This document consists of 327 pages
No. 1 of 1, Series C/C-DEL

[redacted]

410495

DELETED VERSION ONLY

ADVISORY COMMITTEE ON BIOLOGY
and MEDICINE

-on-

January 13, 14, 1956

CLASSIFICATION CANCELLED w/deletions
~~OR CHANGED TO~~
BY AUTHORITY OF S.G. Hawthorne, OC, DOE DATE 7-13-81
BY Memo 4/22/81

-at-

U.S. ATOMIC ENERGY COMMISSION
NEW YORK OPERATIONS OFFICE
HEALTH and SAFETY LABORATORY
70 Columbus Avenue
New York, New York

BEST COPY AVAILABLE

[redacted]

[redacted]

DR. FAILLA: The first item of the Agenda is a dissertation by Mr. Brown.

MR. BROWN: Thank you, Mr. Chairman. I think the last time that I was lead off man was in a relay in grade school, and as I recall I had an awful time.

I'm sure you all recall the story that was popular during the last War, the point of which is, there's always someone who doesn't get the word.

I thought that after the last meeting we had fairly well laid to rest the matter of conflict of interests and anti-trust laws as it applied to consultants and advisory committees in the Commission, but almost momentarily, after I returned to my office after the last meeting, there was a document which proposed that the Commission send inquiries to each consultant inquiring in some detail about the financial interests and financial background of business interests and the like.

I think I should say at the outset that what we are concerned with here is not fire, but water".

I understand that sometimes more damage

[redacted]

[redacted]

is done by water in putting out a fire than in the fire itself. The Commission is generally concerned as this committee is and as the staff and the division is concerned with the situation that exists where employees and consultants have interests which are inconsistent with the job which they are given to do in and for the Commission.

The problem however is to keep the defence from overcoming our paramount objectives in doing a good job.

We are concerned as I know you are in this respect, but I think perhaps it's the job of the staff to carry this burden.

I am particularly concerned that the matter of financial background and personal interests of consultants and employees, hot be cast into the same contacts as sometimes Security matters are.

When it reaches that stage, anyone who is disinclined to reveal information is presumed to be guilty and I think therefore, it is the job of the staff rather than the committee to carry on this fight at the moment.

We wrote a letter protesting this

[redacted]

[redacted]

move, the basis of our objections were not that the idea is wholly that, not that the idea of being sure that one's interests are not in conflict but rather on the basis of what do we do with this information when we have it?

Suppose a consultant did agree to supply information about his or her financial background, what do we do with it when we have it? What are the degrees of relationship between one's private interests and one's duties for the Commission, that would cause us to feel a conflict of interests existed.

For example, if one had an income from a Trust Fund made up of shares of stock of the company which did business with the Commission, is this a conflict of interests.

There is in your folder a copy of our response to this and you may find it interesting to read.

One of the things that we talked about last meeting in an effort to overcome this apparent trend was legislative recognition for this committee. I don't know whether Bill Berman has arrived or not, but we imposed upon Bill to draft some letters of legislation.

[redacted]

[redacted]

A copy of the proposed Bill will be submitted to Congress this session and that is included in your folder. And it would give statutory recognition to the division of Biology and Medicine.

Now that Bill has arrived, I would like to suggest -- I was about to launch on a discussion of bills, Bill, William and I think that perhaps it would be better if you described what's in it and at the conclusion of that Bill, I would like to take just a few minutes to describe what are, perhaps the advantages and disadvantages of legislature for recognition.

MR. WILLIAM BERMAN: It is a fairly simple Bill. It reads very much like the provision in the Atomic Energy Act for the General Advisory Committee.

It provides for the establishment of an Advisory Committee of Biology and Medicine to advise the Commission in particular areas.

In drafting this we tried to find language that we felt best fitted the scope of the general area for advising the Commission. Any suggestions or recommendations to revise this are more in line with your own thinking

[redacted]

[redacted]

of what your duties are will be very much appreciated.

This language is not fixed yet. Incidentally, the Commission has not approved this bill, it is merely a proposal at this point and so it is subject to change.

Then we go on to provide for seven members of the Committee. There is no particular reason for this except we saw no reason to deviate from your present numbers.

It provides for appointment of the members from civilian life by the President.

This is in line with the G.A.C. provision, a similar provision. Again there's no magic about the way it's done.

We try to keep the whole thing as close to the G.A.C. provision as possible.

The term of seven years we provided largely for convenience. It seemed more sensible to have one man drop off each succeeding year. That too, is subject to revision if anybody has strong feelings about it.

The next two provisions in there are

[redacted]

sort of technical. They merely provide that if a vacancy occurs prior to the termination of the period for which the holder was appointed, his replacement should be appointed for the remainder of that term.

The second one provides that the rotation begins - the first man under the new appointment would drop off or be reappointed at the President's determination on June 30, 1957.

I picked that day because we felt that in the event that the President did appoint new people to this and this is always a possibility, he may not take our advice and under this he has the authority.

The man first appointed would probably be appointed sometime between now and June and July. After the Bill is passed we did not want a man getting on there and serving fifteen or thirty days and then dropping out and so we provided for June 30th of next year.

The Committee will have the right to determine and appoint one of its own members as Chairman. This comes close to your heart because it will avoid all problems of the government Chairman.

[redacted]

There's a provision that you shall meet at least four times each year. You do more than this now, so that should cause no trouble.

The per diem compensation -- we provide for that and nothing more. Here again, there has been some feeling that perhaps the per diem compensation should be increased, commensurate with the position involved.

There is some technical problem about paying - that is paying consultants more than fifty dollars a day. What we have done on that is provide in the back of the justification for the Bill, that members of the A.C.P.M., the statutory A.C.P.M. it is planned to compensate them as we compensate the members of the G.A.C. They are compensated at the rate of one hundred dollars a day.

That is pretty much it. The second amendment to the Act that is included in here is technical. There is an exemption for Commission Advisory Boards from certain of the conflicts of interests statutes.

It doesn't go very far but it does give some exemption.

[redacted]

[redacted]

You have already enjoyed the identical exemption but the only difference is because G.A.C. was specifically named. We felt that if the statutory A.C.P.M. was going to be on a par with it, in a statutory sense as well as an actual sense, that we ought to name the new A.C.P.M. too. That's pretty much it.

It's a pretty simple straightforward amendment.

DR. FAILLA: Mr. Brown, do you want to continue?

MR. H.C.BROWN: Bill said it pretty straightforward, and I think the Committee should know that a very considerable amount of effort and time has been involved.

We are very much indebted to Bill for the effort and help he has given us.

Here is another case where all that glitters isn't gold. The statutory recognition has certain advantages. It builds a "shock infested moat" around the Committee insofar as we are able to ward off some attacks on the question of conflicts of interest, anti-trust conflicts and the like.

[redacted]

[redacted]

So far, most of the committees which have been exempted from the application of those rules have been committees which have either statutory status or which are appointed by the President.

MR. BERMAN: Howard, if I may interrupt. This does not do anything for you. These are the conflicts of interests that did not exist before. No great problem of the anti-trust but conflict of interest which does not change at all.

MR. BROWN: Except insofar as the effort within the A.E.C. and the government to make such things as financial interests applicable, the recommendations within the A.E.C. have been to exclude from application the G.A.C. and the Military Liaison Committee, so in each internal effort to address these rules, not laws, rules to committees, statutory committees, Presidential appointees have been recommended for exclusion.

MR. BERMAN: Sure, it may get you out of the Administrative problem, but you still have the substantive conflicts.

MR. BROWN: This is so. I think also that it will probably increase the effectiveness

[redacted]

of the Committee in non-scientific circles.

I don't think any change can do more than the Committee has already done to establish its value in the scientific field, but in the non-scientific field where the Committee may be less well-known, the statutory committee may materially assist.

I think Bill mentioned the compensation flexibility under this law. There also is language flexibility in terms of per diem and travel and other expenses that are perhaps of interest.

The statutory recognition should also permit the Committee to have a staff if it wishes and certain other privileges that now are accorded the General Advisory Committee.

There are some rather distinct disadvantages. One is that it gives the Committee a closer but I believe a more formal relationship with the Commission.

Secondly, as it gives the Committee more authority it also gives the Committee less internal flexibility in forming its own ground rules as to its operation.

Thirdly, as Mr. Berman has intimated,

[redacted]

appointments are to be made by the President.

If you will read the language you will notice that the seven members are to be appointed from civilian life by the President; it doesn't say seven scientific members.

There's the mathematical possibility - a highly unlikely one - but there is the mathematical possibility that the President at some future date should choose to appoint a layman, or even worse, a lawyer to the Committee. (Laughter.)

MR. BERMAN: This would be tragic indeed.

MR. H. C. BROWN: But I don't think this is a likely possibility.

It is also conceivable that having suggested this, that when the legislation gets into the hands of Congress, they may choose to do their own drafting and put in their own terms and conditions which may or may not be acceptable or palatable to us.

It is also possible that even if they were to accept the recommendations as to accomplish what they want now, we all know

[redacted]

[redacted]

what Congress can do and what it cannot do. So there are these risks.

I think the risks are outweighed by the advantages and I think if the statutory recognition for this Committee which is highly deserved is to be achieved, the timing is good now.

If you have comments which will be helpful to the staff and which would guide us in pursuing this, we would very much appreciate them because it isn't enough to leave it where it is.

A good deal more of staff time and perhaps advocacy before the Congress and testimony before the Congress may possibly be in store.

Bill can shake his head or not, if he wishes, but I doubt if we can leave it right here and so your guidance would be very much appreciated.

DR. H. B. GLASS: I notice that the new term specified in the contemplated legislation would be seven years. The present term is six, I believe. How would this fit in with the present appointment of members of

[redacted]

[redacted]

the Committee? Would these all expire when this legislation goes into effect, when your Committee would be appointed or what?

MR. BERMAN: Well, they would not automatically expire, they would either be terminated - I guess you would all be expected to resign from your present capacity. I am certain that the recommendations as far as the Commission is concerned would be that the same people be appointed to the new Committee.

This is, of course, one of the problems that Howard pointed out. There's many a slip.

MR. BROWN: I'm not aware that the Commission's recommendations for the G.A.C. have ever not been accepted.

MR. BERMAN: I think it's a highly unlikely possibility, but it is a possibility.

MR. BROWN: The thought would be that you would be appointed for the remainder of your term.

MR. BERMAN: What I think we would recommend is taking the men in the order that

[redacted]

[redacted]

their terms now expire and reappoint them for an identical period or with a one year extension.

DR. FAILLA: Is there any reason why there should be a Committee appointed --

MR. BERMAN: That could be done. You get into trouble with interpreting that, I think, Doctor Failla. We would probably be posed with the problem then, as to what distribution the statute requires among the disciplines. I don't know that you want to be tied down.

DR. FAILLA: But can't you make some statement that the people are to be scientists or people with scientific background?

MR. BERMAN: I think in the justification language we have done that.

Well, at one point there was some discussion as to whether we should say, "be composed of seven scientists" instead of seven members.

My own reaction to this, I think was, that at some future time we may want a social scientist on there. I'm not sure

[redacted]

[redacted]

whether that comes within the purview of scientists or not.

It seems to me conceivable that the future might bring a need for an economist, perhaps, even a politician.

DR. SHIELDS WARREN: I think there's a real advantage to having fairly broad language used and certainly the Commission over the years past, when there has been no such story mentioned in the Committee of any sort, has always been very careful to pick scientists for the posts and I would anticipate that this would be safe.

My general feeling is that legislation is a good deal like a, well, the more simply it's worded the easier it is to administer and to work effectively well.

MR. BERMAN: It seems to me that with the future being fairly indefinite in the Nuclear field, you people advising on, helping in safety matters, you may seriously want a lawyer on the Committee at some time.

We don't know what the liabilities in law are going to be in the future in this whole field and you may find something like

[redacted]

[redacted]

that quite valuable and I think it would be easier to leave yourself flexibility now than to attempt to change it in the future.

The statutory language has a certain amount of momentum to it.

MR. BROWN: There's another reason and that is to the extent to which we conform to the language that already exists in the G.A.C., the easier it is to get through. If we change it, it raises problems.

DR. FAILLA: I should like to bring out that there is a division of Biology and Medicine in the Advisory Committee and while we appreciate it, we don't want them to feel that we are pushing this thing for our own advantage and benefit.

At the same time we don't want to be in a position of pushing it, although we think it would be a good thing.

For instance, the Board of Estimate in New York City raised the salary for themselves --

MR. BROWN: This is perfectly true, Doctor Failla and I am abundantly aware of that. On the other hand, the staff does

[redacted]

[redacted]

not want to proceed with this if by chance, the Committee was not in favor of it.

DR. FAILLA: I think everybody is in favor of it, that is, in favor of a change that can be brought about.

DR. C. W. SHILLING: I would like to see a little discussion of the other part of this presentation of Howard Brown's, particularly, the second page where we say that we think essentially the same results can be achieved by the following plan.

I'm talking now of the conflict of interests in which we suggest:

One, we inform each consultant or potential consultant that there is such a thing as conflict of interest statutes, their nature and sanctions for violations, other examples, so forth and so on.

I think, Howard, we would like to hear a little discussion of it because this also, is not a finished issue.

MR. BROWN: No, sir. What we are suggesting is rather than to require information from consultants having to do with their

[redacted]

[redacted]

financial affairs, we inform them of what the problems are, and, in a sense, put the burden on the consultant himself. But the burden is already there by law and I suppose he has constructive notice, but what we are thinking about is informing him as to what the nature of the conflicts are and give you examples -- give examples of the kind of problems that exist where you might run into trouble and to offer the assistance of the staff in helping any particular consultant resolve a problem that he might have. .

We feel that this is probably a more acceptable and reasonable requirement than if you make it mandatory.

This is a recommendation of the division and its status at the Commission at the present time I do not know.

DR. J.C. BUGHER: At the present time it stands as a letter.

MR. BROWN: Officially, that's all we know; unofficially, we understand it had some effect.

DR. BUGHER: What kind?

[redacted]

[redacted]

MR. BROWN: In that the ground swell has abated and I think Bill may have more recent information than I have.

MR. BERMAN: I don't really think I do. I am not sure that there was really a ground swell to start with.

I think most of the divisions, apart from organization and personnel, were not too favorably impressed with their recommendations.

Our best guess is that the Commission won't go along with their recommendation. Just where the thing will end up, we are not certain.

DR. BUGHER: My own feeling is, that consultants are presumed to be reasonably intelligent and capable of discerning wherein statutes are applicable to their situation.

And so I would think that the responsibility of the Commission and the division in particular has been discharged when a consultant is informed as to what the legal problems are, what the legislation in the distance means and the examples I think are helpful too.

[redacted]

[redacted]

In general, it's fairly obvious when a real conflict of interest arises, it is pretty evident that the intent of the law is to deal with real conflicts, not with imaginary ones or with ones that may be unduly twisted to appear to be conflicts.

There always remains some slight element of risk as to the legal judgment for the existence of a conflict on the whole, but I think that is a small risk and need not be appreciable anyhow and I don't see why consultants should not accept such a minute risk.

We do accept risks in various other kinds, and without worrying about them. The proposal here in the last three paragraphs, I would say, would be adequate to discharge any moral responsibility on the part of the Commission.

MR. BERMAN: Along that line, Doctor Bugher, I think everyone should keep in mind that this conflict of interest statutes have been very strictly construed over a period of years. There have not been any prosecutions; in the case where it was, it was usually a flagrant violation where almost any amount of discussion would have avoided it.

[redacted]

[redacted]

So that I think, perhaps, while the situation is a serious one, nobody need run to his own counsel immediately for an opinion.

I think you are all aware of the general direction of these statutes. Just a minimal amount of caution will avoid all the difficulty that might be involved.

DR. FAILLA: Any further discussions? Mr. Eisenbud?

MR. EISENBUD: Oh, I think Doctor Albert is on schedule and then Mr. Butenhoff before we take our recess.

DOCTOR ROY E. ALBERT: I'm going to present a status report on the Colorado Plateau.

There are no pressing - particularly pressing problems associated with it now, but there has always been a rumbling of discontent with the status of the health conditions in the uranium mines of the Colorado Plateau because this is a mining industry which is essentially controlled by the Federal Government and by the A.E.C. in terms of how much it can produce and how much it paid for its product.

We also know that surveys in the mines

[redacted]

have indicated that the radon concentrations are quite comparable to those which have existed, at least in terms of the reported data in the European uranium mines at Joachimstal and Schneeberg, and the average concentrations are in the same order of magnitude and we also know that the lung cancer incidence in those mines was in the order of forty per cent.

However, no steps have been made in terms of setting up any sort of "regulatory" system by the Federal Government.

Recently, that is, last February, a meeting was held in Salt Lake City under the sponsorship of the Industrial Commission of Utah to draft some sort of agreed set of regulations which the seven states particularly concerned with uranium mining would adopt and the A.E.C. was represented and made its suggestions and so did the Public Health Service.

And after this meeting another meeting was held, oh, about six months later with the mine operators and out of this has come a set of recommendations which Utah has adopted and which consist essentially of the fact that the mines themselves have to make their own service and report the values of radar and its daughters and do their own controlling.

Gradually, the other states are adopting similar regulations with certain modifications.

For example, in Colorado they will have the mines checked by the State Inspectors and New Mexico will do the same except their standards will be based on amounts of ventilation per miner rather than radon level specifically and so on.

The only State which seems to have no control measures whatsoever in force is in Washington and approximately some uranium is now being produced around Spokane.

As far as the mills are concerned there are about nine on the plateau at the present time and a large new one is being constructed in Utah.

These are usually inspected by the State factory inspectors and the radon hazards there are not nearly as bad as in the mines. As a matter of fact, probably the major hazard is that of silica in the crushing operations.

The feeling of Duncan Holiday who is the Public Health Service Chief representative on the plateau is that it is probably quite

[redacted]

feasible to control the radon exposures in the mines except in some areas where the uranium ore is in high concentration and there it will probably be quite difficult because of the excessive amount of ventilation required and this would be quite an expense.

The situation was again raised as to the role of the Federal Government in this problem by Mr. Eisenbud with Curtis Nelson, the Director of the Division of Inspection a few months ago.

Perhaps you would like to say a word or two, but essentially, as I gather your point was, that the Federal Government does have a direct responsibility for these miners because of the large measure of control it has over the industry.

The decision as I understand it which is really a tentative one, is to sit tight so far as any direct participation in the control of the exposures in the mines is concerned because.

One, this would be an unusual step for the Federal Government to step into the mining industry.

[redacted]

[redacted]

And two, the States are beginning to take up the cudgels by themselves and the A.E.C. might very well take a wait and see attitude toward it.

So far as the status of the health of the miners themselves is concerned, there have been a number of health surveys going back to 1948 and a very extensive one was made the summer before last in which a Public Health Service came and took a hundred thousand mile tour around the Colorado Plateau and stopped off, oh, the most God-awful out of the way places and picked up a few miners here and there and accumulated a total of about 1300 miners out in the field and examined them pretty carefully.

Of this group they found something
2 in the order of forty to fifty who had some evidence of lung disease. They found some evidence of cancer -- of the types of pneumoconiosis which they did find and tuberculosis were relatively mild.

This past summer the Public Health Service ran another census of the miners with the cooperation again of the A.E.C. in terms of providing vehicles and here again, the decision has not really been made as to what

[redacted]

[redacted]

the type of long range health survey program will be for the follow up of the miners. I think that covers it.

DR. SIMEON T. CANTRIL: Will radon test of samples be done on these people?

DR. ALBERT: No, this was a physical examination in which the people had a chest x-ray, blood count, urinanalysis and a history and standard type physical.

DR. BUGHER: Did the Bureau of Mines have a coal mine inspection service that was essential advisory?

DR. ALBERT: I suspect it does because at the meeting, that is at the Salt Lake City Meeting last February, the point was raised that of all the mines, coal mining industry is the only one in which the Federal Government has any participation at all. And also suggested that if the A.E.C. were to take any regulatory function, that they would undoubtedly be asked by the States to pay for the necessary ventilation equipment.

MR. EISENBUD: Well there's one aspect of this that I think you want to bear in mind and you pointed it out, incidentally,

[redacted]

[redacted]

that it is not at all unusual for the A.E.C. to want to take a regulatory function in the field of health and safety.

What we are doing here is taking advantage of a technicality in the way the Act is worded which may or may not have been intended by the people who wrote the Act which says that the A.E.C. shall have a regulatory function when the uranium has been removed from nature.

This means that after the stuff is blasted and shovelled and mucked into trucks and taken out onto the surface, I suppose, somewhere about at that point we take over.

We have not recognized the responsibility on our part for the miners who are in the mines prior to the time when we actually remove it from nature wherever that point may be.

I am not at all impressed with the objections that have been raised about the uniqueness of our regulatory position in the mining industry because it isn't unique by reason of the history of the Bureau of Mines over the last fifty years and by reason of our activities in other fields.

[redacted]

[redacted]

I would also like to say that the immediate problem is that the tolerance which is being proposed in the A.E.C. regulation simply can't be met in the mines at the present time. So what they want is an exemption to that tolerance and this decision is going to have to be made and be made in the next month or two.

My own view is that these tolerances can be met as a matter of economics and I think here is where our responsibility lies, because I think this industry would not exist except for the fact that we need the uranium.

We set the price, we tell the mine operators how much we pay per pound of uranium removed from the ground and that price is predicated on what we think the cost of operating those mines ought to be plus a reasonable profit.

If the cost of operating those mines as determined by us does not permit adequate ventilation of those mines, we have to change the price. It is as simple as that.

DR. ALBERT: I think there is some argument as to whether or not the radon levels can be controlled.

[redacted]

[redacted] [redacted]

The mine operators at the Salt Lake City meeting naturally got their hackles up and attacked very vigorously and pointed out after some discussions by some mining engineers at the University of Utah that these people obviously had never been in a uranium mine. That their schemes for ventilating uranium mines were based on standard coal mine type of excavation with standard tunnels at certain intervals and nice patterns whereby you can ventilate it whereas the uranium mining industry is peculiar in that the veins come and go and they follow a very tortuous course, so that it is impossible to plan the mine and set up the ventilation accordingly.

This view was challenged however, by Duncan Holliday who pointed out that in a number of cases these people really just haven't tried and it is not nearly as bad as it looks, superficially, except in certain cases where the ore happens to be rich and the flow rate of radon from the ore body into the cavity of the mine is so high and it's relatively inaccessible so that it is very difficult to ventilate adequately.

DR. GLASS: What sort of weekly exposures do these miners get?

[redacted] [redacted]

[redacted]

DR. ALBERT: They vary, but the average concentration in the mines runs something - at times, three to the ten minus nine curies per liter, but I think some of the high values may run seven times this at some of the working places.

MR. EISENBUD: The thing of course is that these values that have been reported out there since war days are identical with the values that come out of Europe in only two places where human experience is available and I think as a working assumption, one has simply got to accept that these concentrations are capable of producing lung cancer in a high proportion of people.

Maybe some of the arguments that have been advanced due to the value of other factors may have played a role in Czechoslovakia, in Germany, but as a working example you have to accept that it was radon.

DR. FAILLA: , Is there very much dust present at the same time?

MR. EISENBUD: Out here at the plateau? There's quite a bit of dust and of course the dust comes from the "adsorbed" dust, but the dust is not radioactive except for

[redacted]

[redacted]

that because the uranium is only present oh, about two-tenths of one per cent. So that the silicosis hazard; it's much greater than the uranium hazard as far as that is concerned.

DR. FAILLA: What do they do about that? Do they wear masks?

DR. JOHN HARLEY: What --

DR. FAILLA: It comes from the impurities to some extent; more radon comes out when it's wet.

MR. EISENBUD: The water that they are using is loaded with radon. You see they take it from underground.

DR. E.A. DOISY: Isn't this the situation that we have to have uranium and therefore miners are exposed to radon and other products?

It seems to me it's our responsibility that we actually do the work ourselves and that it is the responsibility of the A.E.C. that they are protected.

DR. FAILLA: Surveys have been made

[redacted]

[redacted]

which don't mean anything as far as lung cancer is concerned.

DR. ALBERT: The latent period in Europe was something on the order of fifteen or seventeen years and a very small fraction of a group of miners on the plateau, have worked for longer than five years.

Of course, this in itself may be a protection and that is turn of rate is quite high, also probably, the general level of health of the American miners is considerably higher in terms of incidence of lung infection which may have something to do with the incidence of cancer.

DR. BUGHER: There's one other point in here too.

Uranium mining on the plateau has gone on for a long time. It is almost of insignificant magnitude until the improvement in the coloring of the low grade ores made it economic to work these type of deposits and that is relatively recent.

So that the whole problem did not exist in the earlier years in the Commission's life when the uranium supply was largely from

[redacted]

[redacted]

DELETED

As you may recall the present price of uranium is high with respect to the supplies from the Colorado Plateau as compared with the price of uranium derived from the Congo which is, I think, our highest cost uranium in terms of uranium oxide. Is that correct?

MR. EISENBUD: The price at the mine shaft -- well, no, at the mill is higher but of course there has been a lot of talk about the cost of uranium at the mine and when you are buying uranium at the mine this is important.

You can get it for a dollar a pound, say, by way of example. There's no reason why you should pay for two dollars a pound, but the factor of two means very little when you are talking about thousands and thousands of dollars, when you are talking about plutonium.

The extra dollar that you are paying

[redacted]

[redacted]

at the beginning - and there is the fallacy I think, so far as mine ventilation is concerned. While it has a big effect on the price of the ore, by the time you get it into a reactor or into a bomb that differential is insignificant.

DR. BUGHER: But it should raise the question as to whether A.E.C. should be encouraging uranium mining on the Colorado Plateau if supplies from other countries are available at substantially lower costs.

DR. CLAUS: There is another study underway observing the workers in nine uranium mines in which radon concentration has been high and this has been going on -- not the study, but the mining has been going on for some years and I think out of this survey there should be much better information as to the possibility and incidence of lung cancer in the American mining camps.

I'm referring to Michigan. There are some in Montana and, are you doing some in New York mines?

MR. EISENBUD: Those have already been done, but of course the ones we have to get more information on is South African

[redacted]

[redacted]

mines that Roy and Bill are going to survey next month.

DR. WALTER D. CLAUS: But the survey in the Montana mines is well underway I think.

MR. EISENBUD: Yes.

DR. CLAUS: That should give very valuable information.

MR. EISENBUD: What are the concentrations?

DR. CLAUS: I don't know, but several thousand times -- at least several thousand times the factor --

DR. ALBERT: According to some surveys run by the Denver group under "Jacov" in the non-uranium mines, I think their levels ran something in the order of ten to the minus tenth curies per liter.

DR. FAILLA: Isn't that the permissible limit now? As a matter of fact, you fellows multiplied it by three too, didn't you?

DR. ALBERT: Not radon.

[redacted]

DR. FAILLA: I think you multiplied by two.

DR. ALBERT: Everything but radon. The complaint of the mine operators was not against ten to the minus eleventh, but it was against ten to the minus tenth which was very difficult to achieve.

DR. FAILLA: Is there any discussion? I think the A.E.C. will be blamed if something happens later on with respect to whether it is a hazard responsibility or not.

Shall we go on to the next topic which is Mr. Butenhoff?

MR. R.L. BUTENHOFF: This topic was brought over from the last meeting and it concerns the Geneva exhibit and future plans for exhibits in this country.

I'm sure that many of you know that the Geneva exhibit was shown here in New York at the Carnegie Endowment for International Peace Building which was a U.N. Building, was shown back in the latter part of October or the first part of November.

For a two week period we had approximately fifty thousand people going through

[redacted]

[redacted]

that exhibit. A large portion of these people were high school students and some college and university people from the various schools in this area. We had quite a few grade school children also.

The type of audience then that we had for the exhibit was considerably different than what we had overseas. There we were talking to and showing exhibits to the technical people.

Here was essentially laymen and school children. I was extremely impressed with the response that we had from the school children. I think it's extremely important also that exhibits of this type be shown in this country to stimulate interest in the school children to get into sciences, particularly this particular branch of science.

We had many people come up to us and many of the high school boys in particular who have asked, "how do we get into this field; what sort of course should we take in college"?

That is a somewhat difficult question to answer, but it showed that there was a great deal of interest that was stimulated in the students here in New York, in the field.

