use of actual analogies as part of this calculation procedure. Would you care to comment on that?

MR. CONEYBEAR: I did not quite get the feeling that was the question. The question, as I understood it, was the simplified sort of treatment for calculating piles.

Is that what you meant?

DR. ELLIS: Yes, piles, and the behavior of this pile in the aircraft reactor, the reflector effect on airflow, and so forth.

MR. CONEYBEAR: I think Mr. Cooper has taken some of these rules and made more or less empirical equations and so forth to cover the results, somewhat similar to heat transfer work.

For what Mr. Cooper asked me to say, I think the easiest analogy to follow on this work is. if you consider the number of neutrons to be equivalent to temperature and the flow of neutrons to be equivalent to flow of heat, then you have a reflector that is equivalent to thermal lagging, and so forth.

It is a little easier to conceive if you are not accustomed to dealing with neutrons.

MR. COOPER: I think the answer to this question is, it is a good point and we have not studied this thing thoroughly, and we should give it more attention.

As I said, there are any number of ways to present the results of these criticality studies. What we have done is just consider criticality requirements for a range of sizes and free flow ratios, and so on. Then the heat transfer thermodynamics people have put in the conditions of thermodynamics and worked up the design studies that way.

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It is possible to introduce heat transfer and thermal stress and that sort of thing right into the criticality studies. There is a choice of a way to do it.

MR. SIMS: Gentlemen, I think that we ought to take time out to have lunch now. We will come back at 2:15.

(Whereupon, the meeting adjourned at 1:10 p.m., to reconvene at 2:15 p.m.)

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AFTERNOON SESSION (2:25 p.m.)

MR. SIMS: We are going to try to finish up here in approximately an hour or an hour and ten minutes.

Mr. Kalitinsky.

MR. KALITINSKY: Before we proceed with the controls section of the report, I would like to ask if there are any more comments or discussion on the reactivity part?

MR. FLADER: During the past two years in these discussions, it has always been assumed, or rather an arbitrary assumption has been made, that the reactor would be round and it would be the same length as the diameter.

I would like to ask if any consideration has been given towards investigating the possible uranium economy if the reactor and the surrounding structure were made spherical?

I have a list of things here that appear to be of advantage in such an arrangement. The volume would be less. You would have better thermal stress equalization. You would eliminate some of the unfavorable ratings as shown in that power distribution curve. You would have lower surface for neutron escape, and probably lower uranium investment.

You know that the mechanical problems and the problems of getting the coating in and out would be severe; but it might be interesting to know what the theoretical answer to that question is.

MR. KALITINSKY: The answer is yes, we have investigated various deviations from the square cylinder. We have investigated the spherical reactor to a certain extent.

The main difficulty there is that you have to have a spherically symmetrical flow if you want even power distribution. If you try to put the flow in more or less from one side and out the other, you cannot have even power distribution because you have then to adjust your power distribution to the passage length. Your uranium investment would be higher with the spherical reactor than with the square cylinder.

Now, to get uniform flow is mechanically very difficult without going to very high porosity because you have to have in-flow channel; then your axial heat transfer channels; and then an exit channel.

Therefore, the porosity of your spherical reactor would be much higher than that of a solid one. That again would tend to increase the uranium investment, but we are definitely considering, have considered, that.

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Also I think one deviation from the standard shape, the split reactor, was mentioned. One of the studies that is under way now concerns rounding off the corners of a square cylinder. Just how far you can go in rounding off the corners, which gives you an appreciable saving in shield weight, and still not lose too much in thermal stress, uranium investment, and so on, which is really approaching the sperhical shape, is the question.

MR. OHLINGER: There is one statement I would like to add at that point. I do not see your point in saying that the neutron distribution would be improved. It will not be improved by the use of the spherical or cylindrical --

MR. FLADER: I did not mean neutron distribution. I meant the surface area is lower.

MR. OHLINGER: You are thinking of leakage.

MR. MOONEYHAM: As you point out, for a sphere, the surface to volume is lower. Hence, the sphere does give you minimum critical mass for a given concentration of fissionable material in a moderator.

The answers we have for square cylinder reactors can easily be converted to spherical reactors at point of interest; so, without additional effort, the information we have can be interpreted in terms of a sphere.

In fact, part of the information is derived from the basis of the sphere and converted to a cylinder.

MR. FLADER: What would be the theoretical saving in uranium use?

MR. MOONEYHAM: It is in the order of fifteen to twenty per cent.

MR. FLADER: Wouldn't that justify a serious effort in making a study of the mechanical design problems?

MR. MOONEYHAM: The mechanical design problems have been studied in the spherical case, as Mr. Kalitinsky points out, but this is really a second order of effect. When you have a factor of four in uranium economy due to the complications of temperature, poisoning, depletion, and so forth, and you only have a factor of, say, 1.2, to minor changes in geometry, and a complication such as cooling channels, where you have non-isotropic voids in your reactor. This leads in some cases to higher uranium investment.

So all these must be considered, but it is a second go-round affair. We are presently studying the effect of separating the halves of the reactor so you can introduce the coolant. It would flow out. We are attacking this theoretically, and we have some empirical data coming out of K-25's

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critical experimentation labs in which we are separating -- I am involved in this work, by the way -- cylinders in uranium solution and finding the effect on the critical mass at various separations for various atomic ratios, and so forth.

So we are doing some work on that.

MR. KALITINSKY: Are there any more comments? Any more questions?

MR. CALDWELL: One question on the split reactor. I wonder if we can clear that up. You would think that you would want to go from the ends into the middle rather than the middle out to get the counter-flow effect. That would be the outer portion, or would it?

MR. KALITINSKY: Mr. Caldwell's question was whether or not there would be an advantage in the split-flow reactor to flow from the outside in, because you follow the natural temperature gradient to a greater extent.

I believe there is. I believe the primary interest in the reverse is due to structural reasons, because you will have to have a baffling and support structure in the center and you want to make that, because it is in a very critical region, out of materials that have low neutron absorption cross-sections. Your choice is wider if they are operating in a low-temperature region.

I think that was the primary reason. But I think that is an interesting point.

MR. MERRILL: I have some more comments with regards to the relative advantages of machines versus human computers. I think Mr. Ohlinger will agree in these conclusions. However many calculations you grant, say, the Eniac, which is the going machine now, per mistake, whether it be a thousand, ten thousand, or a hundred thousand, I suspect it is the order of ten thousand calculations per mistake, you have to search far and wide to find a human computer capable of that accuracy.

Another thing in connection with the use of the machine versus the human computers is the problem of keeping people happy after you have found them. That may be a more difficult problem. It is.

MR. OHLINGER: I am afraid you have my viewpoint wrong. I am not talking against the machines. I am in favor of them.

MR. MERRILL: I just pointed that out.

MR. KALITINSKY: I think we can proceed now with the part relating to controls.

I would like to introduce Mr. Edward Bettis, who is the head of our

Controls and Instrumentation Section, which is primarily interested in the development of controls and the sensory instrumentation that goes along with them, as well as other related phases in electronics, measuring devices, and so on.

Mr. Bettis.

MR. BETTIS: I have some curves here that are quite small, and if any of you want to find a place up closer to the front, it might be advantageous.

I have been accused quite justly in being a little bombastic in my talking, but I would like to say one thing in defense. Have you gentlemen ever noticed how I happen to come so consistently right after lunch, when everyone wants to take a nap? You go right ahead and take a nap, but you will do so in spite of my ranting and raving.

I am going to try to give you the present status of our control picture as well as what has transpired in the past. I make no apologies for the fact that some of this talk is futuristic. Our report was unavoidably delayed in coming out, and this is not a meeting of the historical society. We want to be brought up to date on what is going on.

Another thing; our NEPA #6 Report marks a termination of a kind in the control study. It is concerned primarily with theory and a lot of the practical applications of the control problem have not been treated.

Secondly, this NEPA #6 Report which you have deals entirely with the reactor control itself. We are now changing our viewpoint and our philosophy to some extent; from here on out it will be primarily engineering in nature.