[redacted]

[redacted]

The biggest problem we had was getting across just a few basic concepts in this new field. It is amazing how few people understand what a reactor really is and it is a very difficult thing to explain -- concepts as to what radiation is and what it can do for you. That is the thing that they were interested in.

How does this whole field affect me. They weren't interested in highly technical discussions about chemical processes or what an atom is or things like that. They want to know, "how does it affect me"? "What is it going to do for me in future years"?

In that respect this exhibit was not too suitable, but we had very excellent cooperation from all exhibit attendants, notably from people from this office here in New York who came to our rescue and really got in there and made a very worthwhile contribution in helping out in the demonstration of equipment and explaining various parts of the exhibit.

The exhibit is still kept in its entirety, it is kept as one unit, most of it is residing here in New York in Bush Terminal.

[redacted]

[redacted]

There have been many States and Cities that have indicated they would like to have the exhibit but the big problem is money. It costs a great deal to take the exhibit as it presently exists and adapt it to particular environmental conditions, room sizes and that sort of thing.

I don't believe that the sponsoring groups here in New York realized how much it really was going to cost.

The sponsoring groups were the Atomic Industrial Forum, Carnegie Endowment for International Peace and the Fund for Peaceful Atomic Development from Detroit.

It would seem advantageous to redesign exhibits to meet the specific need of getting across again a few simple, concrete concepts in this field, and that is the thing that we really need to do to stimulate interest and to explain to people what this whole Atomic Energy Field is about and how it is affecting them and will affect them in the future.

A large part of this exhibit as you may know was in the field of medicine and biology and isotope application and incidentally this was part of the exhibit that most people

[redacted]

were interested in because it really did have some bearing upon their own lives.

They thought that the reactor models were interesting but yet they didn't quite in all cases grasp the significance of what these things were. They looked like other plant models and they probably had seen another industrial fairs.

Our problem that presently faces us is this, "how far do we go in assisting in establishing and running these exhibits in the future."

As far as the Geneva Conference was concerned, we saw a job there to do and we went ahead and did it, but it is becoming increasingly difficult to see how we can provide people and time when we all have so much else to do and this seems rather extracurricular in this division, particularly my branch. Just how far do we go in helping out people who want to put on these types of exhibits or in particular in helping out with this exhibit as it now stands.

This exhibit is scheduled to go to Oklahoma City in April of this year. It is possible that it will go to Philadelphia; there are still some negotiations to be made there;

also, there is talk about it being permanently established in the Museum of Science and Industry in Chicago.

As the exhibit stands there is no point in just showing it to the layman without exhibit attendants to explain what the items are in the exhibit and we need to have people there to keep the scientific equipment that is in the exhibit operating.

One thought was to train people to do this at the various places where the exhibit would be set up.

For example at Oklahoma City and that is the plan for that particular city, they are going to send people to Brookhaven to learn something about one part of the exhibit which is the neutron therapy, the brain therapy, the crop improvement program, the radiation exhibit part of it and other portions of the exhibit which pertain to some of Brookhaven's activity.

This is just one small segment of the exhibit, how we take care of all the other needs is another problem that we have.

Personally it is a large chore and in particular seeing that the exhibit is set up properly and operating properly and it is extremely difficult also for many of the people involved when they see contractive people. I'm not speaking of our own laboratory, but outside contractor people drawing as high as $12.50 an hour for the services.

We feel that there should be some -- either we have extra people to do this job or some particular group in the A.E.C. should be set up to do it, and possibly provide it.

Personally these conditions do not exist and they're doing it as catch as catch can. In essence that is our problem and it sort of summarizes the exhibit situation as it now stands.

DR. BUGHER: What happened to the idea of putting this exhibit on a special train, analogous to the "Freedom Train" that was put on several years ago?

MR. BUTENHOFF: This has been before the Commission for some time now. As far as I know, no action has been taken on that proposal. This again involved a considerable amount of money to put up an exhibit of that type, but I think this is in line with what I was suggesting, that the

4

[redacted]

exhibit should be redesigned to appeal to the general public more than the present situation does and require less people to man it.

DR. WARREN: Was there not some question of getting a portion or a replica of a portion over to Hiroshima for their museum also?

MR. BUTENHOFF: Yes, that was brought up at the last Advisory Committee not at the meeting itself.

As I say, the exhibit is being held together as one unit because of these various possible locations in our country where it might go and I might also point out as a side note, that quite a few of the people that came to the exhibit in New York were quite indignant that an exhibit of this type of magnitude should be shown abroad first.

They thought we, the people of this country should at least know what's going on before we send all this information overseas. That is one of the little things that I thought I should mention.

DR. FAILLA: It seems to me that we should decide what we want to accomplish by these exhibits because they are going to cost

[redacted]

a lot of money and time and in my opinion the most important problem is to inspire young men to go into science, not necessarily in nuclear science but to go into science in general.

To do that we must have permanent exhibits in localities where the people could see them from time to time; by going there once every five years or ten years it really doesn't accomplish an awful lot and we spend a lot of money doing it.

Therefore I would say that the best thing would be to prepare the exhibits directly for young people in high schools primarily and to locate those in strategic places all over the country.

For instance one would be at the Museum of Natural History in New York also at the Museum of Science and Industry in Chicago and at similar places all over the country and if possible, in the rural districts somehow or other either in connection with high schools on a smaller scale, perhaps, say not as large as the one that would be in the Museum of Science and Industry but so that the thing would be there all the time and successive classes of students could be taken in by the teachers and inspired to go into science.

[redacted]

Going around from one city to another I don't think we will accomplish very much this way.

The Atomic Energy is in the public eye, there's always something in the newspapers about Atomic Energy and I don't think it's worth the expense and the trouble in glamourizing this for the general population by just having an exhibit of this type.

DR. WARREN: A very good way of reaching the rural youngsters is through the State and County Agricultural Fairs and concentrate all over on that.

MR. BUTENHOFF: I would like to make one other comment.

First of all I have some slides, but I don't think we have time for them. If you want to look at these after while, I have photographs on the exhibits, what it contains.

Secondly, this is a summary of the items that were in the exhibit and I think it has quite a bit of useful information regarding the various reactor characteristics and the biology and medical part which may be of interest and reference.

[redacted]

[redacted]

The comment I want to make is this; that other government agencies are putting on exhibits in foreign countries as you know.

The U.S.I.A. and more recently, the Department of Commerce and sometimes I shudder to think of the impression that a country must make when people in these agencies who know nothing or very little about atomic energy are in a responsible position to set up such exhibits.

Just two days ago I was approached by a gentleman from the Department of Commerce who is in charge of the Land of Italy exhibit which will be put up in April of this year. He said his boss had told him to set up an Atomic Energy exhibit there showing atomic energy as applied to industry and he had no idea at all as to what should go into such an exhibit or how to set it up.

He knew that he had certain component parts left over from some old exhibit and he was trying to work up something from those.

Now, coming to plan an exhibit with a person of that, takes a great deal of time and we frankly don't have it. And yet, you hate to let the person go out on his own and try to

[redacted]

[redacted]

do it and make a bad impression for this country. That's another problem that we have and it comes up all the time.

DR. FAILLA: Don't they come to the A.E.C. for advice?

MR. BUTENHOFF: That's correct, they go to the technical information people and they then refer them down to other people within the A.E.C.

DR. FAILLA: Do you suggest that there should be a Division of Personnel to take care of that?

MR. BUTENHOFF: It would appear to me that A.E.C. should gear itself up to having these jobs. At present they are not geared for that. As far as I know they have no money for doing this kind of job. They have very few personnel.

DR. FAILLA: This is a matter for the whole A.E.C.

MR. BUTENHOFF: Yes, for the whole A.E.C., but we come in from the standpoint of biology and medicine, which is a most interesting part of the exhibit and the instrumentation and that sort of thing.

[redacted]

[redacted]

DR. DOISY: Has anybody considered the possibility of presenting this through a movie?

MR. BUTENHOFF: Yes, it has been thought of and there's a good way of doing it, but actual items quite often are much better.

For example, if you have an example of your radiation source.

DR. DOISY: But talking of the excessive expense, handling it throughout the country and then it would be available all over the country, that is, if it's a movie it could be distributed pretty rapidly.

MR. BUTENHOFF: There are certain movies available and, incidentally, the people who make cinerama are planning or at least thinking about putting on such an Atomic Energy movie.

DR. SHILLING: Number four of cinerama has been in planning now for about five months and they actually have a large sum of money and the next cinerama that you see will be all Atomic Energy.

[redacted]

[redacted]

DR. FAILLA: Thank you, Mr. Butenhoff. Now there will be a brief recess here and during that time, please express your wishes as to lunch.

(Whereupon a short recess was taken.)

3 MR. EISENBUD: I'll proceed with the presentation of the Health and Safety Laboratory.

The Laboratory has a curious history and I think I will only say a few things about that and then let the program as it unfolds, explain the rationale behind the existence of the laboratory and the overall functions of it.

Historically, the Laboratory began in 1947 at which time it became apparent that the Commission would be doing business with a great many small contractors which in toto would have health problems in very considerable magnitude but which individually would involve rather minor problems of the type which would not warrant the individual plant to staff up and provide its own personnel facilities.

So it was conceived at that time that the Commission should set up in their New York Operations Office a Laboratory whose function

[redacted]

[redacted]

it would be to provide a Field Consulting Service to these small plants and an analytical and instrument facility to assist them on those phases of radiological health program.

The Laboratory was put in New York because this was the headquarters of the old Madison Square Office of the Manhattan District and later of the New York Operations Office of the Atomic Energy Commission whose original function was to procure materials for the piles for the diffusions plants at Oak Ridge for Los Alamos.

During the war we processed all of the uranium in plants operated out of this office, procured the uranium from the mines that was bought for this country; processed it for Oak Ridge and slugs for Hanford.

We procured the beryllium or thorium and so on, including such things as uranium, heavy water, tritium and the other basic materials of the Atomic Energy Program.

Now, the program that we are going to present today is illustrative of the program of the Laboratory as it exists now.

The current program has always got

[redacted]

[redacted]

to be very different from the program either a year previously or a year ahead.

For example, I think it would surprise many of you, that nowhere on this program today are we going to talk about beryllium quartz which is one of the most important problems that this laboratory ever faced from the standpoint of the scientific challenge and the magnitude of the effort required to lick the problem; but this is now ancient history insofar as the current program of the laboratory is concerned.

Similarly, the problems of thorium toxicology, uranium poisoning, are all problems that the laboratory has felt within the past, but these are items which do not have a place on the agenda today.

However, for orientation, I thought that the best way you could get some idea of the diversified interests of the laboratory would be to look at the bibliography.

We have selected some ninety references of works published in the open literature since 1950.

I won't go over this bibliography

[redacted]

[redacted]

now, but it is available to you in order to indicate the kinds of things that have been done during the past six years and the changing question of the program.

Now, hold off on any discussion of the size of the laboratory and size of the budget until the end of the program because I think these things can be better discussed after you understand what our program is.

On the other hand, I do think that in order to understand why we are presenting the program as we are, we should know a little bit about our organization, and I will ask you to refer to a table of organization which is in the material given to you in which you will see that at the present time we have a total of about eighty people, divided into six major Administrative groups, one of which is the Administrative Staff, itself, then an Industrial Hygiene branch whose function it is to assist contractors in the problems of educational hygiene both radiological and toxicological, air pollution and waste disposal.

Then we have the Radiation branch which is responsible for the program of our calibrating survey meters and dosimeters for contractors and also undertakes special

[redacted]

[redacted]

radiological studies both in the laboratory and the field.

Then we have an analytical branch which is responsible for analysis and the counting of all samples for industrial hygiene program, the monitoring network and the sunshine program.

We have an instrument branch which backs up all of the programs of the laboratory designs, builds and maintains various specialized instruments that we require.

Then we have "biometrics" branch whose function is to serve as a central unit facility for the large numbers of data, the large volume of data that the laboratory involves. We deal in huge numbers as you will see, hundreds and thousands of pieces of data are processed through this laboratory each year.

I'm going to ask Mr. Harris who is in charge of the Industrial Hygiene work to discuss two items. The first two will be reversed. First he is going to discuss the general scope of the Industrial Hygiene service to the contractors, then typical of the kind of research which is done in order to get the practical answers that we need to give a good

[redacted]

[redacted]

service in the field.

He is going to describe one experiment of open field burning of flammable wastes.

MR. WILLIAM B. HARRIS: As Mr. Eisenbud pointed out, the Industrial Hygiene job is to provide a service to the contractors of the Commission.

Now, the contractors as you probably know range in size from a single operator on a university contract up to the very large production facilities, such as the feed materials plant at Fernald, and we have inherited in the New York Office, we have inherited those production plants which were previously under the New York Office.

In the smaller contractors we provide them as Mr. Eisenbud told you, with the function that they just cannot provide themselves.

We have a variety of techniques and a variety of skills available which we can make available to them. In other words, we frequently have a staff available to give them the health service which is larger than the staff of the operating contractor and we may provide this service for a period of a half day, a day in

[redacted]

[redacted]

six months or a year and thereby insure that they have the highest possible level of health protection service.

The larger contractors, with them, their primary objective is to help them get their feet on the ground, to help them to the point where they can be self-sufficient.

This is almost now true with the big feed materials production center outside of Cincinnati, the Fernald plant and they are just about reaching full production there.

The type of service which we gave them started with the selection of a site. That is, we provided to the production people the criteria on which the site was to be selected, that is from the standpoint of health. And these criteria were overlaid on strategic criteria, transportation criteria and any others that were used and the site was selected so that it would give us a minimum amount of problem as far as health operation is concerned.

Then, we conferred with the designing engineers with the production engineering people and set for them, on the basis of our previous experience the criteria for the design of the plant.

[redacted]

[redacted]

These criteria included the types of material, handling equipment that must be used, the types of ventilation equipment, the type of air cleaning equipment, the amount of fluid waste cleanup that would have to be provided.

The type and size of storage areas for scrap materials and so forth and right until the plant was actually put into being, we were very close to the designs.

Every drawing that went into the final construction drawings of the plant, first came through this office for approval so that during the early phase, before the operating contractor was in a position to provide a health service, we provided it for him.

We helped him in selecting personnel, we helped him in laying out a health program at the plant for the period of operation of the plant and during the break-in period, that is during the time when the plant was being put into service we provided a hand, we provided equipment, we provided know-how for evaluation of potential hazards. So that when the plant finally went on, the operating personnel were able to function in a way which was, according to the criteria that we

[redacted]

[redacted]

have available, that was perfectly safe.

Another service which we tried to provide is during the actual production phase, during the operation of the plant, we helped the plant evaluate exposures and we provided them with a check service, that is, although they have available to themselves people on their own payroll who are in a position to follow the original plans which we laid down, we go back and re-check them and insure that they are doing the kind of job which is necessary for the efficient functioning of an evaluation program and we are very happy to do this because it provides us with a way to keep track of developments in machines - equipment developments and to maintain our data on exposures.

The kind of evaluation of exposure that we do, I think, might be interesting to you and I have a couple of slides which will describe it better than I can tell it.

Can I have the first slide?

This is typical of an air sample. As it happens this one is being gotten in a beryllium plant and I can tell by the very large sample that we are using which takes a

[redacted]

sample of a cubic meter of air a minute because this was necessary of very low concentrations of beryllium which were of interest but the type of evaluation that we do requires that we get very close to the operator to see how much is in the air that he may be breathing.

Can I have the next slide, please?

This is the type of sheet we draw on each operator in a plant. It is made up of several individual.operations that the man does at which we take samples in his breathing zone as I showed you in the previous slide, plus several locations where he is apt to spend an hour to two and a half hours to three hours a day just walking around pushing buttons and generally managing things.

These are all weighted against the time and the final average figure is computed in the number of times of permissible level that the man is exposed to.

These data are used to correlate with year-in data and with possible illness data in the future.

The next thing that I think might

[redacted]

be interesting is to visualize the extent of an atmospheric survey in a plant. This is one of the feed production plants.

Each one of these represents a separate exposure evaluation. These are the individual people; these numbers represent the number of people involved in each one of these and several bars represent several successive surveys so that the plant management, when they see this can tell how they are doing with respect to time as well as how they are doing with respect to exposure.

Would you run the three of them through in quick succession, Al, please?

This is the major production people in the plant. The other two are similar people but this is one plant, as a matter of fact, one building in a plant complex.

As you can see here, (indicating), they are getting down toward people who are less involved with production, down to the managerial personnel and you can see the way the exposures follow as you get to more administrative type people as it falls off.

[redacted]

[redacted]

The next one, I think is of interest. This is a series of successive averages of surveys that we have carried out. This is for a particular plant and the two curves, one of them is the maximum exposure in the plant, and the other one is the average exposure.

In other words, the maximum average, that is the exposure of the individual who had the maximum average exposure and, as you can see here, this is thirteen thousand integrations per minute per cubic meter of uranium which is on the order of two hundred times the permissible level and the average here about 50 times the permissible level when we first went into the plant.

Shown on here are the various things that were done to correct the conditions.

Here we install ventilation and putting in the "sealing" system. At this point we increased the ventilation because it wasn't quite enough and here we have covered the floor with gratings because this was a rolling mill where the material was being dragged across the floor and a lot of oxide and stuff -- why we thought we needed a grating on the floor so that the men wouldn't pick it up and

[redacted]

[redacted]

as you can see the concentration successively decreased to well below tolerance towards the end of a period.

This goes from 1948 to 1953.

With this data we were able to design a rolling mill and on the basis of the information that we got in this plant, we saved about a million dollars on the control equipment for the new rolling mill which is in the Fernald plant.

I think there is one more slide.

I'm sorry, but the slide is not up to our usual technical proficiency. This merely will demonstrate another way in which the data that we collect are effective.

Here we have plotted the observed uranium in bone. These were actual samples from people who died or from whom pieces of bone were taken and plotted against this is a predicted amount of uranium in the bone and this prediction is based on the air sampling data that we gain during successive surveys, and apply by knowing the operation that the man was involved in, applied to the individuals and the correlation is surprisingly

[redacted]

[redacted]

good considering the manipulations that the predicted data was put through.

I think probably I can swing right into the second part of my presentation which is the open field burning experiment.

I think I can save a little time on this one.

The matter of disposal of waste materials from all of the Commission activities is a very big one. It is big mostly from the standpoint of economics and probably more important because it is very difficult for us to define the hazard which might be involved.

We know that uranium, I don't mean uranium, but radioactive material is very easily measurable and almost anyone can pick up very, very small quantities ofit.

In all probability the danger to the general population or to any individual from handling of uranium is pretty comparable with that from the handling of lead or mercury or other materials like this and yet, no one would even consider that it would be necessary to be extremely careful about disposing of scrap

[redacted]

[redacted]

materials that are contaminated with lead or with mercury whereas, with materials that are contaminated by uranium we have, we feel, a very great responsibility. So that, until a careful definition of what the hazards are is arrived at, we are going to have a lot of expense in disposing of materials of all types.

One type of material that is generated in tremendous quantity is simple, burnable, low level, contaminated waste material from laboratories.

This is Kleenex, absorbent paper and just cover paper and various laboratory gadgets, like clothing, that is gloves and cover clothing and wood materials, rubber materials and in every laboratory operation these are generated in very large quantities and at relatively low levels of activity.

We were more or less pushed into a position of evaluating the necessity for this, by one of the operations wanting to use the Lake Ontario storage area as a disposal dump for this material.

They baled all of their burnable materials and put them in crates about 500 pounds to the crate and shipped them a relatively

[redacted]

[redacted]

short distance, to the Lake Ontario storage space and we accepted them and put them in an old disused building until they got to the point where we just couldn't digest them anymore. It was just tremendous amounts of stuff.

We decided at this point, "well, why don't we see if there isn't some other way to do this"? "Can't we just take this stuff, there's a lot of it and it is not very active, and put it in a place somewhere and burn it; just get rid of it and forget about this business of baling it up"?

Baling and shipping to a storage area is actually a relatively inexpensive way to do it.

In many cases the stuff is loaded with concrete, taken out and dumped.

We disassembled some of these big bales and boxes and had three successive experiments. We were feeling our way. The first one was with 500 pounds of material, the second one with about two tons of material and the third one with about eight tons of material.

[redacted]

[redacted]

We burned them and we checked the meteorology at the time; we checked the reduction in weight, the reduction in volume and I can show you in the next slide quickly, what we found. Very briefly, these are the three successive experiments.

This is estimated activity and it's estimated from the ash and from what we found to be distributed. This was eight tenths of millicurie, two millicuries and seven and the per cent reduction in the ash, that is the residual material by way. We had thirty per cent left, that means there was a lot of glassware and metal parts and in each of these cases approximately ten per cent, so we got about a ninety per cent reduction both by weight and by volume.

However, on an activity basis we got a reduction of about ninety-nine per cent. In other words, only one per cent of it was found to have gotten away.

In the fallout, now this fallout was measured by the same technique that we used in the general fallout measurement, that is the distribution of sticky papers elevated above the ground.

[redacted]

[redacted]

In the fallout we found 2/10ths of a microcurie, total fallout that is integrated over the entire area that was sampled in the first case 20 microcuries and 50 microcuries in the last case.

We also measured the activity in the air at 25 feet away from the fire and in no case was it above four disintegrations per minute per cubic meter either of alpha or of beta activity. The maximum distance as we distributed the stuff and this is distance to actual background measurements with about a thousand feet in the case of seven tons of material.

To give you some idea of the appearance of the experiment, I have a small chart here that will show how it was done. This was a second experiment and we had pretty good wind predictions on this. The atmosphere was pretty stable and therefore we set our sampling array only at a 90 degree arc away from the burning.

The samples as you can see were set out to about a thousand feet in this 90 degree on relatively short arcs.

The entire fallout occurred within

[redacted]

[redacted]

a considerably narrower area than we set our sampling stations out, and we plot here the disintegrations per minute per square foot against distance from the site.

The fallout is pretty good with the end occurring at something like six or seven hundred feet and a plot of the predicted, that is using the theory or the various theories of diffusion, a plot of the predicted fallout falls almost identically on this line.

It's really a very good fit and so we were very happy with this.

We went out and did another one and the other one was not quite as happy as the first. As you can see, the circles on this plot-- oh, by the way, this was done around 360 degrees. We weren't very happy with the wind direction, so that we actually set our samplers up on annuli out on two thousand feet, that is twenty-five, fifty, seventy-five, one hundred, two hundred and so forth on sixteen points around the compass of the two thousand feet. And, as you can see the results got down to a point and then just seemed to stay there.

They looked very curious when we

[redacted]

[redacted]

examined the data and it immediately occurred to us that maybe something was interfering with our analysis.

There was a drizzle and rain throughout the day, and we went back to our fallout information to find out was somebody else experiencing a similar phenomenon. And, sure enough we found that this was done at Buffalo and the cities of Detroit and Cleveland each had fallouts of the order of 500 disintegrations per minute per square foot.

And so we feel a little bit secure in saying that the fallout line was like this and then did in fact, level off because we were hitting a "bomb" degree background.

Deducting this figure, I think we deducted 400 disintegrations per minute per square foot from the data which again gives us a fallout curve which is not nearly as good a fit to the "Sutton" equation as the other one is, but putting the proper pirameters in, it does fit pretty well.

And, so we believe that we have this thing pretty well in hand. That the amount of contamination from such a burning is very minimal. It could not possibly

[redacted]

[redacted]

A VOICE: Would there be any advantage in having a low stack?

MR. HARRIS: Hardly.

A VOICE: Would it increase the exolusion area somewhat?

MR. HARRIS: It would increase the distance away, that the material would be found.

MR. EISENBUD: Boys, the nuisance is from the smoke and not from the activity.

MR. HARRIS: I think this last time that I showed that at about four or five hundred feet we had reached a background which was found to be at Cleveland from tests somewhere. We are not introducing a great deal of material into the air.

MR. BUTENHOFF: Does this increase the type of combination that you find in the ordinary laboratory?

MR. HARRIS: This was a general laboratory waste.

MR. BUTENHOFF: In the case of some

[redacted]

[redacted]

specific operation where there might be more volatile stuff --

MR. EISENBUD: You have to know what you are doing; you've got to know what you've got in the waste.

MR. HARRIS: If there were a particular operation that had a particularly dangerous, particularly volatile material, you have to be concerned about it, but a normal waste material of this type could be taken care of.

MR. EISENBUD: Here's a radiological laboratory which for many years has been baling waste paper at the cost of $250. a bale, shipping it 300 miles for permanent storage whatever that is supposed to be and we asked them to do this experiment on trial, and they said, no, they had public relations problems and they wouldn't even discuss it with us. And so we did it.

In burning these eight tons, it cost them $2000. to package this and the cost of the experiment was very much less than packing this material if they took care of the burning and storing and this is the kind of thing where you see the opportunity to do something where you are concerned, both with the health hazard and also at the same time, the economics

[redacted]

[redacted]

in operating problems of these operations.

It is very important I think, to emphasize the combining of the value of tolerances and how they arrive at them, the methods of sampling and the problems themselves and the economic factors that go into the implementation of them.

DR. WESTERN: What do we do with the ash?

MR. HARRIS; The ash in this case was bulldozed into the ground; it remains relatively low level activity. You see they started with only millicuries of material.

DR. WESTERN: I assume you are looking forward to a general use of such procedure. What would be the general advice as to the disposition of the ash?

MR. HARRIS: If the area were large it could be buried in the ground right there. If the area were small, if it required to get rid of it, you have reduced the bulk and the weight by ninety per cent, so that whatever other procedure would be followed, would be followed for only ten per cent of the initial material.

[redacted]

DR. FAILLA: This other land, who controls it?

MR. EISENBUD: This is Atomic Energy Commission property up on Lake Ontario; it's been up there for many years as a storage area.

DR. FAILLA: You wouldn't recommend this for others to do unless they control the land, would you?

MR. EISENBUD: You see they only had a millicurie or so per bale, and if they burn it during the day, they are discharging into the air something like ten microcuries.

DR. FAILLA: If you're doing that for twenty-five years --

MR. EISENBUD: If all you discharge is ten microcuries per day, you would still be all right.

In other words, if you had it in a test tube you would put it down the sewer would you? But because it wasn't in a test tube, it was in a paper, they baled it up and wanted us to store it permanently.

It contains a very small amount of radioactivity.

DR. FAILLA: The thing that I have in mind, a small amount in one place is one thing, but a small amount all over the United States is another thing. So that there is a limit as to how far you can go with this.

Eventually, if there is a tremendous increase in the use of radioactive material, that would be something to consider.

MR. EISENBUD: This is not a panacea. Well, now it isn't generally realized, but the A.E.C. was well on its way to cornering all of the scrap metal in the country because of the policy that we had of not ever discharging through normal channels of trade any piece of metal which is contaminated with radioactivity. This can obviously only go on for a certain period of time until finally you've got a corner on all of the used metal in the country.

To give you an idea of the magnitude in the problem at Oak Ridge, which has, I think the largest pile, had six million dollars of steel piled up by 1953.

[redacted]

We had some up in Lake Ontario and so we undertook to find out what happens when you smelt the steel and we found, as John Harley predicted, on chemical grounds that the uranium went up into the slag and the steel came out clean.

As a result of this, the mass ferrous stock piles of the Commission were liquidated at a tremendous saving and all on the basis of an experiment that cost about a thousand dollars.

Now, we are following that up with similar surveys because we have tremendous stock piles of nickel, copper, platinum and other metals. Mr. Klevin is going to tell you about that.

MR. PAUL B. KLEVIN: During the course of feed material production at the various A.E.C. production sites, large amounts of equipment came in contact with uranium bearing material.

When this equipment was taken out of service either due to upsets or to disrepair, it was placed into a scrap category.

It was the policy of the A.E.C. not

[redacted]

[redacted]

to dispose of any materials containing levels of activity that may have adverse effects on industry or on public health.

As Merril just stated, there had been a vast accumulation of ferrous steels during the course of years and due to some of the studies made by HASL. The policy of the A.E.C. was set to allow for the disposal of these ferrous steels providing certain restrictions in material - uranium material content were met.

Also, as Merril stated, we have accumulated many millions of dollars worth of non-ferrous material, such as stainless steel, copper, nickel, aluminum. It is our belief with restriction, the A.E.C. policy to cover disposal of non-ferrous metals and also contamination of other than uranium, and equipment for re-use as well as scrap, the purpose of our study was to obtain information which may lead to the liberalization of disposal regulations.

A study was made on copper, nickel, aluminum and stainless steel scrap and the purpose of the study was to determine the amount of uranium picked up by these various metals during service; the effect of

[redacted]

[redacted]

melting these metals on the uranium content of remelt and also the residual radioactivity in the metal ingot.

And also, to determine whether or not the treatment of this material would show any adverse effects on either the public health or industry.

Our investigation consisted of obtaining scrap of the various metals from A.E.C. production sites, such as Oak Ridge, Malinckrodt and other sites and also to obtain samples from the National Bureau of Standards of the same metals prior to the years when the Atomic Energy Program was in existence.

Surface measurements were taken of the scrap with smears of examination of the material were also taken and duplicate drilling samples were made on the scrap and these samples were held for analysis.

The melting of the stainless steel, nickel copper and nickel stainless scrap was done in an Ajax high frequency furnace at temperatures ranging from 400 to 600 degrees Fahrenheit above that of the uranium melting temperature of 2100 degrees.

[redacted]

The slag or dross was separated from the remelt and held for analysis. Thirty pounds of aluminum was melted in a gas-fired furnace at a heat of melt, 1400 degrees. The top inch of the aluminum ingot contained the slag.

In order to obtain the worst possible conditions from a potential dust and fume hazard samples the operations were performed in areas where no ventilation existed except for natural draft.

Air dust samples were taken directly from the remelt crucibles to obtain the worst operating conditions.

After remelt, surface measurements were again taken with alpha meters; smears were taken of the ingots and slag where possible and duplicate samples were taken of the ingots and the slab, duplicate drilling samples.

Could I have the first slide, please?

In the nickel, we have here the contaminated scrap, the remelt nickel, the slag and the National Bureau of Standards

[redacted]

sample that was obtained, produced before 1942.

In our surface measurements with the alpha meter, it showed that we had an average alpha contamination of approximately 12000 per hundred centimeter square smears taken and analyzed assimilation count showed that we obtained a smear, a removable contamination of this figure. (Indicating)

Analysis of the duplicate drilling averaged 2900 and 25 microgram units per gram of nickel. A remelt showed almost negligible of contamination on the alpha meter.

A small amount of removable alpha as evidenced by the smears and a value of remelt residual uranium contamination of 1.25 micrograms U per gram of nickel which is comparable with the pre-produced nickel obtained from the National Bureau of Standards of 2/10.

The top surface contained 1.1 micrograms U, while the average of the samples was 1.25.

Next slide, please.

The nickel stainless melt was composed of ten per cent nickel, ninety per cent

[redacted]

80

[redacted]

harm anything anywhere, and if you've got just a little bit of area - this is a thousand feet, if you got just a little bit of area, it is possible to burn a tremendous amount of stuff; seven tons of material at one time is a lot of stuff.

We feel this is a very good way to get rid of this material.

As soon as we get our things in order, we will recommend that this be done and that these laboratories that have this large quantity of material, just forget about it and burn it and not be so concerned about spending money on it.

DR. FAILLA: Did you say that 99 per cent of the material remained in the ashes?

MR. HARRIS: Ninety-nine per cent remained in the ash.

DR. FAILLA: This fallout was then from the other one per cent?

MR. HARRIS: From the other one per cent; that's right.

[redacted]

7

stainless steel. The contaminated nickel was again of the same amount used in the previous study and that contained the bulk of the uranium contamination.

In the remelt we found readings that were negligible in the nickel stainless steel and the residual radium contamination was the same as that found for the pre-Atomic Energy produced N.B.S. standard as was expected. Most of the contamination came off with the slag.

Next slide, please.

The stainless steel remelt, in that we found a similar situation. In remelting the stainless we found that our sub-surface sample was comparable to the sample produced that we obtained from the National Bureau of Standards.

The major bulk of the contamination came off in the slag. We found in the top half surface some slag of contamination of 50.5 milligrams of uranium per gram.

Copper samples were obtained from Oak Ridge and put in three batches; those that showed alpha contamination of 800, 4,000 and

12000 as evidenced by the alpha meter readings and these contained an average uranium contamination of 1,663 micrograms per gram.

The remelt copper showed negligible surface activity and, as we expected the remelt copper showed a very small percentage of uranium - residual uranium per gram of copper and this compares very favorably with the pre-Atomic Energy produced copper sample.

Next slide, please.

We found that the contaminated aluminum showed the highest amount of contamination of any of the samples that we analyzed, 4980 micrograms of U per gram.

The remelt showed considerable surface contamination as evidenced by both the drillings and also by the Juno measurements. The slag contained approximately half the contamination shown in the original slag and this is probably due to the fact of aluminum's affinity for oxygen, having an affinity greater than any other metal ore including uranium. And therefore the uranium was not allowed to oxidize and could not come off in the slag.

[redacted]

The value found for the National Bureau of Standards aluminum was .75. That is all.

In summary, the uranium surface contamination was successfully removed by remelting all metal investigated with the exception of aluminum.

Some other means of separation of aluminum contamination should be investigated; possibly the addition of fluorides or some other fluxing material into the melt would permit a more complete transition of the uranium into the slag.

The results of the air dust samples taken showed no health or industrial hazard to exist during the remelting. Remelted nickel, nickel stainless, copper showed residual U contamination comparable to those found in pre-Atomic Energy produced metals.

Aluminum scrap was not successfully rid of the uranium contamination by melting because of its preference for oxygen therefore an investigation should be made.

It is our belief that the A.E.C.

[redacted]

policy on scrap disposal should be expanded to permit disposal of uranium, contaminated stainless nickel and copper with the present restriction.

The disposal of the contaminated slag can be accomplished by selling the slag to slag processers or, if the economics are not feasible, it can always be dumped at see or disposed of on land.

MR. EISENBUD: Thank you, Paul.

DR. BUGHER: At the time you were working on this steel recovery, there was also millions of dollars of platinum, contaminated platinum stacked up and it was hoped that out of this would also come something.

Do you know what has happened on that?

MR. EISENBUD: This is a little bit more complicated problem. What we did was set up a research contact with Baker and Company and they had devised a small pilot plant and all of the contaminated platinum in the country is now going into this plant in New Jersey where it is being

[redacted]

reprocessed at a rather nominal cost, so that the stock pile has been liquidated, but this was not something which we could do here.

DR. BUGHER: At least the problem has been solved.

MR. EISENBUD: I simply want to point out that while these studies are not of great scientific importance, they are extremely useful and I cannot over-emphasize the economic importance of studies of this kind nor the fact that the need for such studies can come from anybody except one who is concerned with health hazard because their plant engineer doesn't know that.

He has a number that he doesn't know the origin of, and he lives with it regardless of cost because that is his instructions. But it is the person who originates the criteria of the maximum permissible levels that has to see the need for studies of this kind and the studies, the other studies.

Associated with this kind of effort, there's a rather large analytical program and I'm going to ask Mr. Whitney to simply summarize the types of analyses that his laboratory

[redacted]

[redacted]

has to do for the Industrial Hygiene Service and the numbers of samples that are involved.

MR. I.B. WHITNEY: The analytical problem consists of three major groups of samples, one which we do for the Industrial Hygiene - these are the bio-assay samples which concern uranium in uranium air dust and soil or whatever it may happen to be that they go out and get samples for; also small quantities of beryllium in air dust or urine, radium periodically, radon in breath samples, thorium in urine or sometimes in soil samples, fluoride sulphates in air dust.

I think we have done a few and also soil samples for background data at various plants about to be started.

We also have a sunshine program for which we make "cerium", strontium, some iodine, barium and various other chemical analyses.

We have had a program which has followed very much the same type of radio-chemical analysis including a few others such as zinc and ruthenium.

We had an iodine program during the early part of the year.

[redacted]

Summing up in the bio-assay program, we had some 8,400 samples which were analyzed.

In the other major programs in which we have the monitoring program, the world-wide program, we analyzed or at least processed 86,000 samples during the past year.

These 86,000 samples were combined into some 1700 samples which were analyzed for strontium and for mixed fission products.

The iodine program - there are about 400 samples which had several different types of analyses right on them.

The sunshine program - and this includes at least 150 melt samples, had some 2700 analyses made for this program. "Petrol" -- about 1200 analyses were made giving us a total of about 102,000 (I'm not being accurate, but it is within less than one per cent of that figure) about 102,000 samples or analyses were processed in this laboratory this past year. And this is an increase of about thirty per cent over the previous year. I can break this down in any way you want.

DR. BUGHER: What did you do in your spare time?

[redacted]

MR. EISENBUD: You will find as we go through the laboratory later on, that we have had to design a great many mass production procedures in order to handle this large volume. The laboratory is interesting from that point of view alone.

Now, we will go on to the surveys which have been undertaken of accelerators operated by the Atomic Energy Commission around the country.

This survey was undertaken at the request of the Research Division because of the general requirement to establish between the covers of a single report, all the information about radiation hazards, around operating accelerators so that some general conclusions can be drawn and some general criteria adopted. Mr. Solon is going to describe that program.

MR. LEONARD R. SOLON: One should describe the motivation for this program site for the general effects of the radiation field.

There has been a total of twenty-two cases of particular abnormality reported in the literature since 1949 and ten by

[redacted]

[redacted]

Abels and Kreuger since 1949; seven from Europe and five by an "ardent group — Kraus and Bond", which makes a total of twenty-two.

There have been no additional reports of accèlerated produced cataracts to my knowledge since 1951. And particularly the 6th Conference on radiation cataracts did not report any additional cases which perhaps indicates that something successful is being done in combatting this problem.

The accelerated program of the Health and Safety Laboratory has included a total of 30 accelerators on the East Coast to Berkeley. It's included four major synchro-cyclotron sites; it included a large proton lineal accelerator at the University of Minnesota, 10 cyclotrons and a miscellaneous number of generators, betatrons, electrons, synchrotrons.

The survey is established through the local Operations Office. In our own case it will be done through our authorized representative; in the event of remote places such as, let us say, the University of Indiana, Purdue or Illinois, it's done through the Chicago Operations Office.

[redacted]

Contact is not established directly with the university and a preliminary review of the situation, the floor plan, representative, current intensities and whatnot are gone over and we make a very strenuous effort if we can, to do a current survey with the health-physics representative at the university with the instrumentation that he has available.

We visit typical sites, we look at the public unregulated area, the occupied areas, the occasional occupied areas; we stay out of areas where direct personal occupancy is no factor.

At the termination of the survey which is generally of a day to three days in duration, we sit down with the safety people or the health-physics representatives on the site and kind of review with them our general impressions, some observations that we have made and some observations that they have made which may be new to us and at the conclusion of this, a written report is made and relayed through the Operations Office to the contractor of our findings.

A little better picture of what goes on in our field survey I think would be

[redacted]

obtained by the slides which we will have run off for us shortly.

This shows the nucleus of our neutron instrumentation. To your left you see a Failla-Rossi Tissue Equivalent Chamber; to your right you see a carbon dioxide filled graphite walled ionization chamber surrounded by aluminum and down below -- this is the Tissue Equivalent Chamber and this is the carbon dioxide filled graphite chamber. (Indicating)

These devices are used in conjunction with vibrating electrometers. This is a BF-3 long counter, a device for measuring specifically neutrons between thermal and roughly up to three with a flat flux kind of response.

This particular photograph was taken at the large proton "lineal" accelerator at the University of Minnesota at that time operating at forty MEV and they are going to have it back to their full capacity of sixty-eight MEV.

Can I have the next slide, please.

This represents one of the practical problem

[redacted]

[redacted]

that one encounters in an accelerator survey. This photograph was made out at the largest electron linear accelerator in the United States, the one at Stamford, with a nominal value of 1 BEV, but which was actually operated at 670 KEV and here one sees a shield around the 200 Ionization Chamber instruments to prevent RF interference from the "resolotrons" accelerator with the electronics of our measuring device.

Next slide.

Now, in cases where one can use portable survey meters, there are such situations as DC accelerators. Mr. O'Brien will touch on some aspects of that problem that are commercially available, a few reasonably good type of devices.

Inspection is being made here of the shield at the Cockroft-Walton generator of Bartold, of the Bartold Foundation. This generator is being used to accelerate deutrons bombarding a tritium target obtaining 14 MEV neutrons and a number of our laboratories is making a survey using a.. Scintillation Detector, It's a scintillation type detector.

May I have the next slide, please?

[redacted]

Now, one finds neutrons in many places and one finds neutrons in particular not only inside the accelerator but while removed from the accelerator.

This photograph was taken at a single cyclotron site of a large Eastern University which some of you undoubtedly recognize, the University of Rochester.

The situation has since been rectified, but at this public bus stop at the time of the survey, the neutron intensity was something of the order of 300 neutrons per square centimeter per second for fast neutrons or roughly ten times the maximum permissible exposure rate. However, this is of course for a forty hour occupancy. (Laughter). And that's a long time to wait for a bus.

It does however point up the need for public unregulated areas in making these types of measurements because the public is one group of people we want to protect.

May I have the next slide, please? (Laughter.)

This is another phase of the same

[redacted]

[redacted]

thing. This was an older cyclotron. It happens to be -- this is in no sense a reflection on the cyclotron or its operators but it is more or less typical.

This is a small cyclotron at the Sloan Laboratory at Yale University and right where the stone is in this picture, the level was of the order of 200 neutrons per square centimeters per second. This happens to be a sidewalk outside the cyclotron building. This situation has also since be rectified.

May I have the next slide, please?

This photograph is not that of some prehistoric monument. It is thrown in to represent the economic problem in the shielding against fast neutrons originating at a high energy, particularly a high energy synchro-cyclotron site.

This is a shielding wall at the University of Rochester and one should note the massiveness of this type of shield.

An accurate survey must point out the permissible neutron flux within a reasonable value, otherwise one is going to go into

[redacted]

[redacted]

really massive kinds of expenditures in shielding.

In this type of accelerator there, a half value there for the mass neutrons being produced, the fast neutron components can be eighteen inches of loaded concrete. Eighteen inches of loaded concrete can represent an investment of many thousands of dollars for an adequate shield.

Next slide, please?

This shows a picture taken at the State University of Iowa, where the CO_2 Ionization Chamber and Tissue Equivalent Chamber is being used to evaluate not the neutron hazard but simply the back electron current in a Van de Graaf accelerator.

The levels here were reasonably high. It was simply a question of determining what they were.

Next slide, please?

Again, if you have a portable, if one is dealing with a direct current machine, it is desirable to make use of portable equipment.

[redacted]

[redacted]

This is a Scintillation Detector using a five inch crystal and a five inch photomultiplier based upon a Hornyak design and built in this laboratory.

Next slide, please?

Well, that is the end of it. I should just like to say what our general overall impressions were with respect to the thirty accelerators that we have visited and that are more or less representative of 200 accelerators or so in the United States.

We do not think that there are large numbers of people being exposed excessively to high level fast neutrons. As a matter of fact we think the problem is quite the other way around.

At all accelerators where we maintained film badge records and have done service, there's a great tendency for a very small group of technically proficient people at the accelerator site to receive the total site exposure, that is, if there are eleven people wearing film badges and technically responsible for experimentation, one will find two people have received a total site exposure over a period of a couple years.

[redacted]

[redacted]

Another point that should be made in this connection is that the biological information upon which our maximum permissible exposure levels are largely based, have been almost entirely derived from experimentation from fourteen MEV neutrons and less.

The Commission is now in the process of erecting an accelerator at Brookhaven which will achieve twenty-five billion electron volt protons.

There are several accelerators and a half dozen or so in the two hundred and fifty to five hundred MEV range, synchrocyclotrons and yet the biological information on which our maximum permissible exposures are based upon, experiments were done at a much lower energy.

One can argue from physical considerations like linear energy transfer and specific ionization that such extrapolation is admissible. But until the biological experimentation actually at these high energies is really done, it seems to us we must regard this as simply scientific hypothesis that has yet to be proved.

[redacted]

[redacted]

5 In terms of our own evaluation of permissible levels, taking into account the time factor, we feel that still the uncertainties and the biological effects of the uncertainties, the watchword in terms of evaluation in this radiation field around particle accelerators is reasonable in degree.

MR. EISENBUD: Thank you very much, Mr. Chairman. I don't want to deny the committee the opportunity to ask questions.

I have been going along at a fairly stiff pace here because of the time schedule that we have arranged. I have forty-five minutes at the end for questions, we can utilize it at that time or we can distribute it.

DR. FAILLA: Arrange it the way you feel best.

MR. EISENBUD: I prefer to wait until the end because I would like to cover the program.

Let us put O'Brien on now. That will be a paper on Neutron Instrumentation Problems. And then we will hear from Mr. Blatz.

[redacted]

MR. KERAN P. O'BRIEN: The Neutron Instrumentation in use by our laboratory has, as its purpose, the measurement of stray radiation in occupiable and potentially occupiable areas often in the presence of gamma radiation of magnitude on an ionization basis and over a wide range of neutron energies.

There is a further complication in that we are visitors and potential visitors to 200 accelerator sites and therefore our visit must be brief.

The instrument must be essentially portable and the information is derivable from these instruments. It must be relatively clear and unambiguous.

We have made two approaches to the solution of this problem. The first is the use of ionization chambers; the measurement of ionization currents from chambers with walls and filling of a suitable material and the second is the use of instruments which are essentially proportional counters.

The ionization current produced in an ionization chamber is essentially proportional to the energy deposition rate in the wall material at a depth equal to the wall

[redacted]

thickness as long as the cavity is either sufficiently small or if the filling is of the same gross atomic composition as the wall material itself.

We use three types of ionization chambers in our survey. The first is the Failla-Rossi Tissue Equivalent Chamber, the second is a Graphite Wall Carbon Dioxide Filled Ionization Chamber and the third is a Polyethylene Moderator Steel Wall BF_3 Filled Ionization Chamber.

A Tissue Equivalent Chamber is composed with a wall and filling of the same gross atomic composition as average wet tissue.

This means that it measures the tissue dose rate at a depth of 3/5ths of a centimeter. It has one disadvantage due to the currently accepted RV of 10 for fast neutrons. Equal ram dose rates of radiation of different types do not produce equal ionization currents.

Seven and a half millirem per hour of fast neutron radiation produces only one tenth the ionization current due to seven and a half millirem per hour of

[redacted]

10

[redacted]

gamma radiation. For this reason we introduce the use of BF_3 and Graphite Chambers.

The BF_3 Chamber has a six centimeter polyethylene moderator, the purpose of moderating the incoming fast neutrons and enhancing the response of this chamber to neutron radiation; the Graphite Chamber has a negligible neutron response so that the two of them can be used for the purpose of measuring the gamma dose rate and the neutron dose rate.

The relative response of the BF_3 Chamber to neutron radiation is an order of magnitude greater than that due to the Tissue Equivalent Chamber. And so, this improves the resolution as it were.

The response of the Tissue Equivalent Chamber except for possible variations of the dose delivered in the human body from multiple neutron collisions as opposed to single collision is essentially flat with respect to energy.

The flux required to produce a millirem per hour at 500 KEV may be ten times, or rather three times that required at ten MEV, but nonetheless, the energy

[redacted]

10

[redacted]

deposition rate is the critical pirameter and this depends only on the dose rate.

This is not the case for the BF_3 Chamber, however. The ionization current produced in the BF_3 Chamber is proportional to the number of fast neutrons which have been thermalized in the sixth centimeter polyethylene moderator and this depends on energy.

According to Snyder and Neufeld, the ratio according to fast rem rate at five MEV and the depth of six MEV in tissue is ten to one, but at ten MEV it is only one to a hundred.

All our instrument calibrations are done in terms of polonium beryllium and plutonium beryllium neutron sources. These have essentially the same spectron and so for the calibration of counters, counting time instruments all that is needed to know is the output of the source.

However, for instruments such as ionization chambers, which are also sensitive to gamma radiation, it is necessary to know the gamma output of the sources.

[redacted]

/3

[redacted]

This was done in this laboratory by calculating the millirem output of the neutrons that is to say, calculating the number of millireps, the number of neutrons required to produce a millirep and then using the Tissue Equivalent Chamber with a standard radium source encapsulated in a half a millimeter of platinum. The unaccounted dose rate was attributed to the gamma dose rate of the source.

All our measurements are reported with the ionization chamber in terms of the polonium beryllium equivalent flux. This flux is not necessarily the same as the flux, the actual flux in the region. It would rarèly be the case except under rare conditions among which would be the case of the stray radiation, energy spectrum which was the same as that of polonium beryllium.

However, the ratio of a flux to a permissible flux at a given energy is strictly proportional to the dose rate.

Under ordinary use, we use the Graphite and the BF_3 Chambers to evaluate the neutron and gamma dose rates. These are converted to millirem per hour and then

[redacted]

compared with the value the total tissue dose rate as is measured by the Tissue Equi-- valent Chamber and this means that the Tissue Equivalent Chamber is a measure of the validity or the measure of the integrity of our values.

The Ionization Chambers are primary stray radiation measurement instruments, they are little affected, although to some extent but they are little affected by RF pickup and do sudden variations in the radiation field.

It is true.that they may not be saturated for large instantaneous dose rates, but this can be tested for, simply by varying the collecting potential across the ionization chamber.

We plan in the future to calibrate these instruments over a wider range of energies than heretofore.

One big difficulty in the use of these ionization chambers is the weight of the electrometers which we require.

The ionization currents are produced at about ten to the minus thirteen amperes. It is therefore necessary to use

104

a vibrating electrometer with each one of them and they are quite heavy. If an accelerator is built so that potential radiation hazards exist on many levels of the building, and this often happens, a survey can become quite strenuous. (Laughter.)

The development of a portable DC amplifier with sensitivities that range down to this region would be quite a convenience for us.

We could, had we a sufficient number of ionization chambers which we don't, use them as condenser chambers with a single electrometer. This is an approach that we have tried.

Whenever possible we use proportional counters to supplement the information obtained by ionization chambers.

Due to the great flexibility and form of sensitivity which can be achieved with these things, the information obtained from such instruments can be quite valuable.

The long counter, the Hanson-McKibben BF_3 Long Counter, is the most sensible, reliable and generally accurate example of such an

[redacted]

instrument.

It uses a nuclear inter-action to favor neutron pulses in a circuit to accept only those pulses greater than a precept peak voltage to discriminate neutron produced pulses from pulses produced from gamma radiation.

The Hornyak and the Hurst Proportional Counter are both survey type instruments. The former uses the light pulses produced in a zinc sulphide lucite scintillator and the latter uses -- well, in the first case they are produced either by recoiled protons with the lucite or the reaction protons from the sulphur reaction and the latter uses the recoil from the ethylene filling.

Since both of these - since the pulse heights in both of these cases depend on the kinetic energy of the instant neutron, there are fluxes with sufficiently low energies that will not be registered by the counting instruments.

In other words there are low energy cutoffs below which these instruments will not work.

[redacted]

With the Hornyak, this occurs at about 400 KEV and with the Hurst this occurs at about 200 KEV.

For many purposes these values are entirely too high and a large proportion of an energy degraded neutron spectrum cannot be measured, particularly this is the case when the maximum original energy of a neutron occurs at about 15 MEV.

There exists a high energy basis in which the hydrogen cross section on which all these counter instruments that we use depends to a greater or lesser extent varies from its maximum zero energy values of about 80 down to about one barn in 10 MEV and 2/10th of a barn in 40 MEV.

Now, since most counters either raise a count when a pulse is higher than the precept peak voltage, this means that due to the small size of the instrument and the decreasing cross section it is difficult to get reliable results when neutron being measured has energies higher than 15 MEV, the long counter response is down to about 85 per cent at 5 MEV due to the increase in the mean free path of the instant neutron in the paraffine jacket.

[redacted]

This instrument however, has its pulses produced by the neutron of a reaction with boron in the filling and as a result, it's responses is essentially flat down to neutron energies of about 10 KEV.

Now, the biasing circuit is designed then, to accept pulses which are greater than the minimum to discriminate between ambient noise and radiation of other types.

There's another way in which you can fail to discriminate between neutron radiation and radiation - well, specifically gamma radiation and this is in the case known as gamma "pylo".

This is designed on the assumption that the incoming pulses can be recognized as individual events.

This is the bias level. Now, ordinarily, if they come in, the neutron pulse might look like that roughly -- if the instantaneous dose rate is sufficiently large, they are no longer resolved into individual events and the gamma rate; pulse height then, is the sum of the individual pulse heights produced and as a result, may override the bias and be counted as a neutron pulse.

[redacted]

[redacted]

For this reason it makes it particularly difficult to use a counting instrument at a pulse accelerator site.

The average gamma rate level at such a site may be small, the average level may be rather small and a typical example of an electron synchroton, the duty cycle may run to 400 microseconds per second and this means to get the instantaneous dose rate one has to multiply the average dose rate by at least 2500.

Our final consideration is that had we a perfect flux measuring device with a perfectly flat response to all neutrom energy, nonetheless we shall have to know something about the neutron energy spectron in order to make an estimate of the permissible flux in a particular region.

At present our spectro knowledge depends on knowing the maximum originals energy of the neutrons in performing paraffine and cadmium absorption measurements with a free detector.

However, should the neutron be produced to say 400 MEV or something on this order, then our information is hardly reliable.

[redacted]

We hope to, in the future put our spectro knowledge on a more quantitative basis by the use of nuclear microscopy. Thank you.

MR. EISENBUD: The situation in the field of neutron measurements is probably the least satisfactory of all the fields of instrumentation in which we have contractors despite the fact that many individuals become, as our Chairman, for example, spend a long time dealing with many of these problems.

DR. WARREN: Is it fair to say, do you think that some of the biologic test objects are perhaps as good measuring devices as we have.

MR. EISENBUD: I suppose this is true, Doctor Warren of the higher levels and over longer periods of time. It's pretty hard to take the site of an accelerator and get measurable cross-overs.

DR. FAILLA: Which biological material?

DR. WARREN: I was particularly thinking of cancer.

[redacted]

[redacted]

DELETED

[redacted]

[redacted]

DELETED

[redacted]

DELETED

[redacted]

DELETED

[redacted]

[redacted]

DELETED

[redacted]

115

[redacted]

DELETED

[redacted]

[redacted]

DELETED

[redacted]

DELETED

[redacted]

DELETED

[redacted]

[redacted]

DELETED

[redacted]

[redacted]

DELETED

[redacted]

DELETED

[redacted]

DELETED

It is 12:15 now. Lunch is across the street at the hotel, it will be on the table when we get there. We have an hour and a quarter reserved for getting over there,

[redacted]

eating and getting back. I think we can have one more presentation before we go out.

I think your presentation would lend itself very well to a talk in the laboratory. Why don't you plan to describe this system during the tour and we will hear now from Miss Naomi Hallden on "Beta- Absorption Techniques".

MISS NAOMI HALLDEN: In our analytical laboratory we are called upon to analyze samples of a large variety and because of the magnitude of the isotopes that we are working with namely just in some case, a few disintegrations per minute.

We have to -

A. Check the method by which we analyze these substances and also check samples to make sure that if a sample has been precipitated we are sure that that is the isotope we are talking about.

This independent check has to be done with a certain amount of simplicity. We cannot take too much time per sample and we cannot have a tremendous amount of equipment to run each particular sample. And so we

[redacted]

12

have devised a method of running Beta Absorption curves with a simple set up and aluminum absorbers.

Other methods previously used for Beta Absorption Technique place varying thickness of aluminum over a sample, the Beta Emitting samples.

The curves are then plotted as activity versus this thickness of aluminum. This is good for visual use. You can see the shape of a curve or compare it with another isotope that you may consider, but for complex means, samples containing isotopes are possibly the worse case of fission products. With very many isotopes it gets rather complicated.

Other methods such as feather plots use always end point functions, range functions which means that you have to work in very heavy thicknesses of aluminum and our samples that we are dealing with sometimes range as low as one disintegration per minute and if you are trying to be sure that you have found strontium in a sample as counting one disintegration per minute, it is very difficult to do this by any absorption techniques.

[redacted]

We try to work out some method which will give us a straight line function because it is difficult to compare curved shapes and we found that by using an intermediate substance such as phosphorous 32, and plotting a log line relationship, that is of activity transmitted in aluminum for phosphorous versus activity transmitted of your sample, you can obtain a straight line relationship if you have an isotope emitting one Beta particle that is one of a maximum energy Beta.

Now, the first slide I have here is the conventional method. This is the phosphorous 32 and this one. The thickness of aluminum versus activity transmitted in per cent and you see if you compare two curves like this it would be a visual process just looking at shapes.