We have left a lot of the theory, pure theory. Also, from here on out, we will be concerned with the control problem of the overall reactor and power plant, not just the reactor alone. The time has come when we must consider the overall picture.

Briefly now, to just give you what is in NEPA #6 and some explanation of it, we are going again to be faced for a few minutes with this old simulator question. I come today, praise be, to bury the simulator, not to praise it. This is the end of simulator, per se, and I am presenting here this oscillogram.

The reason it is so small is that I did not want to fake the thing in. I wanted you to see the actual oscillogram of the simulator, because you know in this power pile control we had to have some idea of the time response of the pile, and for very complex functions of time in excess multiplication factors, and for that reason we built this electronic computer.

It was based, as we have told you time and again, on the theoretical

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pile equations. It is no better than the assumptions on which it is based; no better than the data, the five delayed classes of neutrons and their contribution.

After having built the thing, we ran an oscillogram here, giving it the same kind of treatment that we give the pile equations analytically, namely, a step function.

When we gave it a step function in excess K as nearly as it is humanly possible to give a step function by flipping a switch for a certain period of time, and then flip the switch down again to zero Ke, a double-ended step function, in other words, the solid line that you see here traces the response of the simulator.

We do not mean that it gets up here in zero time, but it simply means with the oscillograph which we used, having galvanometers which resonated 1700 CC, it did not trace. It could not make the picture there.

The round marks which we have on this curve are the calculated data imposed on this curve. The solid line was drawn by the machine, oscillograph; and the round points are the calculated points.

Right here there is an apparent discrepancy when we came down to zero Ke or excess K again. The machine said we should be down here and the calculation said we should be up here.

That was brought about by the fact that in the switching of the simulator, there was a momentary open circuiting of the switch, which in our particular circuit put in minus 0061 Ke, just a peculiarity of the circuit.

So the simulator came down to a lower level than the theory indicated it should, if you did not take into account this peculiarity of the circuit.

At this point we stopped the recording and let the simulator run on until an equilibrium condition had obtained; that is, virtually so.

Now it approaches it exponentially. But after ten minutes we started the oscillograph again, and we find that it is settled down to the value which it should have reached.

So, gentlemen, I hope, with a few appropriate remarks, we inter this for all future time. It is now built up in the form of a useful piece of equipment. We have said it told us the truth about that; we will take its word for any future information which it gives us; and we are saying that this simulator is a simulator of a perfect pile in accordance with the assumptions on which the simulator was built.

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We said we wanted to get information from this simulator about pile response and we also wanted to use it as a machine against which we could check any control circuits which we worked up. This curve here is even less visible to you in the back than the other, but here we put a step request into the control mechanism and said, "Take the power level up by a factor of ten to one as quickly as possible."

We see that we were operating down here; the new power level is here; the control mechanism brought it up in something like three tenths of second, because the limitation there is in the feet of the servo which we had, and held it constant at that value.

This curve here is the trace of the excess K, or actual equivalent rod motion, if you want to call it that, net Ke, net excess, which was necessary to make this step and to maintain this new level.

The solid line is the trace of that, which we got from the oscillograph. Again the dots on the curve represent analytical treatment of the pile equation, where the curve should have been. The top tracer here is a trace of the delayed neutron contribution, and you see that this is the inverse of this, which is certainly as it should be.

The reason that you have to insert absorber rods after you have made a step request is that the delayed neutrons begin making their contribution, and so you have to put in more absorber rods, or in other words, your throttle has to go back to the zero position and maintain your multiplication factor of exactly 1.

So these dots and curves are simply, again, justification of the simulator and an idea of how the control mechanism does function. We do not recommend that we try to make our power level changes of the order of 10 in 1.3 seconds.

We make these studies on this fundamental premise with which we started out in the beginning. We are going to see what the maximum is that we can attain by way of time response without going to rather specialized developmental work in servos and such like.

So we see that we have now finished a pile computer, a simulator, and a control mechanism for that simulator alone. The control mechanism for the simulator is a very, very small standard electrical servo system. Obviously it cannot move control rods in a pile.

The next question was: Can we build a servo mechanism, a power mechanism, which is capable of having the same time response and at the same time have enough power to move control rods?

So we began development of a servo system. It may never see actual use. There may be a better one. But around this magnetic fluid clutch

we built a servo system which we tie on to the thing, and it runs along like a calf after its mother trying to follow the response of the little fast system. It does follow.

The one that we have built has a continuous power production of about 5 horsepower, or an instantaneous power demand or capability of some 20 to 25 horsepower, because of the inertia that you have built up basically in a clutch type of servo system.

This is referred to in the NEPA #6 Report as future work, this power development. We have built that and tried it as far as the magnetic fluid clutch is concerned. There are obviously some difficulties with a magnetic fluid clutch. You have temperature effects in the oil itself, oil cells. You cannot run the thing above a certain speed continuously without getting centrifugal action and separation of the oil from the iron.

That does not obtain if you stay down at reasonable speeds, speeds which are perfectly useable for our control, incidentally.

Again, I want to emphasize that this is just a system. It may never see application. There may be others which may be much better. But we are trying in this control problem to establish certain beachheads, certain things that we can fall back on in the overall solution. Until we find something better, we are going to say that we can, we believe, make use of the magnetic fluid clutch as the follow-up servo in control of a pile.

We have talked about the pile simulator as being an ideal pile simulator. We have not mentioned poison, temperature effects, and such like. It early became apparent that we must take into account at least xenon.

Equations for xenon changes as a function of time are not available, and we did a lot of calculation as to just how xenon would affect the dynamics of control.

I again want to emphasize the word "dynamics," because here again we made some assumptions. The cross-section of xenon is not too well known; but if we were to get anywhere, we had to assume some cross-section. So we took the most pessimistic figure we could lay our hands on and said for our work, this will be, by definition, a cross-section of xenon. It can be modified as additional information becomes available.

We made a study for a typical hypothetical flight of a nuclear powered craft where we started from zero with a reactor, built it up to a certain power level, warm up; then took it up in a short period of time to an extreme power level.

You notice there are no ordinates over here. I will explain them as we

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go along. We settled it back down to the design point power level, or flight power level of the reactor, and then shut the thing down.

We wanted to know, what is xenon going to do in that cycle? We know that xenon comes from iodine, tellurium, and iodine. The natural decay of radio-iodine produces xenon, and xenon has its half-life, its own decay time, and is also knocked out by neutron capture. The iodine builds up to an equilibrium point. If we had not changed this, it would have come on up here to a certain equilibrium point for a given power level.

We see that the xenon is on the same scale as this is down here, but this plot of it is just simply to bring out the variations which are washed out on the scale here. The xenon builds up here; and when you take a power jump like this, you momentarily knock out a lot of the xenon which is there before with the long-time constant of iodine being several hours, 6.7 hours, or thereabout. No more is formed, and so you get a net decrease in xenon rather quickly; and then it begins to build up again.

When you shut down out here, this knockout factor, the absorption of xenon or transmutation of xenon by neutron capture, no longer takes place. So the xenon builds up very, very rapidly.

What we say is this: From these curves it does not appear that xenon will give us trouble in the control of a reactor at the power levels which we presently contemplate for any operation cycle like this. Obviously, when you get out here and the xenon builds up to a very, very high value, it is questionable whether or not you can start up again until a radio decay of the xenon has brought it down to a reasonable level.

In order to study this more thoroughly than just this one set of data which we had to work out analytically, we built again a simulator. The thing I doubt will ever be used any more, because we made the studies which were necessary and which satisfied us that xenon as a problem of dynamic control does not exist. That is the present view, and I think it will be the final view. But in this matter of control, as far as I am concerned, nothing will be final until that glad day when we turn on the throttle and get the power under our control like we want it to be.

Now then, this xenon simulator we operated at a power level here on this same curve. We have the same pile simulator response here, and instead of the net Ke which we traced on the other curve, we have here a combination of net Ke and xenon poisoning.