The second slide is the same phenomenon plotted on a three logarithmic -- three cycle logarithmic paper and in this case, the transmission of beryllium is plotted versus the transmission of P_{32} for each, for the same absorber and even though the Beta points aren't shown, it's a very good straight line.

Now, the good part of this method

[redacted]

is that of course the line isn't straight up here as it is down here, so with very low activity samples you can run just a few absorbers beginning with a curve and determine the maximum energy.

The next slide is a two component system and mathematically of course, two Beta Emitters will be simply the sum of two straight lines and when you run the total sample, you get the cumulative curve, extrapolating that and subtracting your total curve, you obtain these two straight lines.

We have ordered isotopes from Oak Ridge and run in the order of a hundred of them, and plotted each one separately to determine the slope of these lines and then plotted the slope against the known maximum energy.

The next slide is this relationship. This is our calculated slope versus the maximum energy of the sample which you see over here for just a few of the isotopes.

You can see there's a good empirical relationship.

Can we have the lights, please?

[redacted]

As I said before, we have used this quite extensively in the laboratory to obtain an isotope of zirconium say, for placing in a solution that we wanted to run first as development work zirconium ionium and we have to determine whether the solution is pure and then of course, in the case of two samples, to see if the isotopes precipitator is best for what procedure has been worked out for them.

Other applications that we have found to apply are such things as equilibrium state of uranium and thorium waters by having a set of standards to pitch blend and uranium oxide we can plot these against an unknown sample of uranium bearing ore and determine the state of equilibrium that it is in.

The last topic I would like to mention is the elucidation of some of the decay -- in running a tremendous number of these absorption curves and checking with standard N.B.S. data, we have discovered a few discrepancies and one is a fairly well studied isotope cesium barium 137, and much work has been done on this isotope spectroscopically, but none has been done absorptionwise.

[redacted]

[redacted]

Since this is one of the most sensitive absorption techniques that is in use today, and we have discovered discrepancies, we are planning on submitting this discrepancy to physical review.

The percentages are given for two Beta particles emitted from cesium 137 -- they abe .56 MEV and I believe 1.1 MEV and the percentages are 90 per cent and 10 per cent and after purifying this isotope chemically and running many replicas of the same sample, we consistently get the percentages to be 60 and 40 and an energy difference for this Beta for 2/10 MEV (indicating 1.1 on the board).

MR. EISENBUD: Thank you very much.

Well, we have practically caught up. We will have Mr. LeVine presents his material on the Central Beta Processing Unit during the laboratory tour which will be given at 1:30.

So, after lunch we will assemble on the fourth floor and we will have a tour during the laboratory and this afternoon we will go into our fallout portion of our program.

(Whereupon a luncheon recess was taken.)

[redacted]

[redacted]

AFTERNOON SESSION

MR. EISENBUD: Two of the presentations will be best here and we will pause for about a half hour which will also give us the opportunity to introduce to you, the work of the instrument branch.

Basically, we are a low level instrument laboratory but in addition to that there are involved many techniques which make it possible to do measurements that are ordinarily measurements in the field.

To illustrate that I want to get outside of the field of health and safety because occasionally we are called upon to do other things.

It used to be that when a geologist wanted to find out how much uranium is in the soil product, he drills a four inch hole down to a few thousand feet. All those coils would come out on a truck and the truck would go to the laboratory.

With an increase in the geological activity six or seven years ago, this technique was no longer feasible and we put some

[redacted]

[redacted]

of the know-how that has gone into the design of instruments for health surveys into the field, into the design of equipment of this type.

Here is a probe which will be dropped three thousand feet into a water-filled hole, it will be suspended on a co-axial table and the depth at which it is dropped will automatically be recorded in the radiation levels - will automatically be recorded and the whole device weighs about fifty pounds.

So, instead of drilling a four inch hole, they drill a two inch hole and we now have one inch holes.

The front end of it has a crystal and a scintillatometer. This can be lowered down through three thousand feet of rock.

MR. LeVINE: This particular piece of equipment was used by our group.

A VOICE: Do you have the instruments that were used on boats?

MR. EISENBUD: They are pretty large but we have them here.

[redacted]

[redacted]

For example. on the weapons test, in the Pacific, we have been able to design instruments that go into tin cans and get right on the beaches. Now, at Eniwetok they get their fire from storage battery and feed their information to us through the telemetering system.

Instrumentation of this type is made with widespread moderation that is necessary in the Pacific. These gave warning that a fallout had occurred.

In that respect, why we say we did not use good judgment in setting up the limit of the instrument at 100 per hour, and so it went off scall, but it did work up to a certain point.

Now, to get back to the formal part of the program. You have seen very sketchily what our instrumentation problem is in the analytical laboratory and the need to handle large volumes of data.

We referred earlier to our plans to feed the data automatically on an IBM system. Mr. LeVine is going to describe the status of that system.

[redacted]

MR. LeVINE: I want to make a statement that this is not a low level laboratory. (Laughter.)

MR. EISENBUD: He's talking about laboratory.

MR. LeVINE: I think with a hundred thousand samples that we get together a year, and the need to add additional information such as instrument geometries, accounting efficiencies, where the samples came from, how old it is -- all these other factors to tally into the number of disintegrations per hour and per minute at time zero -- the problem of manually handling this data -- well, it's almost insuperable especially when you can't get enough people to do the job.

It is difficult to train personnel to transcribe data accurately and although we use fairly simple equipment with a lot of people to get a set of reliable data that you can take without question, we have to go to automation and this is in a sense, this IBM system that we are developing now.

Now, whether we put it on IBM punch

cards or any other, is a matter of choice for someone - Doctor Failla or someone to say, "this is the way we'll do it." Largely, we'll go to the IBM puncher.

The IBM punch card has eight positions of data with twelve digits and therefore we can put 80 digits of information across the card and put in such things as sample number, what machine it was counted on; we can put in the machine geometry and a whole array of data, dates, times and so forth.

We have contemplated a system that will take up to one hundred factors all feeding into one simple punch station and, while this may wind up with a lot of wires, we use the telephone technique in the sense that we have ten lines in this direction going from nine to zero.

Each division comes in on a cross bar so that this is the ten, this is the hundred and this is the thousand. So that the number of machines that we can put on are not limited by wires because it only takes one wire to put on a great deal.

The same thing occurs in reading

back and counting numbers. If we decide our total count shall be ten thousand, we will just put another one in here (indicating).

If we have a number like this 9987 for example, we read this, this, this and this one in sequence and we just relate them to the columns that pertain to it.

The IBM machine then, takes the data and throws it on the records completely; we can account for the sample number and we can cross correlate the sample with the original data that came in with the sample because it all starts on a punch card, winds up on this and it's crossed.

Our reasons for going to this system besides making computation easier, is the fact that present and generally utilized number of recording systems are poor.

A typical example, we estimate that we'll be able to say on just 6 machines using the savings for the 6 machines alone, $1,300 per machine per year repair cost personnel time and other things.

In addition it turns out that once

[redacted]

we start building modular types of equipment we also will save on equipment. For example, the Beta Tape Counting Machine, and believe me these are estimates, but we think that the Beta Tape Counting Machine estimate which is built right now, costs us $2,000 per unit for recording.

When we go to IBM and amortize the IBM punch card installation on 30 michines, which we eventually expect to get out, that the cost per machine including recording facility is $1,700. So, not only do we save on machines, but we will also save annually on the cost of maintaining that and our data is better.

I'd like to point out some of the things we can get out of this. There's no limit to the types of data we can feed in. Should we want to get information to build a multi-channel analyzer or have need to order all the data from a multi-channel gamma ray analyzer, could be fed into the IBM cards and the data reconstructed directly from the computer.

We can add orders of magnitudes to the machine without trouble. For example, to punch the complete set of data

[redacted]

[redacted]

out it takes less than six seconds and even if we have 100 machines, no machine is going to stand idle for more than about two minutes.

So that we save a great deal of time, facility and I think just as well on personnel. It will also require a less trained group and have better data compiled because of this change.

MR. EISENBUD: This is going to be a tremendous involvement for us. Now, we are going to get into another subject.

By way of background we learned when we were asked to monitor the totals of the Western Pacific, the professional techniques of monitoring simply weren't adequate for what was expected as to the possible fallout.

We applied some techniques that have been developed for aerial exploration in the raw material field and beginning with Operation Ivy, back a few years ago, up to the present time we had a development program designed to give us better and better area equipment for measuring fallout.

The techniques are well proven.

[redacted]

[redacted]

Actually, the techniques worked very well during Ivy and even better during the other. It was a much better factor of accumulating information after the March 1st fallout. The work began on the operational scale during Operation Wigwam.

We now have what we believe to be a very practical procedure. This was done in order to face the rather difficult problem of monitoring several thousand square miles in a reasonable period of time.

Mr. Cassidy is now going to tell you about this. Unfortunately we don't have an operating instrument here which we hoped to be able to demonstrate due to the fact that they are all being torn down to be put in shape for the coming test in the Pacific but the process will be explained to you by Mr. Cassidy

MR. CASSIDY: I will first try to describe the aerial survey system that we have developed in its latest form and after that I will mention a few of the things we have run into, the Federal Civil Defense and State Defense Agencies.

I would like to give them a little

[redacted]

[redacted]

bit of information on the aerial survey system so that they might be better able to use it for their own needs.

I'll do this on a block diagram basis starting with the detector which is a scintillation instrument used largely in processing phosphorous and photo metron tubes.

There is one photomultiplier tube which is a three inch tube and has a plastic phosphor three inch diameter by four inches high and this tube covers a range of radiation from approximately five micro range to the 100 milli range.

The second tube is one quarter inch tube with a one quarter inch thick by an inch diameter plastic phosphor which covers the range from 10 MR to 200 MR.

The two tubes overlap in each of these ranges on one decade. This was so that we wouldn't have to do any scale switching of the information coming out from the top of the first tube's range.

The plastic phosphor was chosen because of the relatively good energy response;

[redacted]

it's much better than sodium iodine. They have a small "optical" and mechanical strength as compared to a sodium iodine tube.

The output of the multiplied tube is spread into the grid of a small filamentary type tube. Actually there are two tubes in parallel. These two tubes give us a characteristic which gives us a logarithmic response so that we can cover approximately five decades of information on a single range.

The output is fed into a recorder which is connected in the plate circuit of the amplyfier tube and gives you an indication of zero to one mill which corresponds to approximately five decades.

From this detector we go into this recorder. (Showing it on the blackboard). Then we also go into what we call a compensation circuit where a signal from a radar house comes into this.

The signal from the radar altimeter is fed into this circuit and combined with the output of the detector.

The purpose here is that with the

aeroplane you are above the surface of the ground and we want to get an idea of what the radiation is at the surface or close to the surface. So, we have picked a value of measurement at three feet as our reference which shows approximately what a person carrying an instrument on the ground would use, before giving an estimate that is, of total body exposure.

Over the course of several tests we have developed a series of factors so that we can multiply the radiation intensity obtained in the aeroplane by these factors at any altitude from approximately fifty feet to fifteen hundred feet to determine what the radiation of three feet per surface is.

The radar altimeter gives us the absolute distance from the aircraft to the surface and not to see them or some other arbitrary reference such as the barometric altimeter would give you.

The DC signal is confined to this and this compensated signal is then fed to a telemetering unit which is called the telemeter information station.

Here the DC signal which is

14

proportional to the input level radiation testing is converted into an AC signal suitable for radio or for telephone communication circuit.

Actually what we do here is we take the DC signal and then convert it into first pulses, the length of the burst to be proportionate to the radiation.

The length of the bursts as we call them vary anywhere from zero to three quarters of a second from minimum radiation intensity at a burst of three quarters of a second corresponding to full scale sensitivity on any range.

This is sent by a radio or telephone communication to the central station where the information is again reconverted from AC back to DC suitable for driving another one of these recorders.

At this point which is at what we call the central plot, the path of the aircraft is also being plotted and this data appears on the recorder which gives you a profile of the contaminated area, is correlated with the aircraft's position in time and a plot is then made of the area giving

[redacted]

the density and position, the combination.

Up until now, we haven't used radar to plot the position. Civil Defense will probably have to go to something else. They might have some ideas on this.

Physically, the various components are over here. This unit is the information station where the DC signal is converted to an AC signal.

The central station consists of this unit (indicating) and the associated recorder.

Mr, Eisenbud pointed out that unfortunately we are modifying the system right now and we don't have a unit here to show you.

That is essentially the aerial survey system that we are developing. The heart of it really is in this altitude compensation and the development of the factors for determining what the surface denomination is when it is measured several hundred feet above the ground.

If you take the data and all the

[redacted]

tests since Ivy we have reasonable confidence in our factors of altitudes so that we really feel we can measure radiation from an aircraft and within reasonable limits giving you the figure that you have on the surface.

Recently we have had other people working on this problem and they have come up essentially with the same information.

The only way you can know what it is on the ground is to vary your altitude and if it stays pretty constant you know what it is.

DR. BRUES:: Does it compensate the factor very, very much?

MR. CASSIDY: I don't think it is. The compensating factor falls in very well. This is one of the big problems we have; this is sort of an uncertainty and this is something that we have got to pin down some day about just why we are able to take these measurements over a long period of time although sources of widely varying shapes and densities and sizes can still come out with a factor, that is fairly constant over these altitudes.

A VOICE: How close would your numbers compare in the air from those that are measured on the ground at the ground at the same time. Have you tried that as a control? How close are the ground readings with the air readings at the same time?

MR. CASSIDY: This is how we determine the factors. We take a measurement on the surface and then we fly above it.

MR. EISENBUD: It's as close as independent ground readings would be.

MR. LEVINE: You might give the figures where you have your aerial and your ground readings at the same time.

MR. CASSIDY: Yes, but that is actually how we did determine it. There is a spread in data of about twenty per cent. The big problem of getting this factor of measuring from the air is not so much the inaccuracy of radiation data but the inaccuracy of the altimeter.

Until we got that radar altimeter, we had gone all over the lot.

This system was developed primarily for the weapons test and it soon became fairly obvious that this was not an ideal system or technique for the Civil Defense work because they would have to go out and take a look at the large areas of fallout to determine what measures had to be taken for emergency, what supplies had to be brought in.

Our first association with that problem was operation alert, last June, where we undertook to send information from New York to Boston. What we did was, we took the fallout pattern which was described for New York and we reproduced this pattern on regular recording tape, using the type of signal letters put out by our information station. Then, when Operation Alert came we put this information right into a telephone line and sent it up to Boston.

In Boston we have this central station (indicating) and what we did was to take the data as it came over and plotted the iso intensity lines that had been laid down for the New York area.

I don't know whether you can see this, but I'll give you an idea of what it looks like. I am going to give you this demonstration.

What we did on this was to take these predicted fallout patterns which were given on this sheet here (indicating) this was put out by FCTA. In using this we assumed an aerial survey following this pattern where we started out up here in the Bronx down to Manhattan Island, around upper New Jersey, Brooklyn, Queens, Staten Island and then out on the Island.

The whole survey took, theoretically about two hours and we computed the total doses to the person in the aeroplane that were thought to be about 26 R total doses and the area covered was several hundred square miles.

Doing a ground survey of this was practically out of the question and the doses involved were quite heavy. This was the predicted pattern and this was one we developed in Boston. (Indicating).

There are discrepancies and these are mostly due to the wow of the tape recorder. If that didn't occur we'd have a better fallout procedure to work with.

[redacted]

MR. EISENBUD: I'm not sure that everyone will understand how we got the information on the tape. I think that is an important point because this provides very excellent training.

MR. CASSIDY: This is one of the things that I wanted to bring out. We can reproduce any fallout pattern conceivable by very easy techniques.

We use a recorder and we take the intensities along a path here and we plot them on a piece of graph paper.

All we do is, we have a piece of graph paper and this is the distance on this horizontal line. We plot the intensity on a piece of graph paper.

We do this here and on the XY Recorder, we have a curve and all we do is connect the proper DC potential across the potentiometer so that we end up with a DC signal comparable to the signal that we ordinarily get out of here.

Then we feed it to the telemeter information station and that reverses the AC which we put on the tape. By doing that

[redacted]

we can show you what some of the curves look like (indicating). It took 15 passes to do that fallout. The advantage, incidentally of telemetering information rather than having somebody reading it off the instrument and record it and then radio it, is that with this method, you get a complete curve like a profile of a contaminated area and, as you can see from this you get full information.

That is what the signal sounds like (permitting the audience to listen in to it).

Now, the operator at the central station presses this read button, looks at this little scope here - I don't know whether you can see it, but you can see the little train of pulses coming through.

The circuit starts quivering and you can hear the click, click, click, that is a set of relays which open the discharge and the two capacitors in the other relay switches.

As you can see you develop a profile of it, you develop a pattern.

MR. EISENBUD: I think you can

imagine the rest of it from here on out.

MR. CASSIDY: This information is taken off periodically and correlated with the plane's position and you develop a pattern.

After Operation Alert we conducted a training exercise out at Mercury, Nevada, under Robert Crosbie's direction in which a group of people on Federal, State and local levels were brought out to Mercury.

They conducted a ground survey in the area which was the open-shut area of the Mercury Operations Depot and we had ten teams of ground survey groups with a number of vehicles at certain intervals spaced a tenth of a mile off the main access road, off Yuca Flats and then would take readings at every tenth of a mile out across the Flats beyond ground zero. And then we were to follow this up with an aerial survey, following that.

Well, if nothing else, the exercise proved how difficult a ground survey could be because positioning yourself in the air is difficult but I'm beginning to feel that positioning yourself on the ground is worse.

[redacted]

They were criss-crossing each other continually and, well, we just didn't realize the difficulties involved. This sort of proved the problems in a real exercise.

Unfortunately the contamination was very spotty and it would add over orders of magnitude, 20 feet, 30 feet, things like this so that when we followed up the ground survey with an aerial survey, we could not match exactly the readings because in an area that is proportional to its height, if you are flying at 200 feet with this particular instrument, you see an area roughly of 400 feet in diameter.

So that, when you are trying to compare readings taken, when your tests vary within ten or twenty feet, you would find it impossible because this would read an average of the radiation intensity over this area.

If you went to 400 feet this would go to 800 feet. (Indicating).

We define this -- the contribution from this point out here is fifty per cent from what you would get at this central point.

[redacted]

MR. EISENBUD: One of the difficulties in this exercise - ordinarily, this isn't the kind of situation where you use this type of procedure.

Normally the combination is spread out over a few hundred yards and here it was very spotty.

MR. CASSIDY: The only thing we were able to do though, was to take an average of all the readings taken around the area and compare it and we had comparable readings that we extrapolated.

DR. FAILLA: How do you calibrate on this large area as you go up?

MR. CASSIDY: This type of calibration was done out in the Pacific where you have really wide areas and very flat.

MR. EISENBUD: Put the helicopter over here. Thank you very much. I think we'll go back to the other room at this point.

8 I'm sorry that the next part of the program which is Fallout Counter Measures cannot be presented by the individual who did

most of the work, but he came in today with some sort of virus infection. I thought he could make it, but he went home a little while ago with a high temperature and so I'll give you some of the background and then Mr. Solon will carry on for Mr. Breslin.

This study which we call or designated as a Fallout Counter Measure Study, resulted from two things which happened coincidentally.

First of all, Bob Crosbie and I visited over at General Hubner's office here in New York. General Hubner is Director of Civil Defense here. He had a very simple problem.

They were building some schools up in Westchester. They had the designs for the cellar and the super structure and he wanted to know how much protection this design would afford against fallout, what minor changes could be made within their budgetary limitations to give more protection and it suddenly dawned on us that here was 1955, that if you want to solve a problem of this kind you've got to hire a physicist and he has to start from scratch with the quality of fallout radiation, calculate all his

/5

inter-actions and attenuations and come up with an estimate of how much attenuation you get that nowhere had anybody taken these calculations which had been done independently by many physicists and made them available in handbook form so that an architect can say, "I've got a cellar which is so wide and so narrow and has such and such height."

Here's a super structure in pounds per square foot and you can calculate from that how much attenuation you could expect.

At the same time Commissioner Libby, we suspect with his tongue in his cheek, asked that we undertake a study of what the Commission's installations could do in order to protect themselves against fallout.

I think he had in mind not so much what the Commission could do for itself, but rather development of information which could be used more generally. Because obviously, if we could help our own installations in this way, then all of industry and the others all over the country can use it.

What Mr. Breslin undertook was a sort of a feasibility study to see whether it would be possible to reduce the information that had been accumulated out in the field into a handbook form so that it could be used by architects and at the same time determine if it is economically feasible to provide a defense against fallout and I will ask Mr. Solon to carry on and show you what he found.

MR. SOLON: Permit me to begin by saying that the efficiency in this presentation and the merits involved are those of Mr. Breslin. He would have done a very effective job at this and I'm sort of pinch-hitting.

Let me say the initial reaction of people that are engaged in practical radiation protection measures when they are confronted with the order of magnitude represented by the March 1, 1954 Pacific shot, is initially to throw up one's hands and say, "my goodness, there is very little that can be done because the order of magnitude of the prevailing radiation level from such a shot," as you know, is something in the neighborhood of 1000 R, and it can be 1000 R per hour whereas the practical radiation protection measures are something like 7½ thousandths of an R per hour and in the case of gamma radiation that is at .75 thousandths of a rep per R of a fast neutron.

Those are just tremendous orders of magnitude between those two.

However, since the problem is with us, we consider a fallout in a rather idealized sort of model, an infinite plane, a smooth plane -- no hills, a structure initially with zero attenuation. There are three principal components to the occupant of a structure.

There is the component from the roof, there is the scattered contribution from the ground and there is the direct radiation from the ground in this fashion, (indicating on blackboard.)

It is both theroretically and experimentally feasible to determine the direct radiation from the fallout. It can be calculated quite precisely; that is the primary radiation.

The scatter situation on the other hand is altogether a different kind of problem. To determine this by calculation is a rather formidable task. There are uncertainties in the cross section; the mathematical techniques become involved especially when you go out over many mean free paths, so in order to get some fundamental scattering data we went to the biology gamma field at the Brookhaven National Laboratory and made use of a point source, namely, two killicuries of cobalt 260, and erected a narrow pencil of a lead shield to eliminate the primary beam.

We went out to the 275 meters and made measurements of the direct radiation and that is the direct radiation with the shield removed and the scattered radiation with the shield in place.

This is a point source and then it was just a simple matter of integrating around over a plane in order to get the dose rate from a distributed source.

This is the Brookhaven gamma field measurements. We again used the very good chamber of Doctor Failla and the points coincide essentially as they should for gamma radiation and one can see that the direct plus scattered beam measurement and this are approaching closer together until when one is out to the maximum distance that we went out - 275 meters, the scatter contribution was slightly in excess of 50 per cent of the total.

DR. FAILLA: How much of a shadow did you have in that?

[redacted]

MR. SOLON: The angle that we made was a little over two degrees - two out of 360.

The group of drawings which we have here illustrate the kinds -- we utilized a rather idealized kind of structure for mathematical simplicity.

Our structure first of all was a cylinder which is unlike, of course, most practical buildings which are rectangular.

We assumed no attenuation by the roughness of the terrain and we assumed no collection on the sides of the building.

We also assumed for the purposes of this preliminary exploration of this problem that the building was dust-free and one was not concerned with fallout inside the building.

This shows the variation in the relative contributions from the roof and from the extended plane as a function of two pirameters, namely the height of the roof, the height measure is really the height of the roof plus three feet and the path width- or the radius of our cylindrical building.

As one can see here, the contribution

[redacted]

of the fallout for a building forty feet high is less important than it is at twenty feet and it should be.

By the way, we have expressed everything in terms of per cent of infinite plane dose and we have assumed an infinite plane dose rate of 1000R per hour and on the basis of a Brookhaven experiment we utilized 22 per cent of this as being attributable to scatter -- 80 and 20 roughly.

Here again, is the same kind of data exploring the per cent of infinite plane dose rate as a function of the radius of the building at the center of the cellar floor.

This is a rather extreme kind of condition, that is it is a limit to the problem because in terms of the scatter contribution one is looking at the maximum possible solid action. There is protection to be afforded of course, by moving to the side of the building.

Again, in terms of our infinite plane dose rate, that means the dose rate at three feet over infinite plane with no attenuation mechanism whatsoever, this shows relative contribution from scatter and from the roof.

159

[redacted]

Now, for a reasonably sized building, say, a building of this one in height, it is 10 feet - the cellar depth is 10 feet, and it has a radius of 60 feet, one can see in terms of the total dose rate experienced by an occupant of this cellar, he is receiving something on the order of 50 per cent, 50 per cent of his total dose from the scatter.

It is a matter of some importance obviously, to determine what increments in dose rate are contributed by annulis surrounding a person as you clear areas around him and this exhibits the material for the scatter and primer data - for the scatter and total data, the effect of clearing.

If by some mechanism, one could completely clear the area 100 feet around a person with no other attenuating mechanism, no concrete, no cylinder or anything else, one would have reduced the infinite plane dose rate by a factor of more than three and as, you go out to 500 feet it's down to the order of 10 per cent.

DR. FAILLA: Do you mean that it was removed or that it was piled on?

MR. SOLON: No, cleared, actually

[redacted]

16

[redacted]

removed, actually cleared.

MR. BLATZ: Ploughed under.

MR. SOLON: This might be one mechanism that could do this job if you could get enough dirt on top of it.

MR. EISENBUD: I think you'll get to that.

MR. SOLON: Now, we are still however talking as you see, — if we're talking about 1000 R per hour we are still speaking about very substantial dose rates in terms of personnel.

This is more of the same thing except here there was the question of applying this within rectangular limits rather than doing the calculation for a rectangle. The calculation was simply done as a limit of two circles.

This is a rather vital thing; I'm glad Mr. Eisenbud stopped me at this point.

This calculation is based on the infinite plane, the smooth plane which has been cleared.

[redacted]

[redacted]

Now, it's possible to find in the "Buster Jangles" literature, the reference test literature, the actual effect of clearing certain square area.

Here is the Jangle Data compared to this inner circle, the Jangle Data and the inner circle, the essential point being that there is a factor of about two to three between the smooth infinite plane case and the actual case encountered in the field. This is, of course, attributed to the roughness of the plane which is an attenuation mechanism in one's favor in the situation.

The purpose of these curves was to give some idea of the kinds that were necessary to stay away from or not to permit re-entry time after a fallout had occurred.

Let us take this one. This applies to the fallout that occurred at a certain site four hours after the detonation.

One was interested in keeping the exposure, the personnel exposure below a certain level. How soon can a person re-enter this area?

We arbitrarily selected 25R. This

[redacted]

is in no sense an endorsement or a recommendation -- 25R one week after the exposure.

We did this for no other reason than 25R is presumably the level that will not produce any significant life shortening and will not produce any significant, immediate somatic effects.

This is some more of the same type of data.

MR. EISENBUD: The reason for these assumptions and calculations would lead up to an actual example.

MR. SOLON: What are the mechanisms by which one can hope to maintain the radiation level to something which, if not safe, is at least acceptable in terms of survival. There is of course, the attenuation mechanism of the building itself.

This is something that is an intrinsic property of all structures. Also and most important, since something of the order of 80 per cent or up to 80 per cent of the dose inside this structure is attributable to fallout on the roof, there have been suggestions to wash down, using spray systems and things of this nature, the Naval Radiological Defense Laboratory has done a very extensive kind of study on washdown mechanisms, the type of solutions to use, the type of surfaces and whatnot.

This phase of the fallout program perhaps has been more exhaustively explored than any other.

Another possibility is the use of gravel beds by which fallouts can be essentially buried to a wide area around the building and thus supress the very heavy doses from acute fallout.

In conclusion of this phase of the thing, I would just like to point out the further endeavors that are possible along this direction. Namely, an examination of existing buildings to find out what sort of protection factors they represent, what are the real mechanisms that are available to a person down in the basement of, let's say, 70 Columbus Avenue or any other facility?

The Jangle Data to our knowledge is the only data which furnishes any information whatsoever on the roughness, the effective roughness of surfaces.

Certainly a more exhaustive study of this is indicated for turf, clay and what have you.

One of the things which will determine the degree of heavy fallout after the attenuation mechanisms have been made operative is the spectral composition of the fallout, the spectral composition of the radiation from the fallout.

Information along these lines in reference to this problem is essentially non-existent, though of course there has been a tremendous amount of theoretical work done on degradation gamma rays.