We find that now in order to compensate for xenon, which is this line right here, the xenon is a politician when you make this power change.

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That is what xenon is. It sits where it is until public opinion makes a move, and then whichever way public opinion goes, it says, "I am going really to jump in the front now and take the lead."

If you want to lift the pile up, the minute you move to lift the pile up, the xenon says, "Ley's take it up, but for good." If you start to shut it down, it says, "Let's take it down."

So immediately, when you start to raise the power level, you have to put in some positive multiplication factor. But as soon as you start in that direction, xenon, which has been present as a poison, is knocked out, and you find you have to withdraw your rods, or insert them, if they are absorber rods. Not only that, but you have to over-do the thing.

This line right here is zero Ke. This little bit of a hump is the stimulus necessary to make the raise in power. You cannot see it back there, I am sure. You can look at it later.

Then this long line down here is the further rod motion that you have to put in to make up for the over-enthusiasm of xenon.

Here again we have put calculated points on the xenon simulator to see if we could take the word of this machine which we have built for future considerations, and it seems to give a good account of itself.

This extreme case was run for a power level, an absurd power level, of 10,000 megowatts, or 10 million kilowatts, up to 100 million kilowatts, a level that we will not encounter at all.

The xenon effect is much less severe. It is a function of power level. On that basis we say we do not have to consider xenon in the dynamics of control at this point. I believe that further work will vindicate it, bear it out.

We have not mentioned anything about samarium in this discussion. Samarium, of course, is a poison, also; but samarium has only about 20 per cent yield of xenon, has more of a cross-section, and it cannot in our opinion alter the picture materially. It does not have a natural decay, and will stay in the pile once it is formed, unless it is burned out by pulsing methods or some such way to destroy it. It will not of itself decline at all.

I think that with that brief discussion, we will pass on to some other rather brief points and then come to the future planning for the control work. Remote control has been mentioned in the past. We know that we will have to have some kind of remote control for this stationary reactor, and for the first airborne one we certainly will need remote control of a kind.

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So we have developed two systems of radio control and telemetering. These are built and are in use. We are just letting them sit until the time comes when we need to use them, or if in the meantime somebody comes up with a much better system, we will be glad to throw these out the window.

But we do have an F.M. link, command link, that has eight control operations possible in it, so that we can exert control over a pile and control of airfoil surfaces when that time comes.

We have a telemetering link that is capable of giving us 12 bits of information on one channel and will be applicable to get back data of all kinds from a stationary pile or from an airborne pile, when that time comes. There is no need to go into it further. It is discussed in NEPA No. 6. It is really of minor importance at the present time.

Number 5, sensory instrumentation, of course, causes us quite a bit of concern; in NEPA No. 6 we say that the program is being initiated along that line. It has attained some measure of success now.

We have a sensory device in a crude form, but a sensory device which is insensitive to gamma radiation, which indicates only the number of neutrons that are absorbed by this methane counter. So we have a device which, since it operates on fast neutrons, can be located without the shield in low temperature regions and still give us, according to Mr. Cooper's statements about the speed of neutrons, sensibly an instantaneous account of the fission rate within the pile.

This thing is certainly in a laboratory state at the present time. It will need a great deal of development, but it is the first time that we have found a sensory device which is insensitive to gamma radiation and other fission by-products of radiations and which gives you a true indication of neutron flux or fission rate at any time.

Now then for future work. It looks as if we have gone about as far as we can go right now with the pile simulator alone in the study of the control problem.

I want to emphasize here that we do not mean by that that we have all the answers to the control problem. We have to wait until we get a little more information before we can go further into some of the things which we have covered; that is, profitably.

We propose now to go a step further in the study of the overall control problem, and we are going to build a mockup, I hope. At least, the plans are to do this. We now are actually going to get some energy instead of just a voltage proportional to fission rate.

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This is a proposal. You can call it a modest proposal or an immodest proposal; it is a proposal that we operate a turbo-jet by heating with a solid mass the gas which operates the jet.

I have shown two here. We certainly will have just one. In the first one, we propose to heat some silicon carbide rods in a heat exchanger, and to let this small turbo-jet, essentially about 1000 kilowatts of energy, operate from this heated air and to study the overall control problems involved.

We are going to make the power input to this heat exchanger behave like the pile by having our pile simulator control the output of this motor generator set.

I do not want to be too much in the future here, but one thing that makes this look like a good proposal to us is the availability of such a power installation, if we can arrange it; because there are at Oak Ridge motor generator sets, a plurality of them, that are at present in standby condition. If it can be arranged, we could use these for this control problem very satisfactorily.

Here is the control setup for this system. There are a lot of question marks on this thing and a lot of things which we have not put a question mark on. They might still be questioned. But there is one thing, gentlemen, which you see on this chart which will exist in the final system -- this throttle.

You might raise the question, the way the draftsman has drawn this problem, if we had a guide or a power capable of moving this, we would use it to propel the craft. We would not need to go through all this stuff.

Anyway, we are going to have a throttle over here which will be in the hands of a man who is going to fly this thing whether he is aboard or whether he is on the ground. This throttle will be completely at his disposal. He can throw in a speed request here instantaneously, as nearly so as he can move it. He can take it up on a definite slope, or he can use some arbitrary way in which he requests additional turbine speed.

For the time being, since there are so many unknowns, we are going to put a governor on his request, a demand signal shaper which will, in response to this request, say, "All right; you have asked for the request, but I will take my time to go up."

This thing will be variable. We can change the slopes of this shape, but we are going to have it under control. So, regardless of the demand that he puts in here within certain rather wide limits, this thing will send a request signal to the pile simulator control which

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goes up on a constant rate and leaves off.

The signal from here represents this. I want you to know that in time it is delayed a little bit. This is zero time and is the same for all these signals. It is delayed a little bit. The pile simulator control is just an unbalanced circuit, a bridge circuit, which begins to crack in excess K as soon as it is thrown out of balance.

This little servo motor starts the pile to respond to this request. It says, "Get up," and it starts going up. This return line here is the usual servo feed-back. It has got to have something to balance against.

We find here that the curve of Ke which this motor introduces may take a variety of shapes. The faster and the steeper this slope, the higher absolute value of Ke will be chosen, and the faster it will get there. If it comes at a more gradual slope, it will not use as high an excess K and it will be a gradually increasing function.

These marks I made are indicated by an envelope here, and the first red question mark we come to here is of importance. We want to know how much we can allow by way of excess reactivity and still maintain absolute safe control. There is a difference of opinion along that line.

One of our consultants, Dr. Hans Bethe, thinks that we should never have a system which would allow more than double zero three excess reactivity at any time. One of the reasons that we want to set up this experimental thing is to find this out. What can we do? If we knew all the answers, we certainly would not build this thing.

After we get this signal in again with a certain time delay, a very small one, as you have seen from some of these other curves, the pile simulator puts out a voltage which is proportional to fission rate. This is a high fission rate; this is low. We are changing from one level to another. This voltage goes into an electrical power control unit here, which operates on the exciter of a motor generator set and gives us an output of power over here, 6,000 kilowatts. We have about 1,000 that we need for one of these particular little engines that we are talking about.

Let us say, for example, the little bogue unit that I believe has an airflow of about five pounds per second, something like that. We have an overall power gain in here of 400,000. Now we can begin to get some power that is comparable in a mild way to the powers we are talking about in these nuclear reactors.

Here again we have the servo feedback loop. Because the output power level of this thing, which can be thought of as a power amplifier, must balance against the voltage output of the simulator, so that now we

throw into this heat exchanger here power quite similarly to the way in which power is developed in a nuclear reactor.

After we get this heated up, we do not know how it will respond. We want to find out how it will respond. Let me point out here what some of you will immediately say, "Well, the pressure ratio of these little turbines is a far cry from the pressure ratio that we have heard talked about."