The washdown situation seems to be pretty well in hand with the work that NRDL has already performed and is continuing.

And lastly, what is a realistic value of permissible exposure in the event of a major nuclear catastrophe?

This of course, will determine many things, will have many vital effects on the kind of counter measures, the kind of shielding and the economics of protection against this, twenty-five, up to fifty or something of

[redacted]

this nature, then this is going to make the problem materially easier.

I think the feeling that we had after the study was -- before the study, the problem was kind of hopeless and now we think that there is some way of approaching this.

MR. EISENBUD: Thanks. I think you did a swell job since you only had a half hour's notice on this very complex matter.

On the other hand, I do want to explain a couple of things, going over the conclusions that were drawn on the basis of a hypothetical building of which we have the dimensions and the type of structure.

As we take each of these factors, you've only got attenuation factors, if you want to call them that, of twos or threes, but when you multiply them together, you come up with a fairly substantial end result.

Specifically, with the factors we've got and unfortunately, I won't be able to take the time to go through the report or acquaint myself with the exact numbers, but we can summarize them and send them to you.

[redacted]

[redacted]

There is what we might call F1, the factor which is due to the fact that the man is just huddled in the corner of the basement. F2 would be due to the fact that you are washing the contamination off a roof and washing it away from the building in some manner.

The building itself, if it extends out beyond where your man is, provides some measure of protection because that, in itself is a grain area.

In addition to that, if you want to, you can out this gravel bed down and wash your contamination down into, let us say, one foot of gravel or more economically, it's quite feasible.

It takes relatively few bulldozers to actually bulldoze an earth dike out here (indicating). So that all the soil around the plant will be bulldozed out to a radius of three hundred or four hundred feet. This is operationally feasible and economically feasible.

This gives you a third factor, due to the fact that this is now clean and in addition to that this dike is acting as a

[redacted]

shield.

You can do all these things and come out with a protection factor of several hundred, so that your initial dose instead of being 1,000 Roentgens per hour is down to perhaps 3 and 10 Roentgens per hour.

It is an astonishing thing, but the situation is not nearly so hopeless as it seemed to us and as it seemed to others and that is, I think, why we undertook the simple kind of analysis before.

All we've got is a sis weeks' study. Mr. Breslin spent full time on it and Mr. Solon helped him on it, half time and quarter time. But, as I said, it is initially a feasibility study, but it was very worthwhile.

There is one gleam of light that I have seen so far.

DR. BUGHER: In the Nevada Operation to get in a recently fired area they build the tower, built those dikes and built additional soil, that did accomplish it.

MR. EISENBUD: We never saw that data.

[redacted]

DR. BUGHER: It worked very well.

DR. G. DUNNING: There are additional date. It's a clear roadway and backed up.

MR. SOLON: I don't think the clear roadway deal was incorporated in the report. I recall Al had seen this data.

DR. BUGHER: It was an operational problem of getting workmen to build a hew tower and what they did worked out quite well.

MR. HARRIS: I wonder if I can interpose. We have a tentative program drawn up to hold a sort of a roundtable symposium of all the people that have done work in this area in order to gather our thoughts together before we set up a final program of working it out.

MR. EISENBUD: Very good. Well, now the Panel will go on to Redwing. Redwing is the operation which I think was announced in this morning's paper. It was a very meagre announcement which simply said that tests would be held next Spring in the Pacific.

[redacted]

16

[redacted]

MR. EISENBUD: There were two announcements. One was that there would be a smaller shot.

The tests will start May 1st and there will be fifteen events. However, ten of these will range from just a couple of [redacted] as an upper limit of this group of ten.

Then there will be five that are in the [redacted] of which the first on May 1st will be a [redacted] megaton airdrop. And then there will be [redacted] surface shot and a [redacted] barge shot, a [redacted] barge shot and a [redacted] barge shot and when you add that up, it is quite a bit smaller program so far as radioactive yield is concerned than was cancelled.

Because, whereas the devices there were about [redacted] these will average somewhat less than [redacted] and the total fission product yield of a Redwing Operation will be around [redacted] in contrast to [redacted] during the Castle. So, it's about [redacted]

There is something about this program that has upset many of us and I guess this might

[redacted]

[redacted]

be a good time to bring it up.

As I say, of the five large events, one of them is an airdrop. Three of them are off barges and one of them is off land.

Unfortunately, the shot that is going to be fired from the land is practically clean. By that I mean, there are no fission products.

Now, we don't know enough about the induced activities - induced short-life activities that we can expect from the definition of a clean weapon to be able to come with a good material balance. Or, putting it in another way, to come up with a good estimate of what the radiological picture would be if, instead of being a clean weapon, it was a dirty weapon of bravo type or some of the others.

On the other hand, the barge shots which are being fired from barges on salt water or deep water are going to yield a particle structure which is very much different from what you get from a land shot and the fall-out pattern would be completely atypical with respect to what you encounter in actual war.

This means that there's a good chance

[redacted]

12

[redacted]

of coming out of Redwing with essentially no more information about fallout, quantitative information on fallout than we obtained during the one land burst, during Castle and the only way we got that information was that some people happened to be under it and a Japanese fishing boat happened to be in the wrong place, this is despite the fact that there has been a great deal of emphasis placed on the need for better fallout information.

AFSWAP has a really splendid program designed into it; everything that needs to be done in order to get information and the only problem is they are not going to have the right kind of shot.

Basically the trouble is that when this program was conceived -- this is my own opinion, but I do want to make this point.

Many of you remember that the fallout studies used to be a nuisance. You can carry them on, but you must not get in anybody's way. You took what things you could get, you took whatever shot they happened to want to get off and accepted the condition of firing which was determined by somebody else as criteria.

[redacted]

Of course the situation is different. There's a great deal more emphasis on the need for more fallout studies but we still haven't gotten to the point where the fallout studies are sufficiently important so that the people who are studying fallout would say, "we want a land burst, we want the work to be in a certain direction so that we are sure that our equipment is to be where we want it to be and until this is done, there's a large element of risk". And as many of you know, the fallout studies that have been conducted both in Nevada and in the Pacific have had a relatively low yield compared to what they could get if there was a little bit more control particularly over the meteorological conditions.

Chuck wrote a letter to the General Manager addressed to Colonel Starbird and it was endorsed by the General Manager and it seemed as though they were ready to put on a land shot just for fallouts so they would detonate one shot off of land and get the best possible firing conditions and then the thing fell through.

So, in describing the studies for Redwing, I want you to bear in mind that although these are well designed programs of

[redacted]

only agencies that have had many years of experience in this and taken advantages of some of the failures in the past, we may be measuring the wrong kind of fallout.

Salt water is not what we are looking for, we want land shot.

DR. DUNHAM: The program was so far advanced that there just wasn't any spot to burst a really sized fission shot.

Now, with Al Graves' situation, we've just got to go along with this plan.

MR. EISENBUD: This is very serious because we don't have enough information. All we have is a little bit of one shot and we can just use that so far. The more we go over our data that was gathered after Yankee and that other one, which were the last two shots where we did get a lot of information, we realized that these data are not too applicable because they were barge shots and we need a land burst.

DR. G. DUNNING: I think in all fairness with all concerned that we have known for sometime that the series was coming up and if there was - and there was a formal meeting in

[redacted]

[redacted]

AFSWAP many months ago; to try to introduce at this very late date, an additional requirement of a large shot on land, well, it just couldn't be done operationally without a terrific effort.

MR. EISENBUD: Well, it's a nuisance and this was decided at a relatively low level.

AFSWAP told me that they did not feel as though they were in a position to specify the shot. The staff just discussed it among themselves and we were just not big enough to ask for a bomb all their own, and so they dropped it.

DR. DUNNING: They had this meeting in AFSWAP. The General certainly was represented up there.

MR. EISENBUD: That's right; it was settled by the staff.

DR. DUNNING: It was not settled by the staff, it was settled by proper representatives from AFSWAP.

DR. CLAUS: Even though a very sizeable shot would have to be there, we would still not have information that could be transferred

[redacted]

to the continental United States for example, or anywhere else, particularly of the land people live in and have it really mean anything. It's an extrapolation of which there are so many unknown factors and one still doesn't have all the information he needs.

9 There's just no sense in making an accurate determination in the Pacific and then use that. This is a very large consideration in the discussion.

DR. G. DUNNING: Particularly because of the real estate situation in getting a pure land shot so that your crater would not involve a very large amount of water plus the differences in soil.

MR. EISENBUD: Well, on that basis you just might as well not make any study because when you're firing on Coral, the situation is so far removed from what you are really interested in it isn't worth doing because of some other factors that have been enumerated; then why spend several million dollars in undertaking a barge program? They are spending a good many million dollars on this barge study.

DR. CLAUS: There's one shot that

ought to produce a good deal of information. It isn't as bad as I think you intimated, perhaps with somewhat of a disappointment.

There are a couple of hundred thousand kilotons of fission.

MR. EISENBUD: A couple of hundred kilotons.

DR. CLAUS: All right, several hundred - about a third of a megaton , let's put it that way, but the shot is big enough to carry the stuff pretty high, so that you get distribution.

It's true that the quantity is going to be more difficult to study which is the activity when it disintegrates; there's a good deal of it and it is going to be carried out and distributed over a sizeable area so that if one can measure at these levels with any degree of accuracy, we should get a pretty nice determination of the pattern.

I believe that if you folks at NRL concentrate on that one shot, which comes early in the business so that there will not be so much attenuation, personnel and equipment should get some very worthwhile results.

[redacted]

MR. EISENBUD: Well, we hope so but some of us may not be able to read it because of the high ratio of induced activity of fission activities; we don't know what the induced activity means, quantatively.

Well, I don't want to start an argument, but I guess I did.

A VOICE: It may be that this will be the last test that will be made.

DR. JOHN HARLEY: They've said that several times.

MR. EISENBUD: I remember how simple the thing seemed. All we had was one air burst and one under water burst; there were only two sets of contours in the whole world and there were no problems.

DR. FAILLA: I'm referring to the UN situation.

MR. EISENBUD: Yes; the number of points that we actually had and the judgment that had to go into determining the rest of the profiles - you've got no problem because they are there or they aren't; there's nothing to compare them with. We need additional tests.

[redacted]

[redacted]

DR. C. L. DUNHAM: Isn't it more important to get some data on fractionation and things like that? The sunshine problem? I believe we spole about it at the last meeting.

MR. EISENBUD: Well, that is another matter isn't it?

DR. DUNHAM: Isn't it really somewhat more important?

MR. EISENBUD: I don't think so, I think the problem is, how many people you are going to kill with the close in fallout;when you've answered that you know how many hours also are left to worry about as far as long range fallout.

If you believe some of the figures that are in the literature, there is no long range problem.

DR. DUNHAM: I think that we have to look at this thing in many ways because you can get precise data.

It's the old delimma and then somebody shoots off one that is a little different or there's not a different relationship and

[redacted]

you've got a concrete situation that doesn't fit your knowledge anyway so that if we have to come to an atomic war, a lot of the stuff can only be learned at that time.

MR. EISENBUD: I know I did quite a bit of talking. We'll now hear from Mr. Graveson.

MR. R. GRAVESON: The Redwing program is based on two sections. One is the Monitoring program which is similar to the type of effort that has been put forward on Castle.

The prime objective is to document the outside fallout with the secondary consideration of producing emergency early warning in the event of fallout prediction people tell us won't be there.

The system is based on Castle experience but utilizes monitors that have been developed at a number of operations. It is of a considerably smaller and lighter design for completely automatic operation and will be located at one of the islands which I will show you on the map.

It consists of this monitor which has

[redacted]

a detector which has radiation range from 1-MR to 1000-MR per hour. It feeds into an automatic recorder so that we get a continuous record for analysis at a later date.

In addition to this fixed monitoring station, we will have automatic reports. we will have aerial squadrons standing by in case of a reading on an atoll.

These squadrons will cover precept flight plans which will give us information on each individual island throughout the whole area between fixed stations.

This is to give us complete coverage without the necessity of setting up an extremely large number of stations.

They will be equipped with these portable survey meters which were used on Castle designed to read from 005-MR to 100-MR per hour; the altitude factor can be used to extend the range in the higher ends.

Could I have the first slide, please?

Eniwetok and Bikini are located here. (Indicating). The A stations are the automatic stations using the automatic

[redacted]

[redacted]

monitors continuously recording and we will have films such as a Worldwide Network for correlation to Worldwide Network, includes a major station throughout the Marshall Island.

In addition to that we will provide survey meters since they are the most easily maintained to be read twice a day at far out locations -- Midway, Johnson, down in through Guam, Iwo Jima and Truk. These locations are so far out that we don't expect to require any early warning type of things but just documentations so that we can send out the aerial squadrons.

Second slide, please?

The aerial squadrons will be located in Hawaii, Guam, Kwajalein and with the weather planes at Eniwetok to cover the northern Marshalls.

These Eniwetok planes will cover the northern Marshalls probably about H plus 12 hours after we reash the Bikini shots and will be available for special flights as required.

Any one of these flight planes may be requested from the appropriate squadron to

[redacted]

[redacted]

cover the area in and around the automatic monitoring stations. This information will be reported to the Officer of the Headquarters.

Now, secondary is a program we have set up for work under AFSWAP - the AFSWAP fallout program which is the open survey by aerial survey techniques.

Next slide?

This is based primarily on the fact as Mr. Cassidy mentioned, that there are altitude factors that we can correct and these factors are quite reasonable.

From zero to 680 feet, here is 680 feet and we read a fallout of about 10 and at 200 feet where we normally operate we are reading half of the radiation intensity that will be read on the ground.

These "X" points are the only sea-water data we have based on the last two shots of Castle.

There is considerable deviation in the data; there it is here, it has been tied into this and has been considerably

[redacted]

18

worked out based on the problem of determining altitude accurately.

This program we are setting up now, we have got people especially assigned to work with the military altimeters, to keep them in as close a fine calibration as we can.

The detector, (next slide, please) because of these aerial factors, these detectors can be located inside the aircraft itself, and not dangling outside and we can cover large areas and cover them rapidly.

We will use a flight speed of probably 180 knots. A tophat detector at Wigwam was located in the tail of the aircraft with this running all around it.

To give you a little idea of what we have done in the past with some of these profiles, the next slide shows the actual profile traces after the Yankee shot on "Cancel" taken at various distances out. This was out pretty nearly a hundred miles with a radiation -- this is 31 miles on the scale here, so the radiation pattern is quite broad, and it is relatively flat-topped.

Trace "A" here is on a twenty mile scale

[redacted]

and I can't read this one, but as I remember it was on the 30 mile scale again.

Covering a relatively high background reading on our aircraft which was our limiting factor, we redesigned the equipment to try to knock some of this contribution out by shielding and also by careful maintenance of the aircraft which we had specifically designed to this work at this time.

The next picture shows after the Nectar shot; the flight plan that we filled covering around the area, that is that not only for areas where the fallout was but where the fallout wasn't which documentation-wise has a greater significance.

We covered quite a bit of ocean with one aeroplane.

This time we are working two planes in the air simultaneously, with two aeroplanes to be used on the following day when the radiation intensity starts to get down lower.

This survey was run on B plus 1 and at that time we found a small lobe out in this directly which we believe was due to the stem.

[redacted]

[redacted]

While this pattern on the following day - the day subsequent to this had expanded of course, due to ocean drift but was still measurable, backed by decay and dissipation, this pattern over here almost completely disappeared.

The scale on this map is up to 100 miles. We did no surveying within 20 miles on that operation. We worked along in; this time we are planning not only to run through this area but correlating with other projects that will be located on the atoll stations.

Next slide, please.

This is the Wigwam data where we had the scale here. This is only 24 miles but I show this to show the eye plan of the patterns and how it can be followed day by day as it moves.

At H plus 5½ hours, this was a total pattern; by 26 hours the pattern had shifted over here, at 46 hours while we determined this pattern and in 70 hours the pattern is starting to move with a little lobe breaking off it and in 96 hours the overall pattern is considerably larger.

[redacted]

[redacted]

This work on Wigwam was specifically correlated with oceanographic measurements, the oceanographic methods to get those and to find what it is and this was a relatively small pattern.

And so, by the aerial means we could plot out isodoses and determine the location of the samples, the sampling locations and then the ships could go exactly there because the ships move so slow in 12 or 13 miles the pattern could move out underneath before they can get that down to where it is.

The next slide goes into a little more in the heart of this as far as what we do with this. We take the dose rate in the aircraft; we have a factor of altitude and can turn us into that.

On the Redwing program we are going to study this factor about altitude again; we will take gamma spectro in curves at various altitudes by using a helicopter to attempt to determine why the altitude factor remains so constant over our field of measurement.

It should vary with time; it should vary with distance; it should vary with a lot of things, and yet we still come out with

[redacted]

awfully close correlation so that we want to find out whether it's a factor that we are, of course measuring, a high grade of radiation, By the time it gets up to us and whether there are factors that are predominant in here.

Gamma decay can be used to take the various profiles, correct them for the gamma decay and isodose plots as plotted.

The next step is to take this surface gamma rating and correlate it to dose rate as measured in unit volumes of actual samples and integrate this over the area. It should be of course, a correlation and then this will give us the step of going down into unit intensities.

Scripps Institute will study the depth of mixing, how deep it goes into the sea; they tell us from the Castle experience that it is very uniform and produces a homogenious mixture so that by calculation you can take the depth down to the thermic line and shove this back into an equivalent surface layer condition from which the land isodose or a land equivalent type of isodose could be plotted and actually in the AFSWAP program, this is what they are actually

188

[redacted]

planning on doing.

However, this does not tie us down to the percentage of the other distribution of yield. All people in looking at it say that the only way is to take the same samples that determine the Beta activities so that curies can be calculated going through the same steps you go down to a curie distribution when you correlate it to the MR reading and determine it in the isodose pattern.

If we relate it to the total yield of the device, this then gives the percentage yield within the local fallout pattern.

We are talking operationally of 400 miles as a maximum limit to which we expect our craft will have to fly. This covers an awful lot of ocean and will take us sometime to do.

The overall program that AFSWAP has set up to give it a very quick run through to tie it in a little bit, one is the study of the cloud and the distribution within the cloud is going to be done by firing rockets through the cloud to determine the gamma intensity and Ernie Pinson is going to take his aircraft and bore right on through and

[redacted]

the record data will be used to correlate through before he goes through with his aircraft.

This, of course is one of SACS very interesting requirements. The fallout itself or the study on the "coast" by the Chemical Corps, the Signal Corps locating gamma dose rate and gamma exposure systems throughout the Bikini atoll or stations both on the atoll and on the islands.

Fallout isodoses will also be plotted within the atoll region by means of stations, helicopters and data. We will correlate this through what our aircraft get when we go over -- we'll take a couple of passes over the atoll and correlate their data with ours to get more correlation.

The NRDL program -- primarily we will use the YACS, we hope into the fallour area between 40 and 50, 100 and 200 miles.

DR. DUNHAM: You might explain what a YAC is.

MR. GRAVESON: A YAC is a liberty ship which has a place down in the hold from which the whole group can be operated. The group goes down into this shielded room; they can sit right in the fallout or they use a washout system to reduce the situation on the deck and they sit it out down there. It's an expensive way of doing it and that's about the only way --

MR. LeVINE: You may describe what a shielded room is like because this is a rather cute way of doing it.

MR. GRAVESON: It is just a small compartment, but it has tanks all around it, and before they start with clean water this time they'll fill their tanks full of water.

They will fill almost the whole hold - it's a real big thing and they have a concrete slab on top of it. It measures on the order of three feet thick. So they get a little hot and uncomfortable down there, but they can stay down there as much as 48 hours.

DR. WILLIAM BOSS: How many will they have?

MR. GRAVESON: They have two YACS and an LST, but that would be out about 200 miles. They have platforms mounted on posts

[redacted]

above the washdown system where they will have particle collectors and will collect a total fallout - massive fallout and do individual studies on the particle size distribution.

DR. BUGHER: They'll probably be in the right part of the ocean.

MR. GRAVESON: This was the problem in Castle. We have a whole air conditioning room full of fallout predictions now, and so they are feeling very happy about this.

DR. DUNHAM: Clarence Lawson did that in the Spring.

MR. GRAVESON: He did pretty good. We will use that platform for calibration again. We will fly over those, we will have a monitor located off their bow so that our planes can see the same thing that they do to study aerial factors again.

Our aerial surveys will then be used and go back to the Scripps oceanographic vessels and we will take samples from the depth after the fallout is over and the radiation intensity starts to get reasonable.

The last is the effects program. It

[redacted]

is the study of the Scripps washdown systems measures that are possible on ships as shielding measures, as well as decontamination on structural materials, a coating and things like that.

MR. LeVINE: May I ask a question here? I'm not quite clear; wasn't it true that after Castle, Scripps said the mixing was homogeneous but this isn't quite the case, we don't quite believe this anymore?

MR. GRAVESON: This may be true; this is why they are going out to study it.

MR. LeVINE: I think this is too important a point now to leave it.

MR. GRAVESON: Exactly what happens here is the reason for the program.

DR. S. WARREN: Are any measurements being made --

MR. EISENBUD: I suppose Chick is going to have his program.

DR. DUNHAM: They plan the same sort of program that they had before. It's rather extensive across this country.

MR. EISENBUD: We now come to the discussion of the findings of the fallout moderating program.

I don't need to give you too much in the background because it was reported to you previously.

This map illustrates the location of our overseas stations and here we have the location of the United States station.

This network has been operating since 1951 and to date we have accumulated about a half million samples of information that has been processed in that little room in which you have been earlier this morning.

I will ask Doctor Harley to report on the results on what we call our cumulative fallout data up to date.

DR. HARLEY: Largely our problem has been one of increasing nuclear activity on the part of Russians and others as well as our own and whereas formerly, we used to take a large series of samples, that is daily samples and analyse these for total Beta activity and calculate various things we wanted to know, our perspective has changed slightly.

19

We are considering now that we are most interested in strontium. That the total mixed fission products is more or less secondary and since we now have trouble defining the source of fission products activity that falls out, we get the high yield weapon, the stuff goes up and comes down over a period of years.

We used to be able to count on a series being over in three months. We had checked on our network; there was nothing to measure, but after Castle, we are still measuring debris coming down from Castle.

The Russians come in with their shots so we cannot take their data, I mean their fallout and calculate just how much strontium should be there. So we have gone to the system of compositing these fallout samples on a monthly basis for each station and determining the actual strontium present.

If we do this for the world we can see that since March of '54, which is the Castle series, that we have a total mixed fission of product which we still measure a picture which varies -- this Castle just happens to be a mean value we picked for these three months and we have results from the Russian series and from the Teapot series but

[redacted]

as of September of last year we started analyzing these composites for strontium 90 and this is the strontium 90 on a scale such as this which is really ten times what the value is that is shown.

In other words, this value is one tenth of a millicurie per square mile.

This picture does, I think, give you an idea that it is going to be a little difficult to predict strontium 90 too exactly all the time from straight fallout measurements because we don't know the source.

This material up here is Teapot but how much of this level is due to Castle -- in other words, we don't go down to zero anymore in between these series.

DR. DUNHAM: These are worldwide means?

DR. HARLEY: These are means of worldwide area.

DR. DUNHAM: I'm amazed that Teapot contributed so much.

DR. HARLEY: It's rather surprising

[redacted]

and we have spent quite a bit of time checking off these calculations to see why that is so.

Actually the Castle fission yield was 6.6 megacuries and Teapot fission yield .04 megacuries of strontium 90. That is an awfully big factor.

You have a factor of 300, but one thing we must remember is that all of this is extrapolated data to this date. Therefore these have not been used as much.

In other words, this activity will not fall out or fall off as much to January 1st of '56.

DR. G. DUNNING: Each month you get this much? So that if you want to total the data you would compile this up?

DR. HARLEY: Yes. We have done that and I will show you the curve later.

Just on a comparable basis I have the data here for the United States and I think this is striking if you remember the preceding one if you remember the contribution in the U.S. of Teapot is very considerable on a ratio basis.

[redacted]

You see after Castle again, this happens to be a mean of the six months period from March through August. Then it slumped off here and runs along at a nice old level for the total.

We did not get too much effect from the Russian shots and that Teapot affects the US quite strongly. And here you see the strontium rise is so great because this is fairly fresh stuff and the strontium contribution isn't too high.

I think the more interesting thing rather than these sort of monthly figures, is the basic data on the cumulation. This is the world - is the strontium 90 data. What we did was to take this 6.6 megacuries of strontium 90 supposed to be produced in Castle, and take our measured figures and see what percentage of Castle we were accounting for in this worlwide network.

If we take the first six months or so after Castle and draw a straight line we get something like this.

This shows some tendency to fall off. That is coming down at a slower rate which you hope it will do eventually.

[redacted]

[redacted]

Now, the contribution from Teapot to these measurements are -- well, we worked out a sort of a maximum value for this contribution merely on the basis of taking the strontium 90 fission yield for the two series and saying, that Teapot came out during the period of six months.

All of the strontium 90 landed over the world and if it did so, it would make a certain contribution to this and if we subtracted this contribution we get something like this (indicating on the board) which again seems to indicate more of a levelling off.

This is an extreme Teapot contribution, this is ignoring it and the truth is probably in between, but there is evidence of a levelling off, but there is still a contribution and this is from Castle which was March of 1954.

We have a couple of "fudge" factors in here. This only accounts for perhaps, well, actually that value is 3.72 per cent and this is about 3 per cent (indicating).

As Ed Hardy is going to show a little later we have some reason for this.

[redacted]

You take your 3.7 per cent and you say that 50 per cent of the stuff went up and the other 50 per cent was distributed locally, in the Marshalls and in the sea, you bring it up to 7.4 per cent.

We have very good evidence that on the basis of measurements which will be described, that due to losses, in our film, the fact that it is not a perfectly efficient collector and due to the fact that there is fractionation of strontium that the fallout at a distance from the test site, is richer in strontium than you would predict, that you need another correction factor of about four and on that basis we believe that the Worldwide Monitoring Network up until now has gotten perhaps as high as 30 per cent of what we would expect from Castle. Of what we would expect to find.

It's 15 per cent of what actually was in the yield but we don't expect to find half of it -- we figure about half would be here (indicating.)

This enables us to perhaps give a reasonable prediction of how things will look in the future due to continued fallout from high yield nuclear weapons.

There has been a lot of speculation on that and this sort of work may give a figure to multiply or divide the previous guesses to bring them more in line with what we think will really happen.

MR. EISENBUD: I think the Committee will recall at the last meeting I presented the data on the cumulative curve to about February and now it has been extended for another six months and the situation is about the same.

DR. DUNHAM: I'm lost John, on where you get the 30 per cent. The correction factor for separation ought to go the other way shouldn't it?

DR. J. HARLEY: No, this factor is purely empirical which as I say, is going to come up later.

10 Due to one thing or another there's an apparent factor of falls between those two.

DR. GLASS: This is exactly the information that the National Science Committee was hoping to lay their hands on and that Committee will be meeting again February 5th and

[redacted]

6th.

Will this be available to them or is it restricted?

MR. EISENBUD: If they ask for it, all right.

DR. DUNHAM:

DELETED

DR. BUGHER: You are supposed to leave out quite a few of these.

DR. FAILLA: The Committee is not interested in the location of the stations anyway.

[redacted]

302

DR. GLASS: Just the analysis.

DR. DUNHAM: This sort of thing should be readily available right at this point as long as it doesn't disclose where the data came from and how it was collected.

DR. FAILLA: This would be on the total fallout.

DR. GLASS: I think they want both.

MR. EISENBUD: John Harley mentioned one thing that I would like to discuss. One thing he said, this strontium 90 isn't anything that we are interested in. That is true unless we were geared to measure this because we can't measure the other thing which now the geneticists are going into -- they are interested in the total gamma dose since 1951.

Well, about all we can say is that somewhere between 10 and 100 milliroentgens accumulate over the four years -- they say that is not good enough. I can't see how we can do any better. You can see what the daily dose is. It's way beyond the ability of instruments to measure.

I understand if you haven't got the

genes and chromosomes pinned down within a factor -- why should they want this within a factor of two? (Laughter.)

And meanwhile, we are doing the best we can and they say it isn't good enough and I get kind of mad.

If someone can come up with a better method, with a number of roentgens over a four year period, well, it will be all right.

Well, we put those gummed papers in originally because it was about the only thing that was administratively feasible.