It certainly is. All we are trying to do is to increase the area of this reactor so that heat-wise there will be a similarity. I mean, per hundred degrees change of reactor temperature, we ought to get the same heat into the air as we would in our eventual reactor.

When I get over in here and talk about heat exchangers, immediately I cam be floored quickly. So I have some boys here who can answer some questions on that. I will just pass over it rather quickly before I make a fool statement of my opinion. I am capable of doing it.

After this heats up, the air from a turbo-jet comes in and we get a speed response. What is it going to look like? How long is it going to be? What is this time from here to her, or rather from here to here? Are we going to have this overshoot? Can we hold it up here?

Certainly we will have overshoot if we try to shorten this time too much. It is answers like that that we want to try to get, and the thing will be helpful if we use extreme discretion in interpreting results.

If we go off half-cocked and try to extrapolate too far, it will be more harmful than it is beneficial. But if we put recorders on all of these signals, a careful study of the thing, I feel it will give us a great deal of information which we do not presently have.

These dotted red lines here are just like the rest of them, and the draftsman forgot to dot one. We have some parameters that come in to modify the request signal. We have asked for an increase in speed. Even if we kept this down very low, the request signal here by our shaper, we still might heat this thing up to a point where it would be wrecked. We are going to have in here a limit on the pile temperature. If it gets up too high, it will limit the request.

It may try to go up at too great a speed, too high a rate. We are going to have a derivative of it coming in. The same is true for the turbine inlet temperature and turbine speed.

It is conceivable that we might not need any of these things later if we adequately control this; and we do not know in what proportion we need them. We have got them in there in red and with a great big

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question mark, because these things are some of the things that we want to study by this setup. It is simply a control problem now which approaches the actual control problem of the reactor more closely, at least the control of a purely electronic gadget.

I cannot over emphasize the importance of not trying to expect too much from this, and also point out the very definite danger of a misinterpretation of the results. But if care is exercised in all of these fields, I am sure that a mockup of this kind will give us valuable data. We just simply do not have data on turbine operation from a heat solid. We have nothing about turbine response and a lot of other things.

It is obvious that we have in here some time lags which we will not have in a nuclear reactor. If we have accurate oscillographs of this function here, and this and this, the time lag, for instance, between here and here is obviously peculiar to this setup. It is an electrical time lag. It could be subtracted from the total.

That is what I mean by careful instrumentation of the thing and watching out for the pitfalls that might be present. So we propose as the next step in the control problem to set up such a reactor system as this, if possible, and to instrument it to the fullest extent in the effort to try to find where we have gone off base in some of our other assumptions on the control problem.

This represents a thermal lag. Of course, we do not have the reactivity effects of high temperature here. That is one thing that is obvious without saying, but I do want to point that out. We are not going to get any temperature effects here. We have felt that over the range where we are going to operate the temperature effects are negative in nature. Obviously, they may not be at the lower temperatures, but we want to proceed with extreme caution when we first heat one of these things. Some of this information in my opinion, gentlemen, can only be obtained when we first fire up one of these piles.

I think that is all I want to take your time with now. We will go ahead with the questions, if you have some, at the present time.

MR. KALITINSKY: We will open the discussion now. Are there any questions or comments?

MR. OHLINGER: I have one, Mr. Kalitinsky. On this sensory device, I take it that your statement about its being susceptible to a neutron range that will not be affected by any other radiation present assumes that you have a constant neutron spectrum which is known, and that, therefore, you are interpreting the rest of the data by virtue of that one position on the neutron spectrum.

Is that correct?

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MR. BETTIS: Not quite. The thing of it is, it is a proton recoil device and operates in a proportional region. The degree of ionization that you get from a proton is so great at the lowest neutron energy that we clip the other and still get it and all above it.

MR. OHLINGER: A second question is in regard to the xenon. At this stage of the game I realize it is too early to say, "We are going to forget it," or "We are not going to forget it." But you indicated a response in the build-up of xenon after shut-down.

Is that rapid enough that an emergency or accidental shut-down in the air, where you wish to resume operation within a short time, will not give you a controllable pile?

MR. BETTIS: You have a double-barreled question there. In the first place, I cannot tell you how fast you can operate the thing if xenon had never been born because of the thermal lag in the pile itself. That may fox you.

As I recall, this xenon build-up does not start -- I want somebody to check me on this, if they remember -- of the order of thirty seconds or something like that. It does not get any order of magnitude at all. There is an appreciable time.

This time scale on the xenon is in hours down here; and in the original chart which appears in NEPA No. 6 in a very condensed form, it is very difficult to get information from it, but we certainly have, say, thirty seconds before it becomes or increases to any great extent at all.

MR. KALITINSKY: I think we might point out again that what Mr. Bettis was referring to is that we believe we should not worry about xenon purely with respect to the dynamics, to the instability of control.

MR. BETTIS: That is right.

MR. KALITINSKY: Of course, we do have to worry about it, and we do worry about it, with respect to the investment in uranium that is necessary to keep the pile operating.

MR. BETTIS: Very definitely.

MR. OHLINGER: I recognize it is an operational problem, but I was just interested in the results.

MR. BETTIS: It reaches a peak that is a constant around eleven hours, something like that; ten and a half or eleven hours, as I recall, and then starts to decay.

MR. MEYER: Have you studied after a shut-down a quick power build-up

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again? I noticed the cutout of xenon in the beginning that the power --

MR. BETTIS: Yes, sir. There are certain very definite things that could be studied there. If you want to maintain control of the pile at all times, after you have shut it down, let us say, there is the definite possibility that you could give it bursts and keep the xenon burnt out essentially. Since you gain more than you lose by bursting the pile, you might keep it under control. Also, this tremendous rise which I have indicated is for a complete shutdown.

If you had a way to cool the reactor satisfactorily and you wanted to try to maintain it ready for additional use, it might be possible to bring it down only to a relatively low level; then your xenon, of course, would not build up to such a large value.

We rather got away from the xenon problem without going into all of these effects as soon as we found that for the type of operation with which we are generally concerned, a short duration and the idea of just reprocessing after a flight or something of the kind, we would not even deal with the xenon build-up after shut-down at the present time.

Today, of course, it must be faced and solved.

MR. MOONEYHAM: I might add that in some cases it is possible to start up after a shut-down when you have xenon activity if you are willing to put in enough excess uranium. In some cases it is not possible.

MR. OHLINGER: That is dangerous, though.

MR. MOONEYHAM: This is dangerous, and I think the strong argument against it is also uranium economy. Would it be worth it to start up at that particular time or wait until a period of half a day and be able to start up then?

MR. OHLINGER: If you are in the air and it shuts off on you, you do not want to wait half a day to start it again.

MR. MOONEYHAM: If you are in the air and it shuts off on you, you have in the order of minutes before the build-up is very great. This again depends on what the history of the pile is, what xenon has built up. If you are near the end of the flight, this would be the most serious.

However, you would have safety factors certainly in control. I think within a period of a few minutes you would not have trouble.

DR. SODERBERG: This control system is capable of having constants and characteristics that apply to a power plant in all respects except the heat engine itself, which naturally would be much smaller, has much

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less heat inertia.

Have you considered the possibility of extrapolating these results?

MR. BETTIS: As a matter of fact, Dr. Soderberg, the calculations we have made now with the silicon carbide indicates that it is very close to the reactor, at least one of the reactors, that is being considered. An actual reactor of the general type you have heard discussed this morning will rise when you put 200 megowatts, 200,000 kilowatts, instantaneously into it and extract no heat at all, do no cooling. It will rise at about 124 degrees F. per second. Some of you fellows check me on these figures back here.

Now, the reactor quotation marks here will rise under similar condition at about 121 degrees F. per second, so that the heat of the inertia or the specific heat per volume -- again I am getting in terms you may shoot me -- is the same as nearly as we can figure out for this and the other reactor.

The difference is that we do use much greater surface here than we contemplate using in the actual reactor. We had originally thought of trying to maintain a one to one correspondence between unit power production per unit volume, but we had to back down on that to some extent.

Now, some people might say, "What about these globar rods? It takes time to heat them up." It does take time to heat them up if you pet them on at their rated power. But you see, we have available here at least six times in this case the rated power for the things; and if we sock it right into them and they can take it -- incidentally, the particular units we are talking about we have tested; they can take that thermal shock, and they will come up in something less than a half a second. After we found it was that short a time, we did not try to determine it accurately in the very, very rough experimental setups that we have tried to date.

DR. SODERBERG: That is very encouraging. There remains, then, merely the speed respnse, for example. That would be different.

MR. MEYER: You could presumably do that analytically, I think.

DR. SODERBERG: Yes. If you get certain results here, they could be modified.

MR. MEYER: That is the point I was going to make. I am quite sure many of the other companies have taken fast time response on the engine for their control problems. We have recorded temperature and speed and everything at very short intervals of time on just the engines.

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PROF. McADAMS: Yesterday I was reading both the Lexington Board and skipping through part of NEPA No. 6. In one or the other of them I saw a figure of the temperature difference between the reactor and heating surface and the air mentioned.

It struck me as somewhat smaller than you speak of. I saw a figure of 100 - 200 degrees F. mean temperature difference between the reactor tubes and the air. Is that one of the latest figures? Perhaps it might have been in the Lexington Report. Maybe it was not.

MR. BETTIS: May I give this, Professor McAdams, as to what we are figuring on here. We know, for instance, that we can run these rods without oxidation up to 2800 -- actually 3100 degrees F. is where they begin to go to pot, and it is a very sharply defined point. If we want to run these up to the order of 2800 degrees, the turbine which we are using, I believe has about 1600 degrees continuous maximum?

MR. SEAVER: Maximum turbine inlet temperature.

MR. SIMS: I do not think that answers Professor McAdams' question.

MR. COOPER: I believe that would apply to a liquid metal cooled. Anyone considering an aircooled has to use higher than 100 to 200.

PROF. McADAMS: How high do you go?

MR. COOPER: Five hundred to 1000, in that order.

MR. KALITINSKY: I think we might just clarify one point here. In this setup we are not trying to duplicate the heat transfer or thermal stress conditions in the reactor, but we are trying to duplicate the thermal response; or, if you take the reciprocal of that, the thermal inertia, thermal lag of the reactor system.

To do that, basically what you need is for the ratio of the rated power per unit, solid weight or volume, divided by the specific heat of that unit to be the same as that in your reactor. You can achieve that with a low pressure ratio unit simply by having a much more surface area per unit volume than you would in the final reactor.

So we believe that with this setup, we can have all the important variables and constants that enter into the control system, be at least of the same order of magnitude as those that we might expect in the final application.

Therefore, we might, from an analysis of the oscillograms of the whole system, extrapolate, with reasonable assurance, to the conditions that we might expect in the final job.

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Of course, if the difference between the constants that we get here and the constants of the final application is very great, more than an order of magnitude, then extrapolation would become very dangerous, and we still have to be on the lookout for that, even with this system.

DR. SODERBERG: You have actually a chance of an adjustment. For example, the speed response can be adjusted by flywheels, unless this inertia already is too large, which is not likely.

MR. KALITINSKY: We hope to be able to adjust that, and also, for instance, the length of the air passages and so on, so that we can actually cover a range of variation.

MR. BETTIS: Of course, as you mentioned this morning, there is a possibility of trying to maintain constant temperature. The principle could be checked with a setup of this kind.

DR. SODERBERG: Oh, yes.

MR. BETTIS: I would like to say just one thing more, Mr. Kalitinsky. We ask for questions, and some of you gentlemen may be too polite to make the comments you really feel.

If any of you feel that if it were not for the airconditioning in the room you would hold your nose about the whole proposition, I wish you would say so; because it would help us in trying to evaluate it. I sincerely would like you to give us your criticisms. I don't care how rugged they are.

MR. BROWNE: I have a question. I gather from reading Report 6 that the uranium investment required for criticality going from room temperature up to the operating temperature of the pile is the order of two to one, due to thermal expansion and due to production cross-section. I do not know what that represents in excess reactivity, but it must be a good big piece.

MR. BETTIS: Yes, sir.

MR. BROWNE: In fact, that is one of the biggest requirements on the control rod. You have to have a lot of control rods to handle that particular problem.

MR. BETTIS: Right.

MR. BROWNE: Is there anything in this approach here that corresponds in any respect to that inherent stability due to the thermal action of the pile?

MR. BETTIS: None whatever. All we can say is, before you can have the

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thermal effect, you must have a change in temperature; and the change in temperature is slow compared to the capability of the control system to follow even if we put in on one rod the sufficient amount of excess reactivity control necessary.

MR. BROWNE: In these simulators and other control investigations you have made so far, that has not been taken into consideration at all?

MR. BETTIS: It has not, no, sir, except that we have recommended it and have studied it for its dynamic effect, and we feel that dynamically it does not apply. If it adds two tons of weight to the control rod and we could not move two tons with our control system, it would enter. Whether it is one that we will use or not, we have built a system that will move the amount of rods necessary to the degree of accuracy required to take care of the thermal effects.

MR. BROWNE: I appreciate that it is an optimistic thing anyway. It is lucky it is there. Probably if it were not for that effect, we would have some difficulty in being safe about operating it.

MR. BETTIS: Unless the point that Mr. Prentice brought up this morning should obtain in other places, too, up at the high temperatures. If we ever get into a regenerative effect, we will be into trouble. That is why I say that, regardless of how much preliminary study we do, we must operate this first actual nuclear beast as a critical experiment first, and then as a zero power, but a high temperature thing next, and then approach it very, very carefully.

MR. BROWNE: You can put some of that characteristic that you find in a pile in the electrical circuit.

MR. BETTIS: We can, yes, sir, if we knew better. It is difficult. We can again build in some changes in the cross-section with temperature and can circulate them; but the actual data that is known about the change in fission cross-section with temperature at my last contact with it is not too dependable. I do not see how it could be very dependable because no high temperature piles have yet been operated.

But you are exactly right. We can put in, within rather wide limits, those effects.

DR. SODERBERG: Would that be in connection with the temperature of the pile back to the simulator, then, just another control room?

MR. BETTIS: That is right.

MR. BROWNE: Does this silicon carbide increase its resistance with temperature?

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MR. BETTIS: It increases its resistance with temperature; but over the range we are talking about, it is practically linear. You see, we never have maintained that we were going to try to exercise control from room temperature to design point temperature. We are going to operate it only over the control range of the turbine.

This silicon carbide has a resistance curve something like this. It is negative at first, and then it goes like a metallic conductor. This range here is below the control range, considerably below where we would try to control. Actually, we start in about here.

MR. BROWNE: If you control voltage from the electrical power control box, the whole voltage, you get some of this effect?

MR. BETTIS: We are not going to do voltage because of this effect. This thing here, you notice the word "computer." This, in its present form, is a servo, a computing servo, whose signal is power. Regardless of the resistance change over here, it multiplies the voltage across this by the current through it, and the signal that we get back up here to balance a voltage signal is an actual power signal to get around this very non-linearity here should it obtain in the range where we are concerned.

So we are measuring actual power. We are balancing this electronic voltage here, which is proportional to the fission rate; we are balancing that not by a voltage out of here but the actual power input to our heat exchanger.

It so happens that the silicon carbide rods which we are going to use, in case you might bring up this question about silicon carbide, are notoriously different. It is hard to manufacture one exactly like the other; and after it has a certain use, if you put in a new rod, it does not behave like the same rod that has had some use.

The voltage and current characteristics are such that we will use a series of parallel arrangements. The parallel branches consist of about 100, roughly, rods, and then four of these branches in series parallel, so that each one of these can be a rugged individualist and still not throw the picture off too far.

MR. OHLINGER: Mr. Bettis, you asked for adverse criticism. I think on the contrary. I would like to say it looks like a very practical system, and I think you are to be commended for it. It is very practical.