Hanson Blatz hit on the idea that originally we'll try it out for a few days to tell us where the fallout was occurring and where it wasn't occurring. We thought at that time - this was 1951, this wouldn't be good enough. We never thought we would want to use this data quantitatively. Fortunately, those gummed papers have worked out very well.

For the past two years we have been wondering about the absolute efficiency and we have two ways in which we can gauge that and I call on Doctor Brandt to discuss the comparisons that were made between our fall-out collections on the gummed paper and the collections in a pot that sat alongside of it.

DR. BRANDT: These data have been collected on the roof of this building where we have the typical stand that is used throughout the world from which we collect on two stands, a gummed sheet daily; on two other stands we collect weekly gummed sheets and then we have two pots up there, kettles of the old GI kitchen type, so that we have a rather good comparison. But of course we have no standard.

That is because we do not know exactly how much is falling. And so we are forced then to use a regression method of re-evaluating these two types of collectors.

If we took the activity that we get on the gummed papers, on the vertical axis and the activity that we get on the samples which can be collected in the roof part (now, these are for the weekly samples, only I'm not talking about the daily on the horizontal) we can plot the data for all the weeks and we have data now starting the 18th of January, 1954, running up to the 12th of December, 1955.

[redacted]

Ordinarily when we do a regression of this kind, we will call one variable the independent variable and the other the dependent variable.

In other words, we are supposed to have one variable which is exact. It is not subject to some variability and the other one, we state this in terms of this (indicating).

In this case, both are subject to sampling error and so we use what we call the third regression line.

You have a mean value for the activity collected on the gummed paper and the mean value of the sample collected in the roof pots and each one of the three regression lines will pass through that point, one in which we consider the roof pot as exact would be the one in which we minimize the vertical deviation from that line, another in which we consider the gummed paper as being exact and that we will be minimizing the horizontal deviations and a third in which we consider both as being variable is one which is true in this case and that falls in between.

I have drawn an idealized case here, one in which the variability is the same in the

[redacted]

two. That is not true in this case. The records from the roof pots are much more variable than the records from the gummed paper, not only that,, but they are generally somewhat higher.

So, I have done the simple stunt of reducing them to the same variability. There are two ways of doing that.

In one way we can divide each roof pot value by the standard deviation of the roof pot values and we can divide each gummed paper value by the standard deviation of the gummed paper values and we will come out with a variance of one in each case, and so the variances will be equal and we will get this situation, or, we can let these values stay as we record them and multiply the gummed paper values by a factor which will equalize the variability between the two.

We have been doing this piecemeal at various times and we have collected this data and this factor which we multiply this by, in order to equalize this (indicating) is around 1.6.

Summing up the 88 values that we have in this group (we actually have 95 weeks in this period altogether) but during that time there were seven weeks in which we had gummed paper of doubtful quality. So those data have been omitted from this study.

On those 88 it is now 1.5959 so I can say properly that it is 1.6 as the proper value.

The equation -- now, one advantage of this equation when we used the regression line is that we can solve it for either A_G or A_R because the equation is exact and if we solve for A_R this equation becomes this (indicating).

Now, I have just as a matter of curiosity, worked out an equation for these others. An equation for this one is A_G equals .43 A_R, plus 180.6 and the equation for A_G turns out to be this.

Now, if you solve for A_G here you will not get this equation because the two are not of equal variability.

Now, an interesting thing is that in going over some of the data from the NRDL studies of the last shots, in which they are getting the gamma level measured in milliamps, I did the same sort of things.

These were disintegrations per minute per square foot and theirs are in milliamps per square foot. We got a value of this ratio of 1.52 which makes us feel perhaps that this is a fairly constant ratio.

Now, they have the same papers and the same pots, the same as we had.

One of the things we were interested in this is how good our sampling is. And so, out of these records during this period we have 61 weeks in which we have two gummed paper samples and two pot samples so that we can do an analysis and get an idea of the sampling variability. And the average — let's take two pot samples in a week and we have a mean in each week and the mean in all of this period turns out to be 1108 plus or minus 389.4 and the mean of this turns out to be 581 plus or minus 207.1.

So that you see again, the variability in this limited period is about 1.51 A_R for A_R as compared to A_G which is about the same value that we had before.

209

[redacted]

MR. EISENBUD: That is very good. Our conclusion about these papers hasn't changed because our level of confidence has.

We concluded about two years ago that we were about 70 per cent efficient. Well, that was on the basis of rather flimsy tests but on the basis of the studies that Brandt has now completed, I have a pretty high degree of confidence in this, at least compared to a pot.

Of course we can't possibly say that the pot is the absolute method either. What we want is comparison of the two things we were interested in; one is the estimate of strontium as made by our gummed paper method and the other is the measure of strontium as found in the soil.

This is really the test.

There is more than just the inefficiency of the paper to consider here because as Harley points out, there is an inherent fractionation of strontium 90 with distance as well as the fact that there may be selected physical separation of the strontium 90 off of the papers.

[redacted]

[redacted]

It has been hypothesized that, if strontium 90 is on the outside of the particle, it is therefore relatively soluble with respect to the rest of the fission products. And so we collected 18 samples of soil from 18 places where these stations have been operating since the beginning of the program.

Ed Hardy is going to describe it. It is actually one of the most important things that we have to present and we can only spend five minutes on it because it is just a set of numbers.

MR. EDWARD P. HARDY: I am going to try to explain a factor that the Doctor used - the fallout data to describe the estimates of that on gummed films.

As Mr. Eisenbud pointed out, we want to know what the fallout of strontium 90 is on the soil.

I might mention first that you all probably realize that it is impossible, at least at the present time to have a mixed fission part of the soil -- in other words to determine how much is of the given soil at a given station.

[redacted]

[redacted]

However, Mr. Lynch undertook a soil collection at 17 of the gummed fallout, gummed paper monitoring stations in the United States and we analyzed the soil in this laboratory for strontium 90 only.

Here we have the results of this experiment.

The figures in red, the numerator is the measured value, the observed one, and the figure underneath is that predicted by gummed paper monitoring.

This is strontium 90 per square foot at the various stations as they are shown on the map.

The figures to the right in black are the ratios, the actual ratios of this calculation.

As you may be able to see the ratios in most cases are over one. In other words we were picking up more strontium 90 in the soil than we predicted from the paper.

As a matter of fact, the average ratio, the collecting I might point out, stations near the Nevada test area which are

[redacted]

close to ratio one or below, neglecting those three values or four values we find a ration of measured to predicted of about 3.8 with a standard deviation of about 1.2

This is the basis that we have used to apply the factor of four, the gummed paper data and as has been described, previously.

We conclude that this difference between our gummed paper results and actually what has fallen out of the soil is due as has been inferred before, to fractionation as well as the efficiency of our gummed papers.

I might mention, however, that this data may give us a factor to use and it doesn't look too bad analytically but we would certainly favor a continuation of this program.

I think just the success of this data as it stands would warrant a continuation and possibly to include foreign stations as well.

I think that about covers it.

MR. EISENBUD: Except to point out that once you get beyond a few hundred miles of the test site, you have a very narrow range

213

of ratio between 2.4 and 6. It is quite a remarkable thing.

DR. F. WESTERN: Is this based on strontium 90?

MR. HARDY: This is based on the mixed fission products that we have obtained on our gummed papers.

DR. J. HARLEY: This includes that.

DR. BUGHER: Your prediction includes the sampling?

DR. HARLEY: No.

MR. EISENBUD: This factor of two is calculated in what is up in the stratosphere.

DR. BUGHER: The analytical.

DR. J. HARLEY: Where we said, "assumed 50 per cent loss", that is 50 per cent of the cloud that did not get into our system or the fission, rather.

DR. WESTERN: Where you have the 3.7, what is that based on? Down on the

[redacted]

chart, is that 3.7 based on Hunter-Ballou?

DR. J. HARLEY: It is based on their initial fission yield.

DR. WESTERN: You have the factor of four which brings it up to 30 per cent; if you assume that a large fraction of the yield is from thermonuclear weapons, you are going to have still higher value.

MR. EISENBUD: This is based on a fast yield of strontium.

DR. J. HARLEY: They were the ones that put out the fission yield data at one time, or collected it.

DR. FAILLA: Did you examine some of the soil samples to see how much you recovered?

MR. E. HARDY: We don't know how to spike a soil that is a big problem.

DR. FAILLA: It is probable that there is another factor because you only covered one factor that was there.

MR. E. HARDY: It is hard to estimate

[redacted]

[redacted]

what would remain.

DR. FAILLA: Why couldn't it be spiked?

MR. E. HARDY: Well, actually we ran the soil by extraction techhiques - leeching techniques. This was not complete solution. It would be hard to predict how the strontium 90 is tied up in the soil crystal lattice and an actual spike would assume that it would remain in ionic form.

That is the reason why we did not attempt to do that.

DR. FAILLA: You would have to spike it with some more fission product, get it on a glass, something of that sort.

DR. J. HARLEY: You could mix in stuff.

DR. FAILLA: Yes, then you would know whether you are recovering 50 per cent or 90 per cent.

DR. WESTERN: Then you would want to leave it out two or three years in the weather.

[redacted]

[redacted]

DR. J. HARLEY: Our chemical recoveries of added spikes of course are very good -- to solutions at that time.

DR. G. DUNNING: This, I am advised, is that there may be three times more yet to come down as has already come down; am I to understand that?

DR. J. HARLEY: It is possible.

DR. G. DUNNING: It is three not thirty.

DR. J. HARLEY: Three.

MR. EISENBUD: Six. That is thirty per cent of a yield but it is only fifteen percent of what is up there.

DR. J. HARLEY: It's the other way around.

DR. DUNHAM: If what is up there is strontium then what fell out?

DR. J. HARLEY: I'm sorry, it's sixty per cent. The multiplication is wrong because we found 3.7 per cent times four which would be 15 per cent of the total.

[redacted]

217

You are right, but due to the fact that we think that 50 per cent of it has disappeared from the air, we have accounted for 1/3 of what we will account for.

DR. BUGHER: I see where the ratio is shown, but I don't see where the conclusion follows; where the fractionation has occurred.

DR. J. HARLEY: We can account for a factor of 1.6 by the measurements that Doctor Brandt has of just losses on the gummed paper.

Here we have a factor of four. It may be that our losses of strontium 90 are higher than for total mixed fission products or that, further out from the test site that you get more strontium 90 than you would predict from Hunter-Ballou.

DR. BRUES: You don't have to a direct measurement of strontium versus total activity on gummed paper soon enough after a shot.

DR. J. HARLEY: We have found that these ratios vary from what Hunter-Ballou predicts and it is our opinion based on just that accumulation of evidence, that the percentage of strontium is low, near the test site that

218

increases as you go away.

DR. BRUES: So that the real figure for what has descended is between the last two depending on the ratio between the last two variables.

One that the paper may reject is strontium and the other --

DR. DUNHAM: When you say 50 per cent loss, it may have been only 15 per cent loss if there was real fractionation.

DR. J. HARLEY: 50 per cent is the figure you select because then you will be nearest the truth no matter which way you go. (Laughter.)

DR. DUNHAM: Just 50 per cent of the general activity, but if there is real fractionation with the factor of four to one, that you only have ten or fifteen per cent of the strontium that actually was lost.

DR. J. HARLEY: It doesn't appear that the factor would be that bad.

For example, the Teapot. It looked like about two and a half per cent strontium in

[redacted]

the United States, and not quite three and a half in the rest of the world. I mean that is one sort of mild indication of the rage.

DR. BUGHER: Just as an exercise in logic, we could argue just as well that the whole sampling system of pots, gummed paper and everything else, only appreciates a fraction of what actually is available to the surface of the earth or what is bound to it. I think that variability of the ratio shows that.

MR. EISENBUD: All it requires is an assumption of fractionation to say, a factor of a little over two which is quite reasonable and consistent with measurements that we made here in the laboratory which was actually recovered and brought out on the air close to the test and at a distance that we find more strontium on the test papers as we go out a distance.

As a matter of fact, two I think would be quite reasonable because this factor of two is on top of 1.6 which would bring it up to the 3.2 which is pretty close to the 3.8 But I don't think we really know.

I mean, we cannot help but speculate about these things.

[redacted]

220

[redacted]

DR. FAILLA: What are the British estimates on this? Do they estimate that most of it is still in stratosphere?

DR. J. HARLEY: That is by extrapolation to infinity however.

DR. DUNHAM: From 48000 feet.

MR. EISENBUD: A very curious set of data.

MR. E. HARDY: I might just add, that we considered our results a little better than might have been expected. Because the range as Mr. Eisenbud pointed out, is from four to six.

11 As you can see the next topic is Analysis for Strontium-90 in Milk. We've been running tests since March of 1954 on powdered milk. That was first on a weekly basis and as of April of 1955 on a monthly basis.

This is what the data looks like. This is Strontium-90 in wet and dry milk in the New York area. This is January to December '54 to December of '55.

[redacted]

Now, from January '54 through to about April, 1955, we were running between 14D per quart to 2 to 3D per quart in powdered milk and then in April, or sometime between April, May and June, we evidenced a very distinct rise in strontium 90 content.

We felt this was quite real. I might point out that our analytical error is 2 tenths of a D per quart. That also, backed up by our evaluation, this is a real rise.

Furthermore, we analyzed some of these milk items for other strontium isotopes, strontium 89 and we received extremely high 89-90 ratio during this period, whereas through here, the 89 level was extremely well below strontium 90.

This backed up our assumption that this rise very probably was due to Teapot. This was during the grazing period and then around August and September, we presumed that most of the grazing animals are indoors because after August, we did not discover a distinct drop off.

As a matter of fact, it was very hard for us to discern whether there was an increase, a levelling or a decrease because we obtained quite considerable variability. In our eligible

[redacted]

results again, feeling that this variability was also real.

Just as an assumption we postulated that since the animals might be indoors during this period, very probably they are receiving solids and feed, that very possibly had been exposed to Teapot fallout previously and that may have been put into the food chain.

Actually, we will support this hypothesis, by continuing our milk analyses and especially during our next pasture season we will hope to be able to tell whether this is a levelling off or a decrease or at what level this decrease will remain at.

I might also mention that we are analyzing strontium 90 in milk from other milk sheds besides the New York area. We have Mississippi sending in samples; Wisconsin, North Dakota, Oregon and St. Louis areas, and this rise was very definitely confirmed by the analysis of strontium 90 as a result of the milk shed location for we did receive very large increases during this May, June and July period.

As a matter of fact some of the Mid West stations, North Dakota, for example [illegible]

[redacted]

[redacted]

went as high as 15D per quart.

Now, in terms of -- well, let me say just one thing, first. Back here, around April of '54, we also purchased 150 pounds of powdered milk to run as a control in order to determine whether contamination had occurred at any point in this sample and this is the result of a continuous monitoring of this controlled milk as we call it. Say from January '55 through this period when we discovered this rise.

Then as you can see, it's running between two and two and a half D/M per quart as it was at the beginning of 1954.

We were also continuing this control monitoring except we ran out of the initial control at this time. We purchased a new batch because we are now at a higher level because this is during the same rise.

In terms of tolerance, assuming 1500D per minute per quart of the highest result we have obtained to date, is about one per cent. And as of the fall of 1955, the last sampling period, we were running about 5D/M per quart which is about 1/300 of tolerance. Exactly what this means in terms of human accumulation

[redacted]

[redacted]

is quite difficult to estimate. I'm not even going to attempt to do this except to say that a human analysis program is under way to try to estimate this body burden of strontium 90 accumulation.

Just one other point concerning the dry milk in the New York area comparing this with our soil data we counted -- we feel that we have accounted for 400 D/M per square foot of strontium 90 on the soil of the New York area and this may be compared with 5D/M per quart of strontium 90 of milk as of the fall of 1955.

DR. SPIEGEL: Do you have any figures on the samples that have been collected from overseas or have they been too irregular?

MR. E. HARDY: Milk samples, you mean, I believe. Chicago is the best on this. And looking briefly through that report, there are some really highs and real lows as a matter of fact.

DR. SPIEGEL: The same thing with the soil?

MR. E. HARDY: Yes. We are monitoring DELETED [redacted] as well. I forgot to mention that

[redacted]

[redacted]

and [redacted] although we have no [redacted] results, [redacted] has been running about the same as controlled as a matter of fact, right through August and it is only during this period of September, October, November, that we are also seeing an increase in the strontium 90 content in that milk also.

MR. EISENBUD: The last item on the program is John Harley's final recapitulation I guess, of the Pacific Contamination story.

This summarizes everything on this fission analysis. And now that we have that cleaned up we can start all over again.

This is the Troll Cruise.

DR. J. HARLEY: We haven't gotten this cleaned up as much as we would like it. I'm just going to give a few seconds' summary here.

I think, as you all know, after the March 1st interest, there was considerable interest by the Japanese.

They sent out an oceanographic vessel on this cruise which is outlined in red.

11/7

[redacted]

[redacted]

The highest result that they obtained was on this leg which was about 150 kilometers west of Bikini. Then on this leg they have their highest spot here and on this leg here (indicating).

This was with the idea that this was going out with the ocean current and it might be worthwhile to explore this even at a time, a year later than the March 1st shot.

That was Operation Troll and we covered considerable distance in the Pacific in our course and we found values which are indicated here as a number which is D/M per liter of sea water per slash as against D/M per gram of plankton.

DELETED

Our most important feature of a study such as this, this is nice scientific information, would be the results on fish - that is

[redacted]

[redacted]

the edible portions of the fish and how much material was present.

In the first place we didn't catch very good fish, we weren't very good fishermen, we bought three from a Chinese fishing vessel which we hailed off the coast of Merti.

The only incident I'd like to bring up is the fact that after about ten minutes they managed to run up a Chinese Nationalist Flag but we felt that if our ship had had a Russian flag they could have gotten a Communist flag up in ten seconds. (Laughter.)

They had to dig into the sea locker to get that.

Our general conclusions from this cruise roughly was, that there was widespread low level activity throughout the Pacific with a maximum value of about six hundred D/M per liter and that the plankton ran roughly a thousand times the sea water at times; that is, a liter of water was roughly equivalent to a gram of plankton and that there was concentration in the current streams.

When it came to fish, a few fish that

[redacted]

[redacted]

we had we came to a value of three and a half D/M per gram of fish ash which is about one per cent of the permissible level and that is assuming that the person lived entirely on a diet of that fish which of course, in Japan, is not completely impossible.

DR. WILLIAM BOSS: What type of fish was this?

DR. J. HARLEY: This was the tuna. We felt after we got through that one interesting feature would be to calculate very roughly, (you understand that we didn't really cover this ocean in the way we would like to for this sort of calculation) how much of Castle did we account for by these sea water measurements and it looked like about twenty per cent. we could account for in this region in here.

If we take a larger region where there was some activity on the return to the States in the lower level, you might be able to bring that up to thirty per cent by integrating a large area of ocean and a low level of activity.

But, it was rather interesting that we were in the same region.

[redacted]

[redacted]

[redacted]

I think one of the conclusions has to be that in any operation such as Redwing coming up, that we are going to have some sort of interest in what goes on in the sea, both in forestalling public relation problems and in the actual scientific interest.

For that reason, I feel that what we should try to do is to perform a time study of the activity at some region of the main North equatorial current, that is to traverse this North equatorial current at reasonably close intervals and measure and the rough course that I would set up is to take Guam as the headquarters and to take a run, roughly like this and go back to Guam. This would be a cruise of about 400 miles which could be run every four days by a slow ship (indicating).

Samples could be taken off the surface; the correlation there of surface and depth is pretty good and it would give us a chance to see at what rate this material is borne through the North equatorial current.

At the same time, we will have to do

some work on fish samples, preferably I presume, taken more or less at the consumer level to see that the levels remain at about what we found for Castle.

Now, there may have been hotter fish found in here right after the test, but in terms of the actual -- probably the most toxic of the isotopes still, we are only dealing with a few per cent of the permissible levels so far. This may not hold, but that is our best estimate to date.

MR. EISENBUD: Thank you, John. Gentlemen, the adjournment time is 5:15, it is now 5:00. I had hoped to be able to make a few summary remarks and project a little bit of this into the future, but I don't need to do this now, I can do this tomorrow.

DR. FAILLA: It might be better to do it now.

MR. EISENBUD: I want to re-emphasize that the program you have heard today is a program that is in progress now.

If you heard it six months ago, it wouldn't be very different with one exception. None of the items have been reported in internal reports.

[redacted]

We have a few things that we are thinking about for the immediate future and I would like to mention a few of these.

We think that one very intriguing study can be made and plans are on the way to implement this -- "Uterik" Atoll is the atoll furtherest from the March 1st shot where people were exposed got initially about 15 roentgens and then they were evacuated and they returned.

They had been living on that Island; now that Island is safe to live on but is by far the most contaminated place in the world and it will be very interesting to go back and get good environmental data, how many per square mile; what isotopes are involved and a sample of food changes in many humans through their urines, so as to get a measure of the human uptake when people live in a contaminated environment.

Now, data of this type has never been available. While it is true that these people do not live, I would say, the way Westerners do, civilized people, it is nevertheless also true that these people are more like us than the mice. So that is something which will be done this winter.

[redacted]

[redacted]

We are very much impressed by the fact that this may be the last decade maybe only the last few years in history when it will be possible to really get some good data on natural radiation.

The natural background has not been really disturbed yet. It still reads the way it always has in my estimates, but it is changing fast. In ten years from now it may be too late to ever know what people were exposed to back in the aboriginal days of 1945 and 1950.

I think also that with the instrument, with components that have become available within the last year or so, it is quite feasible to make these low level measurements over large areas, relatively inexpensively.

We are going to take worldwide, we hope, documentation of background both as respects gamma radiation and the preponderance of natural isotopes and Leonard Solon is going to undertake the gamma background study and John Harley and his staff, the isotope abundande study.

We think that this is something -- well, what we are going to do is try to have

[redacted]

a plan ready in the next few months so that we can present it for possible international cooperation.

Included in such a study for example, would be the quality of the natural radiation which has never been really measured.

Then, Mr. Blatz has been concerned over the general problem. I know he has been concerned, Doctor Failla for many years of how you expressed the dose in relation to this and how particular do you evaluate the soft components of radiation so that we are going to take a look at the whole of our basic concepts of this measurement in which everything is expressed in one unit regardless of energy which may vary quite a bit, anywhere from two thousand up to a few million.

This will involve some better information about the gamma spectrum of radiation as it occurs not only in military situations, but in industry -- how much of it is up near the 2.2 of radium or how much of it is degraded down. It's never been measured and that is going to be undertaken starting soon.

As regards the long range plans in the laboratory. I think it's a curious thing and as a historical reason for it, we really don't have a charter.

We started out as a sort of - just a local laboratory in the New York Operations Office, and more recently, in the last few years, we have been working for every Operations Office.

We regard ourselves, and I think we are similarly regarded by Donovan and others, as a sort of an operating arm of the Division of Biology and Medicine but this doesn't appear on paper.

We do these things, we get huge sums of money with which to carry out these programs, but we have to describe our mission, and I think for the first time it is possible to describe it because civilian application program is involved and the kind of things that we have been doing for contractors rather successfully, I think for the past nine years are what we can do for the long pull for the licenses as the Commission undertakes to implement its responsibility in the health and safety field.

Somebody is going to have to provide a central check point, analytical facility and instrument calibration facility -- most of the

235

things we have been doing are going to have to be done to some extent for the licensees. On what scale, we don't know; it just depends on how big the Atomic Energy gets.

We certainly will continue this fall-out monitoring program longer than we ever anticipated we could do it, because, even if the testing stopped now, we would continue to have fallouts certainly for a decade.

There's a continuing program there which we regard just as a one-shot proposition which would be over once the tests are over.

We are very much interested in the economics of radiation reactions and some of the studies that have been described to you today, particularly this morning, are important.

We will continue to design programs - to design projects of that type and also depending to some extent, on the cooperation we get from the contractors involved because when you go in to this relationship for this kind of a job, you are challenging some of the administrative wisdom, telling them they ought to burns these bales of paper out in open fields instead of wrapping them up and sending them

[redacted]

to a warehouse.

This is not easy to do, but we will try it.

Then last, as just something to think about, the small Atomic Energy Programs that are being developed around the world, are going to need some place where they can go to get basic information of the kind that we have got here.

I was rather impressed by one visitor we had - a man named Ahmed, who was Chairman of the A.E.C. in Pakistan. He had been over the country by the time he came here. He had gotten to all the National Laboratories, he had seen most of the reactors that were allowed to be shown, seen all the big accelerators and he was very much impressed and I showed him through our laboratory in about an hour thinking that it would be boring after having been to Argonne and Brookhaven. And then we went to lunch.

At lunch he said, "would you mind if I came back to your laboratory this afternoon and handled some of those geiger counters"? And I said, "sure, come on back."

[redacted]

[redacted]

It turned out that his mission in this country was a very simple one. He had two thousand dollars to spend; it was his first A.E.C. appropriation; he wanted to know how to do it and he decided he was going to buy two thousand dollars worth of geiger counters for his geologists. And here we are taking a man like that, we are showing him God knows what.

He came back here and he spent all that afternoon and part of the next day and this impressed me with the fact that, "by golly, we are the shirt-sleeved kind of people that the overseas laboratories can get this kind of information from".

Well, if they want to, they can take a scaler and operate the way our people do it in the afternoon, using survey instruments and the kind of things they will have to use and not sending them over to see those big reactors.

This capsule is the kind of thing we see for the future, but we have a very cloudy crystal ball in this business.

I certainly appreciate the opportunity to present this program to you and I hope you

[redacted]

will come back soon.

DR. FAILLA: Thank you, Mr. Eisenbud. I think that we have enjoyed visiting your laboratory and we appreciate all the work that is being done and you are doing and all the wonderful plans you have in mind, and we wish you good luck.

MR. EISENBUD: Thank you.

DR. FAILLA: Doctor Dunham wasn't here this morning. Do you have anything that you want to bring up now?

DR. DUNHAM: I have nothing to bring up. I'm sorry I couldn't be with you this morning. Merril did give me, about a month ago - or his people did, a very quick run over of things and maybe if I had the chance to listen to it again, I might have understood it a lot better.

I know you have all enjoyed this, and I think at this point we ought to make arrangements to go out to -- who's the fellow who is having dinner for us or something? What about tomorrow morning? I think instructions on that might be helpful.

239

[redacted]

- PROCEEDINGS -

Saturday - January 14, 1956

- - -

DR. C. W. SHILLING: To those of you who were with the Advisory Committee at the last meeting in Washington, I think you will recall that we had two presentations for that group. One by Mr. John Hall relative to the problem of our foreign effort as it related to education and activity, and the second one by John Kaufman relative to the domestic problem, the problem here in our country.

And after this meeting I did a very foolish thing, I talked to Chuck Dunham and said, "we really ought to do something as to the Division of Biology and Medicine because the challenge had been handed to us as you will recall and I felt we should pick it up.

Chuck said, "fine, that's a good idea. You pick it up." That is the reason I am standing before you now, making an attempt to pick up certain parts of it.

I would like to have you be very

[redacted]

[redacted]

critical of what we have in mind and if possible construct it so that we can go ahead and accomplish what I believe is necessary.

I will not take any time to develop the necessity for the problem of having more and better trained scientists in this country. This has been developed by every speaker, I think, within the last year and in Washington particularly, they are all jumping on the bandwagon.

However, as far as the highest authority of the A.E.C., is concerned, I will read two short quotations from two rather long speeches by Strauss.

Coming before the International Atomic Exposition he told about the tremendous possibilities of atom in virtually every field that contributes to life, liberty and the pursuit of happiness and I am reading what he said.

"Yet there is a cloud just over the horizon of that bright promise even though it may be obscured for some by the dazzling brilliance of what has been already accomplished in exploiting the peaceful atom,

[redacted]

[redacted]

and the glowing descriptions of the untapped treasures still to be found within the invisible nucleus.

"You men and women of science and engineering realize -- more acutely than the rest of us -- that our country could forfeit many of those treasures unless it continues to have sufficient numbers of trained people like yourselves, possessed of the skill and imagination to prospect and mine them, and to bring them into the lives of the people. We must have a large reservoir of scientific and engineering talent, and make certain that this reservoir is constantly replenished, otherwise as a nation we will be out-distanced and left behind.

"The fact is that as a nation, we face a mounting shortage of skilled manpower -- a shortage which involves every field of science and engineering, and one that is particularly serious in the field of nuclear technology since the nuclear art depends upon practically all the many specialized skills of science and engineering."