MR. BETTIS: Thank you.

MR. KALITINSKY: Would you like to comment: You have had quite a bit of experience in control simulators.

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MR. LUNDQUIST: I think I would say that a system of this type would certainly be an excellent initial attempt at an experimental control; and I very strongly recommend that it get into the experimental phase of this thing as quickly as you can because simulators are good and they are very necessary. We have had excellent results with the use of simulators in connection with gas turbines.

It should only be used to help you solve some of the basic problems and then you had better just as quickly, as possible, try to control the apparatus you are aiming at. I think it is a very good start.

MR. BETTIS: I passed this parallel operation up, which is another thing. It is rather apparent that we are going to have to use a multiplicity of engines and one heat source if we utilize this thing, or when we utilize it.

I know now of no application where units have been so operated. We might get into some violent hunting between the two. We certainly would want to try parallel operation on the thing and see if we got additional problems from operating two power units from one heat source.

MR. MERRILL: Mr. Bettis, I do not think the fission cross-section of the uranium would be so much the limiting factor in making that extra control loop to represent the temperature effect on reactivity as far as the controls are concerned.

I think the cross-section would be well-known. It is probably the cross-section in the other elements present in the pile which are not so well-known that would be the difficult thing to calculate.

MR. BETTIS: The fission cross-section itself.

MR. MERRILL: I think it would probably provide a fairly reliable indication of that temperature.

MR. KALITINSKY: Dr. Selden, would you like to comment, or Dr. Pinkel?

DR. SELDEN: I do not believe so, thank you. This control problem is something I do not know anything about.

MR. WEBSTER: I would like to throw in something in connection with this question over here. In connection with Mr. Ohlinger's question, there would be possibly a good deal of excess uranium to balance this xenon, which might mean a prohibitive number of control rods to hold that balance.

MR. BETTIS: You are not thinking now of the actual control problem but just of getting a pile built with that many control rods in it?

MR. WEBSTER: Right.

MR. BETTIS: Because of the uranium economy and other things.

MR. MOONEYHAM: We mean a pile that is capable of starting up after it is shut down, with the maximum xenon is built up. This is what I mean by it is questionable whether you can have such an arrangement. The control rods become prohibitive, in the thermal region especially.

MR. KALITINSKY: Dr. Baker, would you like to comment on this?

DR. BAKER: I do not have very much to say, except I might like to ask if you have considered any sort of parallel control system from the long line from the throttle to the reactor; and if part of that went crazy, is there some sort of other loop which would have an influence of sanity, say multiple controls, and if one did something violent or some internal system, some sort of internal governor which would throw out insane requests?

MR. BETTIS: That is a point that I certainly should have covered. The whole question of safety of this thing falls into two distinct classes: One, the experimental test-stand reactor; and the other, the airborne thing.

Now then, in the airborne unit, there is no such thing as failing safe. It either operates, or it is damned unsafe, whether is shuts down or whether it blows up.

As far as I am concerned, I think that anything in here can go wrong, obviously. We know that. A better thing is to have duplicate control systems throughout, with the idea that the one takes over, a limiting device; the one which says to do the least drastic operation is the one that has command. That is the philosophy at the present time.

In the experimental test-stand, of course, the safety control should be completely divorced from all of this. They are there and operate on limits, and they care nothing for anything except get as much absorber in the pile as it is mechanically and electrically possible to get in in the shortest possible time, and let xenon go visiting or do what it wants to do.

That is the point, to get it shut down.

DR. BAKER: I have one other comment that I might make. It is probably obvious to all of you. If you have a method of inserting uranium rods in order to start up after shut-down, these uranium rods, if they are not used, are not to the same extent a uranium investment. If the aircraft returns, they are not so hot that they cannot be handled. If the aircraft is lost, they are lost. But if the aircraft returns and

they are not used, they are altogether useful.

This is a slight simplication of that problem.

MR. MERRILL: Do you propose using these to start up after a xenon buildup?

DR. BAKER: You would have extra fuel rods available which you would not normally insert. In case of emergency, if you were shut down and had to start up, you would use them because you had them and you needed them, and it is a good thing you had them.

If you did not use them, you just carried them along as excess baggage.

DR. SODERBERG: Would that be any appreciable pre cent of the total uranium?

MR. BETTIS: Mr. Cooper can answer that better than I can, Dr. Soderberg. I can only tell you percentage-wise.

MR. COOPER: What would you say percentage for reactivity?

MR. BETTIS: For equilibrium, xenon for hours at the point we are talking about here, it represents about 5 per cent, a little more than 4. That is, if we just compensate it for maximum xenon poisoning, it builds up here, and for any power level, it approaches asymptotically a limit.

Now, this limit is about 4½ to 5 per cent, whatever the thermal utilization factor is, about 5 per cent. It is a fairly large figure compared to 0.06 that you use for control.

MR. COOPER: That might mean that you would have to put in 25 to 50 per cent extra uranium. There is one thing we have not brought up yet directly. If the reactor cools down from the flying temperature, it has a good deal more reactivity available, and we are making investigations playing off the xenon against the increase of reactivity.

That might mean that it is possible to start up the cold poisoned reactor without putting any more uranium in; and the problem then would be the control, to see that you juggled things just right.

MR. BETTIS: We have gone around and around about that. It is almost wanting something for nothing. It is a close race. I would not want to stand here and say positively that Mr. Cooper and I disagree, because I do not feel that strongly about it.

What he is saying, of course, if that if you put in 10 per cent for

temperature correction, that is in there. If the pile cools down and you have got xenon built up, the thing is cold; and you have got that 10 per cent available. Can you get there before it gets hot? Can you use it before it gets hot?

I am afraid it is more or less like pulling yourself up by your own bootstraps. But I have not thought it out, and I would not want to say on this point, in public, that Mr. Cooper and I take opposite views. I just am not sure,

MR. PRENTICE: How much is your pile going to cool down as soon as you shut it down, due to the fact that it has got a lot of heat being liberated there all the time?

MR. BETTIS: It certainly would be hot for a long time, thermally hot, I mean.

MR. COOPER: I will tell you what this study is. We are trying to find out just how much it would cool down. It is combining the thermodynamics for reactivity considerations to see if we ran all the turbo-jets at part power or part of the turbo-jets at full or part power.

When you are through with the flight, then you might either cut off the power altogether and just run on after-heat; or bring down your reactor power, according to some schedule.

It is pretty darned complicated, and we do not know the answer, but we are fooling around with it.

MR. KALITINSKY: I think, as far as your question on the temperature of the pile after shut-down, that can be controlled within pretty wide limits, depending on what cooling system you use.

MR. PRENTICE: That is what I wanted to say. What are you going to do with the pile after you shut down to keep it cool?

MR. KALITINSKY: That is still under investigation. A possibility I think we mentioned before is to have a small auxiliary steam plant which would windmill a few of the compressors, and the power for the steam plant would be supplied by the after-heat.

That would also be used as a starter. That is one solution for it.

MR. PRENTICE: You would have to cool the pile down by some external means?

MR. BETTIS: Definitely.

DR. SPEDDING: What happens if you have a cooling failure? You

[redacted]

immediately shut your pile down. You still have got enough heat there to be disastrous.

MR. KALITINSKY: It is rather marginal. I believe we figured that out, that if you integrated the whole after-heat production and figured that you just throw that in right away into the pile, it would reach temperatures that these moderator materials that we are talking about can definitely withstand -- they are still below their melting point, if I am right. But we do not know what it would do to some of the coatings and structural materials, and so on. It would probably damage the pile.

One of the answers to that is the use of a direct cooled open cycle with several turbo-jets and check valves, so that a cooling failure is quite unlikely in that system. You can see that if one unit fails, you still have plenty of cooling available from the others.

MR. OHLINGER: That still would depend on the operating history before the shutdown.

MR. KALITINSKY: Well, you would always assume that you had equilibrium. That is the worst possible condition.

MR. MERRILL: With the turbo-jet and a plane in the air and on the ground, and if the power shuts down and you go into a strong glide, is the cooling effect as strong as if you were operating under power, or is it half as strong, or a small percentage?

MR. KALITINSKY: It is a small percentage, ten or twenty per cent. What would you say?

MR. GERDAN: Windmill speeds could be as high as 20 per cent of the maximum rated speed.

MR. MEYER: The density of the pile coolant would be down, too.

MR. KALITINSKY: And if the speed has gone down 20 per cent, the flow would be down quite a bit below 20 per cent.

MR. MERRILL: After-heat would be below 20 per cent of full production, too. So in the case of the plane flying through the air, there would not be any trouble with the pile melting with an open cycle. Is that the idea?

MR. KALITINSKY: After a few minutes, your power production from after-heat is quite a bit down.

MR. BETTIS: There is one thing, Mr. Kalitinsky, I failed to bring out here. There are a lot of things I will think of later.

[redacted]

One of the important things of this set-up, as far as I am concerned, is that it lends itself to a test to destruction, which we cannot do with a pile. If we want to know how much excess K, we will push and push; if we just had a nuclear reactor and say, "This looks as if it is far enough."

But this thing we can destroy and see what actually is way too much and get a much better idea of our eventual safety factor that we determine on.

Then also this thing can, I believe, later take the various forms. We can change it. I mentioned only one. If it were desirable to study another system, not the open cycle system, it could be adapted to some study of an open cycle system or something of that kind.

The point is, we certainly want to wreck one or two of these, and I do not want to wreck a reactor.

MR. PRENTICE: I might make comments that our engineers are working on similar control problems. I think we can sum it up best this way, by saying of course, any two groups of engineers will come to different solutions of a problem.

I think you people do have access to our solutions of the problems, so there is no point in bringing up those differences. Our fellows feel that some of the things you have done may be the hard way around; but, again, that is a matter of engineering technique, not a matter of principle.

I think one of the most important things about this whole problem, at least in our opinion, is a problem which can be solved without any question. We do not feel that the question of control, either of the reactor or of such a system as this, is one in which there is anything other than simply to solve the problem.

MR. BETTIS: That is correct.

MR. PRENTICE: It is not the main question on which this project hangs, by any means.

MR. BETTIS: Not at all. We agree with that.

MR. PRENTICE: You have introduced here a very significant thing which is a demand signal shaper. If you find that this entire system does not react as you want it to, you can change the demand signal shaper to prevent a man from calling for something that throws the thing into trouble.

That is a very important element of flexibility in your system. That

accomplishes in a straight-forward automatic one-man control what we do in a critical experiment of having two men call for a particular change in reactivity before the pile is allowed to do that, to prevent a man from making a mistake.

Your demand signal shaper prevents the operator from making a mistake here. I think that is a very important element of your system.

It seems to me that this phase of the job probably is much farther ahead than almost any other phase of your reactor work, because primarily it is susceptible of straight-forward solution with known techniques.

MR. BETTIS: Right. I want to say just one more thing, and then I will sit down. I failed to mention about the simulator.

One thing that we did want to do sometime, whether we run a critical experiment or whether somebody else runs it or whether we have a pile or somebody else has it, I would like very much to have the fission rate of a pile feed into this simulator as a demand signal and let it trace its Ke. Do you see what I mean?

Then that would give you the net Ke of the operating system from which you could extrapolate your poisons and temperature effects and things in a way that I do not believe is possible to do now. Invert the thing and use it as a net Ke computer.

MR. KALITINSKY: In other words, analyze the operation of a real pile on the basis of a simulator.

MR. BETTIS: That is right, to gain full control information that way. We have not been able to do that yet, but as things progress I hope we can.

MR. PRENTICE: I presume you people are familiar with our theoretical solution to the kinetic pile equations. We do not have a pile simulator as such, and have used analysis to solve the same problem. Do you have access to that?

MR. BETTIS: Yes, sir, we have.

MR. KALITINSKY: There is one point, I think, in connection with Dr. Baker's remark that might be brought out, in connection with safety.

In the actual control system of the proposed power plant which corresponds to this schematic, there is a much more direct connection between the control motion and the pile than appears here from the fission rate indicator. That is why we are anxious to have a good fission rate indicator, because that is a much more direct connection.

If you analyze the problem from the point of view of safety, the things that can do damage are the engine speed; you can blow up the engine; the turbine inlet temperature; you can burn up the turbine; the reactor temperature, which can burn up the reactor.

Those are the three basic things that can go wrong, and therefore, if we use those three as the control variables for the control system, we obtain a maximum of safety.

If you probe deeper into the reactor temperature, you find that the original effect there is actually fission rate. So we might add the fourth, direct measurement of fission rate.

I think by analyzing it from that point of view, "which are the variables which can actually cause damage?" and using them as directly as possible in the control, we will attain a maximum of safety.

Mr. Caldwell, do you have any comments?

MR. CALDWELL: No, I do not have any.

MR. KALITINSKY: Mr. Flader?

MR. FLADER: No.

MR. KALITINSKY: Do you have any comments, Mr. Bachle?

MR. BACHLE: No.

COMMANDER BLISS: I was wondering what considerations have been given to a reactor at constant power levels? There would be a lot of advantages to that constant temperature operation.

MR. KALITINSKY: One of the advantages of constant temperature operation, but not constant power level, is that we also eliminate the transient thermal stresses.

COMMANDER BLISS: If you eliminate that, you do away with most of your stress and thermal shock.

MR. KALITINSKY: That is being given quite a bit of consideration. We hope to test it out actually in practice on this mockup system.

However, there is quite a problem there of controlling the turbo-jet system, because how do you initiate the power increase in a system like that?

COMMANDER BLISS: I said operating at constant power as far as the reactor was concerned. I do not know exactly what kind of a system you use.

MR. KALITINSKY: I do not believe you can do that because you cannot withdraw the heat, you see, if your turbo-jet is not operating at a corresponding speed.

COMMANDER BLISS: Operate everything under the same conditions through your turbine and judge your power by deciding what you want to do with your air from there on.

MR. KALITINSKY: We are definitely considering that.

COMMANDER BLISS: In conventional jet engines they have been able actually to cut their thrust down to where they are getting negative shots.

MR. KALITINSKY: We are definitely considering that. There is one difficulty there, however. The heat release in the reactor between takeoff or full power operation at sea level and cruise conditions at altitude is quite considerable. There is quite a big difference there, and a jet spoiler might not be able to take care of that difference efficiently, you see, although that is, of course, a slow change in power.

MR. MERRILL: I would like to know how long it will take to get that thing operating and how much will it cost, roughly, if anybody is prepared to say.

MR. KALITINSKY: Do you mean this?

MR. MERRILL: Yes, Mr. Bettis' system. Are there any time estimates?

MR. KALITINSKY: I think if we can go through as planned, we can have it operating in a few months, maybe five months.

MR. BETTIS: The biggest thing is whether or not the MG sets at Y-12 become available. They are there doing nothing and could be used. That is the biggest expense and would be by far the most time-consuming thing if we could install it. Frankly, if we had to buy and install such equipment, I would review the whole problem and wonder if it were worthwhile. That is my opinion.

The turbo-jet units, this little one that we are talking about, that we are looking at, look like to me to be the most feasible.

We have a price on a new unit. I do not know whether I should mention things of that kind. It is not secret. It is of the order of $27,000, if we buy a new one. They have some that they think are good for several hours of operation, 50 or 100 hours of operation, used ones that we could get at a very much reduced figure. There are some other units that are kind of toys. We could use them if we had to. They come much cheaper, about $3,000. They look as if they would do the job.

What we would like to do, Mr. Merrill, is not get an engine of questionable

performance, where we would have to be spending all of our time keeping that thing from getting into trouble. We would like to eliminate that and get a dependable engine unit. We do know that we have one that studies at the present time indicate would be satisfactory, and that is the price of the new unit of that kind.