One other very short quote - he found something in Shakespeare I thought was good here, ".... ourselves and children

[redacted]

have lost, or do not learn for want of time, the sciences that should become our country."

Shakespeare said this way back there and I've heard a good many people say the same thing recently.

He goes ahead and in speaking of the need for trained scientists he closes with these three sentences.

"It is a paradox that we should find ourselves at this point in history suddenly poorer by the very means by which our greatness was achieved.

"This is the cold war of the classrooms.

"In five years our lead in the training of scientists and engineers may be wiped out, and in ten years we could be hopelessly outstripped. Unless immediate steps are taken to correct it, a situation, already dangerous, within less than a decade could become disastrous."

That is a quote from our boss and with that in mind, we began -- there are lots

[redacted]

of other similar speeches, many of the Commissioners have said the same thing and we are familiar with it.

But, with that in mind, I just had to find out if I could, what A.E.C. was actually doing and I was quite surprised in this review.

I got in touch with Massey, and she is writing a big document. This is a rough draft and I am definitely not going to read any of it but it indicates, I think, if I simply read the list of things she discusses what we are trying to do, so far as Atomic Energy is concerned - to do our part in this matter of training.

She points out University or Research contracts have people trained on them. Of course we realize that - graduate training of the A.E.C. laboratory, resident graduate program, college graduate program, research participation program, fellowships, radio isotopes training program, cancer research, reactor technology training, on the job training and other kinds of training and she writes quite a dissertation as you can see here of the various aspects of this training.

[redacted]

Then the McKinney Panel asks the question in effect, saying that the Act isn't quite clear, that the Commission has a definitely defined responsibility in training.

"What do you consider the responsibility", the questionnaire asks.

And the answer was, "the A.E.C. has for years provided considerable support and impetus for training in the Atomic Energy field". The following efforts should be noted.

1. Through several assistants to schools at all levels by providing information, training, materials and equipment exhibits and advice on, of course, content.

2. Courses in instruction --

3. (Continues reading).

And sudden visits of foreign students to the A.E.C. facilities. That gives us some idea of the training in a general way that the A.E.C. as a total organization is sponsoring.

When I went down to Oak Ridge, I started talking training there on some of the ideas that he had been trying to work out, and I found that Alex Honger, in his Biology Division has two pages of things that he has been doing in this area.

I think, as far as I am aware - and I stand corrected if this is incorrect, Oak Ridge is doing more than any of our other laboratories so far as training is concerned, both in the Oak Ridge laboratory and of course, in the "Orens" Institute of Nuclear Sciences.

They have, for instance, a traveling lecture program in which you give one hundred to one hundred and fifty lectures each year -- I'm talking about Alexander Hollander's section right now; this whole paper is on that, - the Oak Ridge National Laboratory, Biology Division and the interesting thing is that I saw some letters when I was there, one from the Dean of a girls' school where one of the scientists from Alex's lab had talked and they were really quite excited.

Four or five of the other girls had changed their major because they had suddenly found out in the South, that a girl could do something besides being a secretary. It was the only thing that they thought they could do as far as work was concerned and they found that there was a place for them in the laboratory.

Four, after this one talk changed their major and when you realize from the information we are able to get between 70 and 80 per cent of the MD's in Russia are female and many of the other scientists are, we certainly shouldn't neglect that area of interest too.

Then they have a research program down there. They have, for example, summer courses, radio biology, a six weeks' course in radiation biology which was taken by the members at the University of Virginia during the last two years and then our radiation biology course was started under Duke University last year and will be repeated and so on.

They have a whole series of things where they bring people in to be trained at the laboratory and where they go out to give lectures, where they especially arrange for a special course to be given during the summer, all designed toward creating more interest and eventually developing more people who work toward a scientific degree in one of the many disciplines.

The ORENS is particularly active in this, but active almost entirely in the physical sciences and nothing in the biological sciences.

What the Alex Honger's group is doing is the only effort in the biological sciences that I was able to find so far as the Commission - the total Atomic Energy Commission is concerned.

We had a chance to hit this in the 202 hearings. This is a presentation before Congress coming up sometime in the next month or two. I will not read these because they are not of sufficient interest but to let you know that the Division of Biology and Medicine did put out a 202 Training Document and this was combined with the total training of scientists and engineers, a document which the total A.E.C. is going to present. And so there is an interest in this from that standpoint.

Then, in addition to this, Chuck

248

[redacted]

Dunham can talk better on some of these things than I can because he was a member of the Committee, but they have draft papers that are designed toward a final effort to really clarify the total problem of education and training and the duty and the responsibility of the Atomic Energy Commission.

These draft papers are to present the things they now consider to be legal and the things that are doubtful or illegal under the law are to be presented with corrective legislation proposed so that we can do the things that are necessary to do.

This draft paper is again mixed up extensively but gives a lot of interesting information.

Then there's a special Ad hoc group that is rather interesting.

They met together to talk about the business of fellowships and this is the one that you were on, Chuck.

A number of very interesting ideas came out. I was quite intrigued with one. They had three types of fellowships discussed

[redacted]

here. One was the Top Prestige Fellowship; this is the one I want because it carries a stipend of ten to fifteen thousand dollars a year which I think is very nice.

Then there is a High Prestige Fellowship and so forth. Actually what is happening here is that the boys were told at the beginning, dream anything they want to dream, let's see what we can get out of this and this document is not one that has the wholehearted approval of the total A.E.C. at the present time but this is a series of ideas - and they have a great many different ideas for travelling lectureship programs, so forth and so on with honorariums and all that sort of thing that one would like to do if you had the money and had the ability to really get into the training program.

I think this is good because there is no use in starting out of thinking in niggardly terms, you might as well think big and then go down; it's much better than to start with little and get beaten down to nothing.

I am rushing because I want to have some chance for discussion because there are several things I'd like to have. I am not yet at the point where we present what we

250

really want to do. But there is one other backed up piece of paper. These are facts bearing on the high school science of students.

In case anyone doubts the fact that there is a problem, read these yellow pages and see what we are up against there. It's a little bit rough.

We have, I think, by this at least established the fact that there is a need for something to be done. If not, I'll be glad to document that further-and we have established that the A.E.C. is at least aware of that because we are bearing an interest in this area.

We have a few ideas of our own that are quite largely borrowed from others but there are some additional pieces here that make a little difference.

So that there will be no misunderstanding about this, I will stick fairly close to this text, although I will not read the whole thing.

This is the presentation that we, as a Division of Biology and Medicine - this is not my own, everybody has worked on this, present to this Ad hoc Committee that is going to consider these new ideas and in the meantime, in talking with these various people involved, I talked with a number of them - John Hall, Kaufman and others.

They said, "well, wait for the Committee. Let's get going because what you want to do sounds to us to be both right and legal and very much what should be happening."

With that in mind, we have made some tentative contacts that I'll tell you about.

Just a review of the facts in background material. I said in the context of this particular document a brief review is all that is necessary on some of the facts bearing on this problem.

a. Technology in our nation is advancing so rapidly that an ever-increasing number of scientists and engineers are required each year.

b. National survival depends quite largely upon our technological advances.

c. More trained scientists are needed for research activity vital to our national progress.

d. The Russians are reliably reported to be directing their educational process so as to produce an ever-increasing flow of trained scientists and engineers into their national effort.

e. It is reported that the number of high school students taking mathematical and scientific courses is decreasing each year.

f. In addition, it is reported that with each passing year, fewer students are electing science courses in college.

g. At the next level, there is also an alarming shortage of graduate students in science and engineering.

h. As a logical corollary, it follows that we have a decreasing supply of career scientists and engineers -- and this in the presence of an ever-increasing demand.

This sets the stage for something to be done.

253

[redacted]

Next in the discussion we point out that there are many possible solutions to this problem that we are not in any way thinking about one solution as better than another, but at least we have some idea what we would like to go ahead with.

We will also point out that this is not, so far as we are concerned, a unilateral approach. This should be done as it is being done by the physical sciences and so forth, that we are simply adding our little bit to the total program.

Now we recommend one

1. Establish at A.E.C. National Laboratories and other selected institutions -- and this is the point that may be in question so far as how we handle it -- the other selected institutions a summer course of instruction for high school and junior college teachers and for science professors of small colleges.

The rationale of this is, that from what we are able to find out in talking to the various educators around Washington and in other places, and in talking to Science Service at the National Science Foundation and

[redacted]

[redacted]

some of the others who have been working on this longer than we have by a good deal, point out that the really only way you are going to beat this downward trend is to get the science teachers in the high schools interested in science themselves to the extent that they resell it to their students and this is not too easy a proposition when you find out that some of the high school teachers were trained, so far as their formal education was concerned, either in music or art or literature or athletics or something like this and then they turn around and are told that you will teach a science course and so it is a fairly rough proposition.

2. Plan the content of the course of instruction - which we plan to give - so as to provide enough general information concerning the use of atomic energy, so as to equip the teacher with material for his general science course, both for lectures and for demonstations.

The idea back of this is that the science teacher himself sees boys and girls every day during the nine month period of their school. You ought to give him the tools and let him present them to the children, because what has happened is that you are

[redacted]

[redacted]

undercutting the teacher in the high school and in the junior college when you bring in the great beautiful demonstration and a man from the outside gives the lecture -- it's really quite easy to come in with a lot of gadgets and a lot of power, you see, and give one lecture and the students would say, "well, wasn't that wonderful; what's the matter with that dummy high school teacher; why isn't he on the ball, why doesn't he know about this?"

The approach therefore here, is to equip the high school teacher in the summer course so that he will be the one who can inspire his students because he sees them on a daily basis. You have got to set up credit for this too, if you possibly can.

Now, in talking with the colleges - with the few that I have talked with, they are perfectly willing to give these summer courses so that a person can have credit towards a Masters of a Doctor's degree because many of the high school teachers -- of course they get paid according to how high a degree they have and this is part of the idea. And then, of course, a number of the small college teachers may be

[redacted]

[redacted]

advanced in degrees, but they are an advancement and we have to set up different types of instructions so as to accommodate the getting of advanced students.

This perhaps will not be the first year, but from then on you certainly will have to set up courses that are at different levels of difficultness.

We will have to give some type of assistance to the university itself or to the laboratory.

We cannot turn to one of our labs and say, "look, set up this course and do this thing", without being willing to put out a little additional money on this.

If it is in a university we would have to be willing to equip the laboratory that they are going to use for demonstrations and for training in the laboratory part of the program.

Then, it seems to me, we will have to furnish at least one full time salary. I'm talking of the university for the moment and not for our own laboratory - one full-time

[redacted]

[redacted]

salary of someone who can set up, ride herd on the thing and run the laboratory part and introduce the visiting professor, so forth and so on, so that you have one man who is identified with the course of instruction and the high school teachers who are taking the course can go to him for the little bits of advice that they want from him in each day.

This would mean of course, one full time salary. We are going to have to run these schools at any other than present A.E.C. installations.

I'm sure for the first year or two we are going to have visiting lecturers come in to cover some of the more complicated parts of the program.

This means that we will either have to increase the amount of money allocated to the laboratory involved -- for instance, if it were up here in Boston at Harvard, for example, in one place where they are willing to do it, we would have to have some of the Oak Ridge, Brookhaven people go up there and we will either have to put money in the Harvard contract to pay for transportation or we will have to augment

[redacted]

[redacted]

the contract down here, one of the two it seems to me.

In other words, funds for travel expenses and an honorarium for giving a lecture providing it's an outside man, you would probably have to get.

Then, of course, comes the problem that you would have to face. We can't run this on a one year basis. We have to look forward with it for at least two or three years before someone else can begin to take it over.

Another point that is very important is, you have to assist the person who attends the course - you have to assist the student because the National Science Foundation found out in a fairly exhaustive study of this that these poor high school students are about the same as college professors; they are going to do something in the summer.

Most of them either high school teachers or the young professors in some of the colleges have to do something during the summer to supplement their income.

Lots of them work in the nearest gas

[redacted]

[redacted]

station, so forth and so on and to get them into this, the National Science Foundation says you have got to pay them something.

They suggest on the average of a $100. a week to take care of all expenses. In other words, you pay them this money. That is what they are doing. If we do work out anything, we would not want to pay more nor less, I don't think.

We can't run into competition in Washington between various groups, the National Science Foundation has worked this out. They give an extra $30. a month for each child too, so that they can bring their families along if they want to and have a little extra for that.

The National Science Foundation has definitely promised that they will - if we start anything like this, will be very happy to come in with us on a joint sponsorship basis. This takes a little curse off so far as the Bureau of Budget in Congress is concerned if there is any part of this that might be in question. I don't think there is, but if there should be a question on that the National Science

[redacted]

[redacted]

Foundation has the law to do all of these things and the part that is in question could always be supported by the National Science Foundation. Being a good Washingtonian, this ought to work out all right.

There is one other thing that has to happen and that is, you have to assist the teacher in one other way.

(Discussion off the record.)

What we plan is this. That a high school teacher who comes to the summer course, who has successfully completed it be allowed to take with him a kit.

In other words, the man himself, like we say in the Navy, is married to the equipment; the equipment without the ability to use it is really worthless and in addition to this, many of the high schools in States have laws that you can't even give them things. So you have to give the man this piece of equipment.

This is being done now, down at Oak Ridge, and they tell me that it is legal in this sense - what you would do for instance

[redacted]

[redacted]

if you have ten students visit Harvard this summer, ten high school teachers who are students at Harvard this summer and you would have ten kits in the laboratory and this kit would be a simple source and whatever the boys in that outfit think we ought to have -- well, it would be enough so that when the teacher goes back and appears before his students he wouldn't just have to say Atomic Energy is wonderful.

He would say, "look boys and girls, here is what we can do biologically, physics and so on.

This is not too ambitious a program I don't think. Harry Kelly wants to have at least five. He is a National Science Foundation man in this area of interest, but what we are thinking of is perhaps three or four, I don't know whether we'll have money for four or whether we can get them activated, but three or four such programs, anyway.

For instance, I went up to Harvard and talked to them about it. The Harvard Medical School combined with the Harvard School of Education, are very happy to put on such a program this summer allowing credit,

[redacted]

26

[redacted]

the laboratory to be the Harvard Medical School and some of the educational parts to be run by the officers.

I talked to all the people involved and they said, "this is fine", they would be delighted to do it. Harvard would be one.

The University of New Mexico already has a preliminary proposal for something so nearly related to it that it could be changed without any trouble at all and there you have close by, Los Alamos and the Randy Lovelace set up.

The thing we talked about was our ability of actually running it at Los Alamos but there may be complications for running it, particularly this summer, this year, but we could run it at New Mexico, for example if these other people come over and give some of the lectures and then let people go back - the student group go back to the laboratories there for special demonstrations and so forth, that would be inconvenient to give so far as transfer of equipment is concerned to New Mexico.

Then, down in the Southern area, they already have been running one at Beaufort

[redacted]

[redacted]

under Duke and this would be just a matter of reactivating that this summer. The University of Virginia - Alex is not quite certain as to whether or not he ought to tackle that one because he has not been too happy with the way it ran and he does not know whether he can spread his forces too thin because he wants to work with the Beaufort.

Then of course, we are talking now to Doctor Dunham and I talked about this one actually at Brookhaven here because there has been a slight amount of political pressure concerning Brookhaven taking over more training activity. If we started one there, this seems to be fairly easy to do.

This has been cleared around the line in Washington so far as the other agencies are concerned. There is real interest on the part of every agency I have talked to including representatives of Health, Education, Welfare, National Science Foundation, Science Service.

I talked with some of the people in the Navy just from the standpoint of advice; they think it's a good idea.

[redacted]

[redacted]

There is one other rather controversial thing that I would like to bring in. This is quite a separate situation now, that is they are running what we call the station-wagon program down at ORENS.

That is starting there this summer. What they are doing - and this is under the National Science sponsorship jointly with ORENS, they are bringing in high school teachers who are willing to take a year off of their high school teaching.

What they are going to do isequip these high school teachers with a station-wagon as a means of transportation both for them and for their demonstration materials and they will furnish them with a large demonstration kit, bigger than the one we had planned to give to the high school teachers themselves.

They will have training both at Oak Ridge and under contract - as I understand it, with Duke.

The National Science Foundation wants Harry Kelly and personally Harry Kelly wants to add a biologically oriented station-wagon.

[redacted]

[redacted]

They had a pow-wow down at ORINS and ORL together, and got on the phone jointly with me, and said that they could not see running it for one; would it be possible for us to add two and make it three biologically oriented station-wagons. There are only going to be eight altogether.

The physicists don't think they can give up half of them or even three, but they would be willing to give up one if we could add two to it.

This means paying a salary, of course, of the high school teacher during the summer and they can train them down there without any question in this joint training program.

These are the two concepts at the moment that we are trying to push for this summer.

One, the training of the high school student, and two, questionably, the picking out of two biologically oriented station-wagons and these are the things that I would like to hear discussed.

I made this presentation much too

[redacted]

[redacted]

rapid, perhaps. I am, I hope prepared to answer questions in more detail, in case you ask them.

DR. FAILLA: Thank you.

DR. GLASS: May I start the discussion? I am extremely interested in this program. I think it's a great step in the right direction and having said that much, let me go on to make a few points about methods of approach that I think are extremely important in any effort of this kind.

14 Just the day before this meeting of the Advisory Committee started, I was attending here in New York, the first meeting of a Committee set up under the American Institute of Biological Sciences on this very question - the border question; not just this question of education of people who would know something about Atomic Energy, but a biologist and recruits for the teaching profession in general.

My point in bringing that up is this. I think it is a matter of fundamental strategy.

As this program is laid out, it appears to me to be a kind of benevolent

[redacted]

[redacted]

paternalism. That is, the A.E.C. sees the problem and is interested in doing something about it and it is going to initiate and carry out a program to that end.

Now, the A.E.C. has already gotten the reputation among biologists and a reputation quite undeserved I think, of being a little bit stand-offish.

It is so wealthy, so powerful that it goes its own way, somewhat isolated from the point of view of the generality of biologists.

I think that inaugurating a program like this, what should first of all be kept in mind is the desirability of encouraging the biological profession itself to do this thing; to come along and work with the A.E.C. in developing it and not to try to set up a program and say, "all right, here's a gift package for you, how about taking it?"

Both ONR and the National Science Foundation have recognized the importance of trying to get the biological groups to work this thing out for themselves - to work out these problems for themselves and the existence of this Committee that I mentioned to

[redacted]

[redacted]

start with is an indication of the fact, that the biologists themselves, are interested in this.

The high school teachers are our representatives as well as the college professionals, the physiologists as well as the geneticists and so on.

I think that it would be highly desirable to try to get a closer working relationship with the biological society or the biologists outside the A.E.C. to push these things.

I think that the success or failure of a thing like this is in the long run, a psychological matter and if you can get the high school and college biologists are working on it, it may well be very successful but if we try to work it out from the top and present it as a gift package it might not be nearly as bad.

And whether through the instrumentality of the Federation of AIBS or in some other way, I think that perhaps it's not sufficient just to get these courses set up in particular institutions and the availability of these things advertised. But if you can get

[redacted]

[redacted]

the biologists to come in and say, "this is a fine man; you let us help work it out and we will get behind it and push it and advertise it". It may be a much bigger thing even than we think.

DR. SHILLING: I ought to go to Milton Meade then.

DR. GLASS: Yes. For example at the present time the AIBS has worked out a scheme with the National Science Foundation for a contract to support the visiting biologists who will be selected by the Committee arrangements of AIBS and the bill to be drawn by the leading universities, to go out to the various colleges and communities throughout the United States, and visit for a couple of weeks, maybe giving one or two formal lestures on subjects of their own interests, holding conferences and conversations with students visiting the high schools and in general working up enthusiasm for scientific careers on the part of young people.

There's another thing of the kind that you are proposing here and it would be so suitable, it seems to me and feasible to make sure, through discussions with AIBS and

[redacted]

[redacted]

perhaps financial support for that particular end of it, that radio biologists were included among those who were selected to go out with visiting biologists.

DR. DUNHAM: I think Chuck's whole thought is going to be integrated with NSF's overall program. I gathered that AIBS has already been brought into that; the Science Foundation has already developed down at ORENS what it thought was a reasonable part of this overall program and we are just working right into it; isn't that it?

DR. SHILLING: Yes, I think perhaps you've got an idea that if we got in Cox of AIBS and Miltley of the Federation and talk with them at this stage, that we might get more wholehearted cooperation all along the line.

It might even be conceivable that it might be better to contract to one of them for handling this.

DR. GLASS: For certain things, not contracts with institutions probably, but maybe for the fellowship part of the thing -- something of that kind. That is the way some of these other summer workshops

[redacted]

are being handled now.

DR. SHILLING: I would think that legally we could not do it at all -- well, I may be wrong on this but I just don't think we can pay this portal-to-portal pay under our present law because we have these clearances and all that sort of thing.

I think it would have to go before AIBS or NSF or something like that.

DR. WARREN: I would like to reinforce very much what Doctor Glass has said and also to look beyond the biologist as such.

I think thatthe National Educational Association, they ought to be brought fully into the picture quite early also, into the general planning.

For information I gather that we still have the handicap of the clearance requirements on fellowships and scholarships.

DR. DUNHAM: Yes?

DR. WARREN: Yes, so that where we

[redacted]

ought to come in whenever possible, is more on the planning - the business end, the supplying of kits and so on and to hope that other sources can help take care of the stipend angle.

DR. SHILLING: Yes, I guess we cannot take care of the stipend; it's perfectly possible for NSF - for us to pick up some part of what they are doing and they'll take care of the money and material for that.

DR. WARREN; One other point that has some bearing here is the fact that these people once trained, ought to be called to FCDA's attention because they will be good trained monitors in areas where you may not have any trained radiation monitors, and that has been one of FCDA's main stumbling block.

They might be interested to come in to some extent if they thought it would help them get their hands on some key people.

DR. SHILLING: That's a good idea. I don't know whether they have any money, but I know they'd be interested in that. They would certainly like to have

[redacted]

these people identified.

DR. BUGHER: I think it is a very important concept in this. You can also find some very important elements in here. Things can get too cumbersome and too involved. Sometimes it takes so long that by the time they are accomplished they have lost their point. So that promptness in operation is one of the important factors.

I think in addition to the National Education Association, one should also think of OAS which is also pretty much of the grass-root type of outfit.

DR. GLASS: They have a committee on this type of problem.

DR. BUGHER: They are plenty of committees that talk about it, but this is when they'll do something about it so that in an attempt to put all these various efforts together, rather than make them go parallel would be quite fruitful.

It would help further that within the Commission, once a program has been generally agreed to, a set of directives could be given by the Commission to cut down the amount of just sheer labor and time consumption that occurs in getting something moving within the Commission's structure.

Oftentimes a simple set of directives would clear the way for Operations Managers and Administrative Personnel to get right ahead with the program without it being stopped all along the line.

It would also be helpful if the areas of active participation could be finally, reasonably well described so that various outside agencies could give their support to see where they could fit in and not wait until a crisis develops and then try to do something about it.

I do think the general approach of getting down to improving the performance of the high school teacher, is very fundamental in order to alleviate the present development difficulties.

DR. SHILLING: One of John's points is the timing of this; the total Commission is in the throes now of a restudy of the Commission as it relates to education in all the different divisions of fussing with this and the commissioners are all

[redacted]

talking about it and there's quite a ferment.

The best advice I can get however is that this will not be activated for any summer work until a year, for the summer, because you have to go through all of the scrap paper organization and all the rest of this business and probably will have to for the total big picture, get Congressional approval and the probable allocation of additional funds and so on.

And so, in talking with the various people who are working on this they said, "well, why not run three or four as a trial balloon this summer, to see how the thing would work and then, feed this information on how they work into the total picture."

That was one thing. The Directives I am sure, are essential and I hope will come, but as you are well aware, better than I, it's an awfully slow process.

What we visualized was running some of these this summer simply as an experimental approach to it, but I agree that we ought to talk with the various groups.

I have talked with a number but I

[redacted]

[redacted]

will see these other people.

DR. DOISY: I have heard one recent report that some have started it now, of college and high school teachers in the vicinity. I was referring to Washington.

DR. DUNHAM: They did have some courses and actually developed a little booklet, I think; you've probably seen it with the typical experiments which could be done.

It was done here and it was also done in Baltimore, but that was several years ago.

DR. SHILLING: Science Service is sponsoring something similar to this with their science affairs. They have booklets to tell what sort of scientific experiments high school students can do, where you can get equipment and that sort of thing.

I don't know whether this is what you have in mind or not.

DR. DOISY: I think you should inquire of the National Science Foundation for information. I think that is where

[redacted]

information can be obtained.

I found that there was a man who spent most of his time in the summer school, teaching high school teachers. Apparently this has been financed in recent years by some commercial organization. Maybe it is Esso, and it provided for tuition and scholarships.

DR. SHILLING: That's right.

DR. DOISY: Why don't you inquire about that?

DR. SHILLING: There are a lot of those going on. As a matter of fact, industries are interested in this and for instance, the University of Wisconsin has a quarter of a million dollars from the National Science Foundation to run a year round, in addition to the summer - a year round course in the training of high school teachers and small college professors in the special sciences, but none of them are approaching it from the standpoint of the radiation business that I have talked to.

DR. DOISY: There's been too much emphasis on radiation. It should be

[redacted]

on fundamental sciences.

DR. SHILLING: They're going to have a lot of fundamental sciences, I hope, anyway.

DR. GLASS: I'm not sure, but perhaps what you refer to in the Washington area is the project which the AAAS Committee, in answer to a comment that was made - we had many committees, but this committee is actually doing something.

It has, I think fifty thousand or a hundred thousand dollars, something like that from the Carnegie Fund for Education to carry out this experiment and it would work right in with this kind of plan on a different level, on a different stage.

The idea that they are working on is that of taking the specially trained and better prepared high school teachers most of whom are now located perhaps in large cities, in the better school systems, and in sending them for a period of time to smaller and more isolated school areas, to consult about teaching methods, the science teachers in those local school systems.

[redacted]

[redacted]

We have specially trained people who are familiar with the possibilities in high school education in applying the principles of atomic energy to fit into that program. It would help to diffuse this knowledge much more rapidly by them.

DR. DOISY: That's different. I understood it was being supported by the Citizens Committee.

MR. EISENBUD: It surprises me that this is a problem, because I think here in New York, they've had a pretty good program that goes back for five years and I think it's interesting because it serves to illustrate how much you can do.

New York is fortunate in that it does have a large school system so that it has full time people.

He walked into the Operations Office here about five years ago when Cornell was with us. The two of them got to talking and one afternoon there was a lecture course and then they came down to the laboratory and asked us about that.

John Harley got interested in it

[redacted]

[redacted]

and did quite a bit of writing about it and much of it has been published in the educational journals. But, out of it came a 20-lecture course in which - that is a laboratory course, in which the school teachers came to the laboratory with the kit -- of course, this was financed by the New York City Board of Education and they developed the teachers. And, John Harley and "Carbonel" developed a course for the students which has to do with isotopes, and I know also with audio-radiographic parts of the plant, assembling geiger counters and component parts, studying their reaction to alpha rays, absorption shielding.

It was a very nice course and it is now about five years old and I don't think it cost New York City very much, maybe about $100. per high school class to equip. It's a very modest program and yet it's been quite effective.

We came out of it ourselves with a very good geiger counter.

DR. FAILLA: That is an interesting repercussion to this program, that the Teachers' Union became alarmed at the fact that the teachers were made to handle

[redacted]

[redacted]

radioactive isotopes because of the danger to the teachers and so they wanted an investigation made and the representative of the Department of Education here, and the representative of the union and somebody else came up to see me to discuss a very serious problem.

I made some simple calculations and indicated that neither the teachers nor the students could get enough radiation to get hurt under any circumstances.

The union representative was very nice; she thanked me and she was perfectly satisfied and she said she was going to make a favorable report and there wouldn't be any trouble from there on. I don't think there's been.

MR. EISENBUD: I've never heard of this. The entire figure of all the teachers is about ten microcurie in New York City. I'll make a note of it and send it to you.

DR. CORSBIE: I would like to ask Mr. Eisenbud if he knows whether it's spread about New York or whether it's spread beyond New York City or was it just New York City.

[redacted]

[redacted]

MR. EISENBUD: It was just New York City and I don't know what happened to it.