MR. GERDAN: I might suggest for an engine unit you consider the use of turbo supercharger bootstrap arrangements. We have operated those very successfully up to 1800, 1850 on materials problems.

MR. BETTIS: Now you are getting me out of my field again. I will ask you boys to interrupt me if I make a misstatement.

Since I am up here I will say that the main difficulties in that unit which we have looked into definitely seems to be the narrow range of power over which it will operate dependably. It seems to be subject to surging problems and things of that kind.

The other unit is much preferable as far as the range over which it operates, although that is the other unit.

MR. KALITINSKY: What we really want is plenty of reserve power for response so that we will be safe. Basically the units we are considering are modifications of the bootstrap. They use turbo superchargers. Anything above that is just out of the class of electrical heating.

MR. GERDAN: The costs would be reduced considerably with the bootstrap arrangement.

MR. KALITINSKY: Are there any more comments or questions?

MR. BROWNE: Are they moving these tests over to the electrical apparatus, or moving all these machines?

MR. BETTIS: Oh, no, I played nursemaid to those machines all during the war, and I do not want to move a one of them.

MR. KALITINSKY: They are pretty big units.

MR. BETTIS: They are enormous. We would not have to move in much additional equipment. This is relatively small and there is plenty of area. We would not have to interfere with anything over there.

We would disconnect the busses that go from these units at the present time and put in our return busses to our equipment; because all the rest of this stuff, this power control unit here, all the rest of this stuff we have in a fairly advanced state, most of it completed entirely and tested thoroughly.

MR. KALITINSKY: If there are no more questions, I think we can take a five-minute recess.

(A short recess.)

MR. SIMS: I would like to request that each of the member companies that received this NEPA No. 6 Report, after they get through digesting it, write us a letter and give us their comments about the report and about this meeting, particularly any comments that might be pertinent to improving any subsequent meetings we hold.

I do not want to put any definite time limit on the receipt of that letter, but let us say thirty days from now. We will have by then, in approximately one week, the verbatim transcript of this particular meeting. The verbatim transcript, plus your letters, I think will be very valuable to us towards planning and continuing the work.

At this point I would like to ask Gordon Simmons to summarize the important points in the technical discussions that went on today.

MR. SIMMONS: I feel at a little disadvantage, gentlemen. I just got back from two weeks in Cuba. The boss has finally caught up with me and put me to work.

There are several points I want to touch on. Mr. Ward outlined to you the important milestones of the project during the past year, and one of the important things that has come about has been the emergence of the engine component as an entity.

We have abandoned the idea of utilizing any existing power plant in the nuclear power package. This has come about as a result of certain peculiar demands of the chain reacting mechanism on the power plant.

The pile affects the engine. The engine affects the pile, and we find that we are forced to develop a new engine component which is compatible with the demands of a chain reacting system.

This component is no longer separate from the pile. It is a part of the entire power plant design and both of them are very closely tied together, much more so than when we were thinking of using existing engine components. I think that that point should be emphasized in our thinking.

We have had some discussion of the tug-tow approach to a solution of the problem, and it has been pointed out that this stems from a desire to reduce to a minimum the shielding problems inherent in this application.

The Lexington Project certainly had this in mind in emphasizing the tug-tow system in their report.

However, the tug-tow system is, in a sense, another guidance system for an uninhabited aircraft. You remember in the beginning we gave considerable thought to the possibility of a guided uninhabited vehicle to deliver bombs. This is still another guided system. We are planning to continue a study of that and to ask the Air Forces to make certain experimental tests involved in the towing of aircraft at high speeds.

In Mr. Cooper's presentation and in Mr. Bettis' presentation an unusual combination of variables that are inherent in the control system and in the nuclear dynamics of a chain reactor were brought out. I think that Mr. Cooper's presentation is quite analagous to some of the evolution of our thinking in presenting a simultaneous picture of a large number of variables.

If you will remember, Mr. Cooper first showed some graphs illustrating the behavior of single parameters in terms of uranium investment, the effect on uranium investment of the moderator, of the reflector, and the free-flow area of the reactor core.

He then showed a composite picture showing the simultaneous behavior of several of these variables in one graph. The reactor maps that were developed and included in NEPA Report No. 6 show the simultaneous behavior of a large number of parameters that must be considered together to arrive at an optimum compromise in our design specifications.

This path leads to the full effect maps that Mr. Cooper showed you as part of our future working program.

The Monte Carlo Method which was described by Mr. Cooper promises to become one of the most powerful analytical tools, not only for reactor work but for shielding work. At the present time we have a program going on at Northrop involving application of the Monte Carlo analysis methods to shields.

I think that Mr. Cooper did an excellent job in his description of spinning the roulette wheels to predict the behavior of neutrons. We have high hopes for this method and feel that some day it may even replace the old-fashioned crystal ball and be capable of predicting activities on the political levels.

Many things in the NEPA Project are quite a bit like the blind men and the elephant, if you remember the old fable.

One group of blind men will feel the project and say it is a reactor. Another group will feel around and say no, it is a high pressure ratio turbo-jet. Another group will feel it and say oh no, it is a shield.

We are still working on this problem trying to decide which one is

right. I think they are all right and that the thing must be carried on as an integral effort.

In simple language, we might say that from the point of view of the engine, the reactor is a heat exchanger which is capable of releasing into the working fluid a sufficient amount of heat energy to operate the power plant at its rated output.

If we look at the engine from the point of view of the reactor, it is simply a cooling system that is capable of furnishing sufficient quantities of coolant to permit the reactor to operate at a definite pre-determined power density.

From the point of view of the control system, the reactor component and the engine component are two somewhat incompatible bedfellows who must be kept in line.

In addition to the intricate control and reacticity relationships that have been described, there are many other factors that must be handled as simultaneous variables along with the ones that have been presented today.

For example, the internal geometry and the nuclear physical requirements of a chain reacting mechanism cannot be separated from the aero-thermodynamic operation of the complete power plant. This involves the free-flow area of the reactor, the heat transfer surface, power distribution, and distribution of fissionable material within the reactor; and the selection of the specific materials of which the power plant is constructed. Any changes in any one of these parameters affect all of the other variables.

The selection of specific materials, flow area, pressure ratios, operating temperatures, determines and affects the ratio of the number of structural atoms and moderating atoms and fissionable atoms that may be contained in the system.

Likewise, in the final analysis, the quantity of fissionable material that is available for use in aircraft reactors has significant bearing upon the gross weight and performance of the final article.

So, in order to produce an intelligent design, it is necessary to consider simultaneously the nuclear physical requirements of a chain reacting pile in relation to the thermodynamic requirements of an aircraft engine; and to also consider the materials and the required properties of structure, matrix, and fissionable compounds that will fit together into a coherent unit which is capable of performing the task at hand.

In order to accomplish such a design clearly requires a unified

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responsibility with a working group who is capable of handling these aerodynamic, nuclear dynamic, metallurgical, structural, thermodynamic and power plant design problems, and integrating all of these elements into an optimum compromise capable of meeting the tactical requirements of the U. S. Air Forces.

This approach has been one of the basic foundation stones of the NEPA Project since its inception. Although this ideal working group has not yet been achieved, definite progress has been made in this direction; and it is hoped that this progress may be able to continue in the future.

MR. SIMS: Mr. Ward, can we call on you for any closing remarks?

MR. WARD: No, I think it is late, Admiral. I think everyone is anxious to go. I am myself, if you are not.

So my remarks will be simply I appreciate very much the attendance here today, as well as the number and kind of people who came.

I think there has been a little more participation in the problem today by the Board of Consultants here and by our consulting members, which I think is very much to the good.

I hope we will have some more of that in the next meeting.

That is all, Admiral.

MR. SIMS: I think at our August 11 and 12 meeting we were delighted to see the participation there, too, Mr. Ward. I think it is improving at every meeting.

Thank you very much.

(Whereupon, at 3:55 p.m., the Board of Consultants meeting adjourned.)

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