DR. CORSBIE: The second question which does seem to have a bearing, is whether or not this would be reflected in the Regents Examinations which are required in New York or exactly what is the position that the Boards of Education or the State Superintendents would have in this overall scheme.

It would seem to me that it would be absolutely necessary to have their support if it is going to be in the curricula of the school and the requirement, both as to courses and selection of science teachers.

DR. GLASS: This is not an answer to the question, but the fact that it might reflect the nature of an answer to it, -- because of my own concern about this diminishing enrollment in the high schools and science courses and because of being on our School Board I was able to put a little pressure on to get the school work done.

I asked to have a survey made of an enrollment of sciences in the city of

[redacted]

[redacted]

Baltimore for the past ten years and much to my surprise, considering all of the advertising that the answer was quite clear. There had been a dip once after the war, but following that, it increased again, and the overall education was not changed in the percentage, it seemed, that had taken place in the physical and the biological sciences whatsoever.

I suspect the same is true for New York City and for other good school systems. The decline then, most probably exists where the teachers have been isolated and have not been able to keep up with things and haven't had these advantages.

DR. SHILLING: One point there; you cannot just maintain the status quo; if the other figures are correct we have to increase the proportion to meet the technological advances of our times.

DR. FAILLA: I figure that the ultimate objective of all this is to get more talented young people into science; those that have an aptitude for science. We should not waste talent. So that the training of high school teachers is perhaps the first

[redacted]

[redacted]

step in that direction.

I think most of the talent is lost, not in the large cities or where there are good public schools, but in places where there aren't good public schools and in the smaller communities that I would say the system of priority should be established in picking out these teachers for special training in places where the need is greatest or where they do the most good.

Once they get back, rather than pick out people say from New York City or Boston or Baltimore -- pick out teachers in the smaller centers where, however, there is a great potential of innate brain power that can be tapped, say fools, for instance. (Laughter.)

DR. DOISY: Out in the hills of Missouri instead of St. Louis.

DR. FAILLA: Any further discussion on this problem?

DR. SHILLING: I would like to hear one point on the station-wagon. Should we or should we not join that; no one has

[redacted]

[redacted]

spoken on that. I'll bet I'll have to answer it in the next three or four days.

DR. FAILLA: Where's the station-wagon going?

DR. SHILLING: It'll have a circumscribed area of probably three states near here, wherever that may be and he will visit during the entire school route. High school after high school, all over the area.

DR. FAILLA: I should think that for the course that might be very profitable.

DR. WARREN: I would be strong for the station-wagon idea.

DR. BUGHER: It would not cost much and there's nothing much to be lost. That would be something like the book-mobile idea.

DR. DUNHAM: You could learn a lot.

DR. FAILLA: Is that a satisfactory answer Doctor Shilling?

[redacted]

586

[redacted]

DR. SHILLING: I gather that everyone seems to be in favor of the station-wagon.

DR. FAILLA: Do you want a motion on that?

DR. SHILLING: No, it is not necessary. You may not be able to do it, but this is perfectly satisfactory.

DR. BUGHER: A trailer exhibit is already in existence. One of the finest pieces of demonstration equipment that has been devised and it can roll tomorrow.

DR. FAILLA: Let's go back to the 1:15 item.

MR. EISENBUD: In the last eighteen months the Division of Biology very quietly seemed to take on additional work of very considerable magnitude that hasn't been talked about very much and I suspect the reason for putting this on the agenda is, so that the Committee can be informed of the ramifications of the New York Operations Office.

First, let me say something which I should have said yesterday, but I didn't, that

[redacted]

[redacted]

may not be known to all of you, but I wear two hats. I am an officer of the laboratory floor and when I go upstairs I'm Manager of Operations. This situation has been in existence for about a year and a half. It has been working very well, largely because I'm blessed with a very good staff, both in the laboratory and in the Operations Office and Mr. Joe Clark whom you have met right now, carries the main burden of administration in the office.

I told you yesterday a little bit about the history of the New York Operations Office. It's quite a bit different from the others because it was originally a Headquarters Office for the Manhattan District. I guess it was the first office to be established.

General Groves operated out of there for a good while, particularly before the other sites got rolling and was all through the publishing office and the headquarters office and they had a very large measure of responsibility during the Manhattan District days and the velocity which was applied in the Manhattan District carried over into the Commission for a while and as the Operations Offices were developed, each was given a piece

[redacted]

[redacted]

of a program to administer. It was a rather large delegation of responsibility and as it was conceived originally, there would be a very small Washington nucleus at the Headquarters Operations.

In the course of events it didn't work out this way and gradually there was a centralization of responsibility. In fact they changed their name.

It was quite a significant change because originally we were the New York directed operations; we had complete responsibility for the feed material program for the research program.

We got our directions from Washington, but once the directives were spelled out the scope was defined, then it was up to the member of Operations to accomplish this. Then they changed it to the New York Operations Office.

It was a long period of getting adjusted because it never really was defined, just where the Washington responsibility left off and where the Operations Office would pick up.

[redacted]

For example, just to illustrate some of the problems that we were faced with.

I guess under the Act, we realized that we would have the Biology and Medicine program and I think at that time there was as yet no division. We had funds.

I guess your program, Doctor Failla, the Rochester program and quite a bit up at Harvard, that had carried on from the war and there was a considerable amount of money available in 1946 for research and we just went out and began to get some contracts placed and then there wasn't any organization to handle it in Washington and so it was handled over in ONR for a while.

We added at that time, one of the first people to join our staff after the war was Doctor Brant; it was partly because it seemed a good idea to have one of those men to be available under consultation. He still does that, but Doctor Failla, as you may have noticed this morning, when he came in on consultation except for Columbia, Rochester and Western Reserve -- he is working for the laboratory.

[redacted]

The feed material portions of the program were transferred over to Oak Ridge about eighteen months ago and at that time the Operations Office was placed under the Division of Biology and Medicine and I was named Director.

I assumed that responsibility in addition to the responsibilities which I have had for some years with the direction of the laboratory.

A program at the present time involves about $40 or $29 million dollars of operating dollars. By that I mean, a Search and development program which we administer amounts to about $29 million dollars a year.

In addition to that we have about $40 million dollars worth of construction on the books.

The biggest pieces at Brookhaven was with building the $26 million dollar alternating cyclotron which I believe you know about and there were two large accelators which have been approved. One will be built in the Boston area and they will be operated between Harvard and MIT and the other down at Princeton.

[redacted]

[redacted]

Yale is building a lineal accelerator at our expense. We have a Mattehorn project at Princeton.

The Medical Center at Brookhaven is already under way and so that totals about 40 million dollars which is a very large program and the interesting thing about it - and this is what makes it different from other offices is this:

This is all peacetime operations, it is all unclassified except Mattehorn and the size of it is simply a reflection of the educational attention that is given and the size of the institutional facilities.

All of this has made work for the Division of Biology and Medicine and I must say that we have had very, very excellent support from the Division.

In Washington you need a friend because there are many, many problems which arise almost every day which require concurrences of several staffs and most of it, I think, goes on the shoulders of Howard Brown.

DR. FAILLA: Yes, he's really done

[redacted]

[redacted]

a job.

MR. EISENBUD: Howard was always so busy and he seemed to do all the things he used to do and now he looks after our problems as well. And of course, Chuck has done a great deal of this.

It's a little strange the Division of Biology and Medicine should have to concern itself with the scope, the way in which a contractor works with nuclear metals up in Boston which is the metallurgical institution operated by Allegheny-Ludlow and I think this must have taken half of Brown's time for a while and I know you sat in it.

It's the peculiar way in which the Commission operates. They take the Division in Washington and give regional responsibility, and they say, "since you are looking after us, we have development problems in this area and special problems and application problems; military applications, civilian ones"; everything else the Commission does.

That very briefly is what we are doing and I'll be glad to answer any questions that you have.

[redacted]

[redacted]

MR. CLARK: The only thing that you didn't mention was the application program. We have about 200 access permits as they are called; we will probably have some licenses later on.

DR. DUNHAM: I think you have twice as many access permits as any other group.

MR. CLARK: We have on the order of three times that many. This involves quite a lot of your administrative work, for example.

I think we have had on the order of 1,400 clearances to date on those 190, 200 permits; about one quarter of those are "Q" clearances.

Merril, you have about covered the waterfront. You may be interested that our total number of contracts is around 400. Off the record.

(Discussion off the record.)

DR. FAILLA: Shall we move on to International Affairs, now?

[redacted]

[redacted]

MR. HENDERSON: I first want to thank you and the Committee, Doctor Dunham and Merril, for the opportunity to sit with you for these two days.

Both George Speigel and I are quite involved on the UN Scientific Committee and Merril thought it would be a good idea for just a background, and I think we've got that.

This morning I think I was a little over my head. I still don't know quite what the figures mean up there.

DR. DUNHAM: That was just to impress you.

MR. HENDERSON: I'm impressed. John Hall wanted to be with you at this meeting to follow up his discussion and some of the suggestions, I believe that were made at the last Advisory Committee Meeting.

He was not able to attend and so he askedme - since I was going to be here, if I couldn't give a brief statement of his report.

He mentioned in checking the transcript the bilateral agreement which we have, the research agreement that we have with some

[redacted]

[redacted]

24 countries.

DELETED

Checking on the status of these agreements very briefly, of the 24 research type agreements, 14 of these countries or I should say, 12 of these countries have actually made some choices as to the research.

Fourteen of these countries have actually made contacts with American industry and it is expected that five of these will let contracts early this year.

This gives just a very short status report on these contracts.

We are working like mad, to keep up with them. I hope we get on tops of them one of these days.

I believe the specific suggestion that was made last time regarding the establishment of radioisotope research centers

[redacted]

[redacted]

primarily because they could be established with the minimum amount of funds, they could be established immediately, and I have gotten the rest of the reasons behind it.

Ralph Strong in our Division has done an amazing job in six weeks and he has been in pretty constant contact with the Division and Doctor Pearson on working out a program to get this thing under way. I'm happy to report that it looks like it is going to work out pretty shortly.

They broke it down into three major parts. The first part is to send consultant teams to countries that are interested in this type of approach.

The second part - and I will come back to these, is to continue and increase our activities on the training aspect of this, particularly here in this country.

The other point is in providing funds and equipment, that is funds for equipment for use by this country.

Now, a number of them have funds of their own. But there are private funds available and in order to make a comprehensive

[redacted]

[redacted]

approach to this, we have blocked the three of them together.

Getting back to the first part of it, which is a sitting of consultants, I understand from George that IES has expressed willingness to underwrite this during the next year to come out with some ten scientists at the present time.

The thought was that they would send out teams of two and that we would have to recruit these consultants from our own officers, AEC laboratories, Universities, wherever we could get them.

The ten to twelve people going out would be what I would call, a pilot type project to provide experience during this next Spring in the establishment of such programs.

DR. FAILLA: How long a time would these teams be out of the country?

MR. HENDERSON: They were thinking of some four to six weeks on the trip. Now, whether they would actually be in one country for that period of time, I don't know.

[redacted]

[redacted]

It should be recognized that these trips will require a follow up, too.

The purpose of the original trip would be of two parts; first it would be first to find out what possibilities there are. To develop something in a specific country, you have to start with something first, and secondly, to provide consultant services to the country - to the scientists that are there and then thirdly, if they do have programs under way, to assist them in appraising that program from the standpoint of continued development.

I think these will be the three major things that we would expect these teams to do.

The total cost; we had originally thought of some twenty-five people in teams of two, in other words, twelve to fourteen teams. It cost around 100 thousand dollars to carry out this program which is really rather small when we stop to think of the results that we could achieve out of that. Particularly when you think of the research reactor; we share half the cost of the research of the reactor and that could cost 300 thousand dollars alone and I think that

[redacted]

[redacted]

gives a pretty good comparison of the cost involved and I am sure we could get a lot more out of it.

The second program which is increasing the training opportunities, I checked as to the number of foreign students that had gone through the appropriate school, and I found out, to my amazement there were some 97 students in the last three courses that had been graduated..

They would hope to increase the enrollment of foreign students in the isotope technique school at Oak Ridge and also to provide more opportunities on a planned basis.

I recognize that this has already been done informally for more specialized training, say from three to twelve months in AEC laboratories and hospitals.

I think Brookhaven has a plan. The estimated cost of this is some $285,000. I won't go into that because I am not too familiar with exactly what plans they do have marked for it.

The providing of the laboratories,

[redacted]

[redacted]

they have broken that down into cost. They figure that it would cost ten to twenty thousand dollars per laboratory.

At I understand it, there are funds available from the funds for peaceful uses and they, by the way, have been very interested in this whole approach.

Doctor Pearson and Ralph Strong have been in contact in a number of meetings with a number of representatives from this group.

The biggest problem that we have is not on the problem of funds as I see it, it is on the problem of getting qualified people who can devote the time to this thing.

I ran into this myself at the Geneva Conference. IES requested people to go over to see this and find the persons that they wanted with the background that they wanted who were available.

It's a really difficult task and it is to this point that John wanted your suggestions and how best, what techniques we could use.

[redacted]

[redacted]

Of course this is our main priority. We don't think we have to compete concerning other programs. This is our program and we have to push it, but we do need some help on the techniques that we should use in locating people, either within our own organizations, in universities - as a matter of fact, preferably, in universities and settling, on specific people.

We view with great importance, this first program because it will be the first really planned approach that we have taken on this.

The third point that John wanted me to mention was that we had received excellent reports from Doctor Bugher on his trip to Latin America, Doctor Farr and we are now at the point where I think we can do something about a lot of the suggestions that were made.

It's been a number of years that there has been no evidence of anything being done, and won't prevent further suggestions from coming in.

And in your contact with foreign scientists, there are a lot of suggestions like the ones Merril has told me about that

[redacted]

[redacted]

can be of use to us in formulating and in carrying out our part of this total picture, perhaps. If you can drop us a line this would help.

There are three things I would like to throw on the table, Doctor, and any help that you can give us will be greatly appreciated.

DR. FAILLA: Thank you very much. Now, they might make some suggestions.

DR. GLASS: Is the National Register of Scientists which is run in sections by the National Science Foundation of any aid in getting hold of a category of people an initial group that you could then screen further for these experts you want?

MR. HENDERSON: Yes, I'm sure; also, ICA. They have tapped a number of people but the problem has been that they have come to us, to the AEC to these particular skills. They don't seem to have the resources that they can go to and say, well you've got this training and they know it.

They want from us, recommendations

[redacted]

[redacted]

that these are the types of people that can do things. We are taking a much more definite interest in this particular program to the point where we want to be sure that there's some overall planning before they go out, for example.

I think Doctor Farr mentioned that in his report -- the need for comprehensive overall planning before these people even leave the country. I'm not too sure that that has been carried out in every instance.

DR. WARREN: There is a hand picked group among university contract personnel and I think that quite possibly a circular letter to the B & M Contractors as to their staff, their colleagues, they themselves that could give "X" weeks or months to this during the next one or two years, might not bear very useful fruit and any of them would be pretty competent individuals especially if you get them together for a little bit of exchange of information first.

DR. FAILLA: One thing that might be mentioned is the fact that if you are thinking of Europe, the summer months would be very, very inopportune. On the other hand summer months here would be very good in South

[redacted]

[redacted]

America, so that you have to make that distinction for those who might be sent to Europe. It would have to be during the academic year in Europe where they would find the activities going on full blast, and that is a little more difficult to find than the other type who might take the summer to go down to South America.

MR. SPIEGEL: Doctor Failla, this is geared to Latin America and for the Near and Middle East, not the European countries. They would not be interested so much in one of these isotope laboratories.

DR. FAILLA: That was one of the things we wanted to do.

MR. HENDERSON: The idea here was possibly on the first approach was that we would do Latin America if IES would buy that and the reason is quite obvious. They are not as advanced as Europe; it would be a good testing ground plus the fact when you really get down to it, we really haven't done very much for Latin America in this whole "Atom For Peace" program and I think Doctor Bugher made the suggestion of a reasonable approach which I think has a lot of merit.

[redacted]

[redacted]

I don't think that this should hold up this program at all, but it could conceivably tie in.

MR. SPIEGEL: We have recently tried an approach in Asia, with the Asia Nuclear Center and that has caused no end of heartburn.

DR. DUNHAM: You thought of only one center.

MR. SPIEGEL: All six of the countries wanted it located there because they know that the Philippines are going to get it and that is confidential that the Philippines are going to get it. They have all be informed confidentially but it's never been publicly announced; they all want one for nothing now.

DELETED

DR. BUGHER: Well there are natural regions and regions synthetically established.

[redacted]

[redacted]

DR. FAILLA: In fact, it's done more harm than good.

MR. SPIEGEL: I think this happened in the case of the Asia Nuclear Program.

DR. FAILLA: You antagonize our country by helping one.

MR. SPIEGEL: Just one other point. In the last few weeks five countries have approached us on a research reactor, Austria, Costa Rica, Yugoslavia, Germany and Ireland, bilaterally and so within a matter of days, we will probably have five more agreements.

DR. WARREN: Has anything been heard from Japan as yet, officially?

MR. SPIEGEL: Their 27th Mission is coming over this month to decide on their reactor.

DR. FAILLA: Another thing that I think is very important is the choice of the institution that is going to be helped or the individuals that are going to be helped because sometimes, if one chooses the wrong individuals, that would antagonize all of the other people in the country.

[redacted]

[redacted]

I have seen that happen several times both ways, that is Europeans misjudging the importance or standing of individuals in this country and we on our side, misjudging the standing of someone of the Europeans, with rather unfortunate repercussions. So it is very important to be sure that you are picking out representative people in institutions that would not arouse antagonism in the majority of the scientists in that country.

DELETED

Well, now they do have an Atomic Energy Commission and the second man on the Commission is the one who is coming over this month to make the decision on the reactor, but I think it would be safe in that case to deal with him.

MR. HENDERSON: In that connection I think your group and the Division of Biology and Medicine and all the other technical people that have contacts with scientists overseas, why they can keep us informed just on who to

[redacted]

[redacted]

contact and what kind of a fellow he is. This could be extremely useful when one is going to Latin America and visiting several countries.

DR. FAILLA: Especially there, because the fellow who is the head today, may be there because he is a friend of the President and the President may be somebody else there next week and he would be out.

MR. SPIEGEL: In connection with the Asia Nuclear Center, the Japanese sent us a very nice note asking us to be located in Japan and the last paragraph, just as an added thought, they expressed an interest in receiving a medical reactor at Hiroshima.

Whether they were thinking that that was part of the AsiaNuclear Center, I don't think that was in the way the note was worded and he would also like to have this medical reactor tacked on there.

DR. FAILLA: I think in choosing institutions or individuals, the general practice should be to let the people in the country themselves, the scientists in the country decide what should be the first

[redacted]

[redacted]

laboratory or the number of laboratories that should be helped and where they are located and so on because then you see, you are not making the choice yourself and if things don't work out, you are not responsible. You have followed the advice of the scientists in the country.

MR. HENDERSON: On the other hand, too, and one of the primary reasons we are trying to develop this integrative approach that we have in the past is that I understand after we have gone through our training courses and so forth and when they get back to the country there is no equipment that they can use so there we have wasted their time and wasted our time and by trying to pull them altogether again, leaving the discretion up to the country and we are here purely on a consultant basis by being sure that if they do want this equipment that they can't finance it themselves, we tried to help them.

Again, I'm not one for giving them everything either.

DR. FAILLA: Sometimes a fellow comes to study here and he is not the best individual or the most popular individual

[redacted]

[redacted]

in the country. He happens to have either money himself or political connections that would give him a fellowship or something of that sort and therefore we have a difficult situation to make a choice on that basis too.

DELETED

[redacted]

[redacted]

[redacted]

DELETED

DR. FAILLA: Thank you very much.

MR. HENDERSON: Thank you, sir.

DR. FAILLA: Doctor Warren?

DR. WARREN: The great concern of the scientific and lay people in different parts of the world is over the changing environment and later to the proposal by Mr. Lodge in the UN that there be established a scientific group to gather such information as existed and make it available.

That was passed by the UN and there

[redacted]

[redacted]

[redacted]

are 15 countries who will have each one member of this group.

I am fortunate in having Austin Brues and Mr. Eisenbud and Chuck Dunham unofficially as reinforcements. Mr. Spiegel and Mr. Henderson are keeping the paths clear from the International UN relationships and it is proposed that the first meeting of this group, chiefly for organizational purposes will be held in the latter part of March.

The nations as I recall them are Argentina, Australia, Belgium, Brazil, Czechoslovakia, Canada, Italy, Egypt. Then there is the UK, the USSR, ourselves.

MR. SPIEGEL: France, Switzerland and India, but not Italy.

DR. WARREN: Thank you. The secretary will be a competent man. There will be a small staff for the committee. It will not meet in continuous session but intermittently.

The Chairman will be in rotation from each country alphabetically in the English language.

[redacted]

[redacted]

The chief task of the committee is to gather the information available, largely in the country of the committee member and see that it is made available to the UN and through that to the world.

This is a very appropriate time for such a thing to be done because the National Academy - Rockefeller Foundation Committee- in which you had a hand, John - is going to have its report ready in all probability in early April.

The Medical Research Council in the UK has been studying the problem independently but I keep in touch with what is going on and if all goes well, their report will be out in April so that if the organizational meeting of this committee is set up, it will not only have the vast amount of raw data, the United States will be able to contribute largely from the standpoint of a tremendous amount of work.

You saw yesterday Mr. Eisenbud's study but in addition there will be the pretty complete appraisal of the value to put on the scientific work done to date in the biologic effects field both in this

[redacted]

[redacted]

country and in the UK.

I am therefore rather hopeful that with these reports to serve as a skeleton for the final report, we will get something quite worthwhile.

I would like to ask Mr. Eisenbud if there's anything he would care to add.

MR. EISENBUD: The thing that concerns me a little bit, not so far as the end product is concerned because I think the end product is going to be good, I'm afraid that each of these countries is going to come prepared with data that they are going to want to present and I can see us getting engaged in a long period of evaluating individual reports of each of these countries and perhaps some of the countries that aren't represented.

I am very familiar with what the Japanese have and in the importance that the Japanese attach to it and they are going to want to have the prestige of being able to present their own data through the medium of this committee to the world.

We know that the Burmese have been

[redacted]

[redacted]

collecting data. We have from our laboratory correspondence with many investigators and many countries and presumably all of these data are being gathered together by representatives and I can see a great deal of paper work and a great deal of time spend and I don't know how we can get around that but I think it is something that we simply have to expect.

DR. WARREN: One of the things we hope to do early is to establish general standards and criteria for measurement and evaluation of material and I think before we are through some type of interchange of standards for calibration of instruments and so on will very definitely be indicated also.

DR. FAILLA: That will take a long time.

DR. WARREN: This is, I expect, going to be something that will continue for a pretty fair period of time. Mr. Spiegel, would you care to add to that?

MR. SPIEGEL: I don't have anything to add. As I mentioned last night, as we talked over this, I think it would be very

[redacted]

[redacted]

helpful to invite the Japanese to collaborate with us on the distance during Redwing - have them out there patrolling the sea and exchange data with them because they are going to be out there doing it anyway and you might as well.

DR. DUNHAM: Well, they are quite efficient and so there isn't any question on that; they'll be in on it.

MR. SPIEGEL: Perhaps doing that it will help ward off some of the compensations.

Speaking on behalf of the Department, we attach a great deal of importance to the radiation resolution and we were confident that if we had introduced it in the United Nations, that is if we had not the Swedes were going to and of course did introduce a resolution.

They were supporting the United Nations complete cessation of nuclear testing pending a complete report and surprisingly enough, 16 nations voted for that and about 5 abstained and the Soviet Union supported the cessation too.

[redacted]

[redacted]

Of course there were the usual political trivialities and Chuck was there too. I think we had a resolution that was a good one. There is limited evaluation in it; scientific evaluation not political evaluation which was what we were afraid of might happen.

Can you think of anything to add?

DR. DUNHAM: No, I don't think so other than to say that we were successful through Mr. Wadsworth and the UN mission in having it clearly called a scientific committee.

The Indians wished to have it called a committee. But they were overruled so as to be sure that the thing was as scientific as it could be with a minimum of political overtones and so stated, I think it put a very great factor in the hands of responsible people like Doctor Warren representing many other countries to stand up and indicate they would have no more to do with if it became political.

16 It gives them a real basis for standing aside.

MR. SPIEGEL: We will talk about

[redacted]

[redacted]

the principles of the committee and before the committee meets we started out with a committee of seven and ended up with a committee of fifteen largely because there had to be Spanish speaking scientists on there and others.

DR. GLASS: When will the first session begin?

MR. SPIEGEL: The end of March. We though at first it would be in January, but the British and Canadians didn't want it; we couldn't have it in February and the Secretary-General was out of the country and so it would be at the end of March.

DR. FAILLA: Would that be just before the test?

DR. WARREN: That will be just before May, that is our test.

MR. SPIEGEL: The British would be in April; isn't it?

DR. DUNHAM: I think that's a little earlier than ours.

MR. SPIEGEL: The British test

[redacted]

would be just going on then.

DR. FAILLA: Any other comments?

DR. WARREN: I think we will need to come back for advice and help to all of you from time to time on various aspects of this.

While there are a relatively small group immediately concerned, we are simply the spokesman for all of you and will heed your help on a number of the points that will undoubtedly arise.

MR. EISENBUD: I might point out, I believe there's a provision in the resolution for consultants.

DR. WARREN: Yes, that is it.

MR. SPIEGEL: As needed.

MR. EISENBUD: Yes, as needed.

DR. FAILLA: Any other business to come up before the Committee?

DR. DUNHAM: First I would like to call your attention to Doctor Failla's letter to the chairman; I think you've all received copies of it and particularly, I wish to direct your attention to the paragraph which concerns FCDA.

This paragraph was composed by the Division after I had presented to the Commission at the time of the last General Manager's monthly report - the feeling of this committee and the feeling of the Division with respect to the problem of giving Civil Defense greater help.

The Commission was very favorably inclined to do more about it and has indicated that we will be able to stand up and do a better job and so this paragraph here, which as I say was written with that in mind will not come as a surprise to the Commission, but will merely emphasize officially rather than through me, as spokesman, your feelings on the matter.

And particularly the statement that it recommends to the Commission the importance that this matter be recognized by the Commission and that such steps be taken as may be necessary to provide additional staff for this purpose.

[redacted]

You also have in your folder, over the General Manager's signature, to Mr. Pike, with reference to Brookhaven. I don't think there need be any discussion of it at this time, but I would hope that you would read it when you do have the time.

It was, of course, largely composed by Lee Hayward to whom I made available a copy of Mr. Pike's letter to the General Manager. I think it's rather a nice statement of Brookhaven's concept of its own mission and it points out in the last paragraph that they are willing to consider doing a little more in the training and educational field for non-research teachers.

DR. WARREN: This impressed me as a very sound statement or what Brookhaven is doing, the way in which it is fulfilling its responsibilities as a regional laboratory and I think that Mr. Pike has performed a real service in pointing out that the AOI and the immediately associated universities are not only the ones that were looked to, to operate and cooperate with Brookhaven, but also as essentially trustees to see that the interests of the smaller colleges and universities throughout the area are also weighed heavily.

[redacted]

[redacted]

As is evidenced here in the letter, quite a little progress has been made in that but I have felt that there is an extraordinary contrast between the very effective liaison that ORENS has with all of the schools in the southeast and the extraordinarily inadequate contact that AOI has with the other schools.

I happened to be talking with a man on the faculty at Brown, for example who had had personal contact but he felt that it was much more a personal contact than a Brown University contact that was represented there. And I think some of the smaller schools have had the feeling that Brookhaven was entirely out of their league and had done nothing to orient either their faculty or their students with regard to the opportunities at Brookhaven.

DR. GLASS: I'm glad you made those remarks. When I was on the Advisory Committee for the Biology Department at Brookhaven, we raised the same question, drawing attention to the rather distinct contrast between the Biology Department at Brookhaven and its relation to neighboring colleges and universities and that at Oak Ridge. And, while recognition seemed to be made of the problem, nothing ever seemed to be done about it to

[redacted]

[redacted]

really remedy the situation.

Their summer program is a step in the right direction and is very commendable but I think it's primarily a matter of the personality and the vision of the problem on the part of the Directors.

A great deal of credit goes to Alex Hollander for getting out and making these contacts himself with the other colleges and universities in the area and there isn't anybody at Brookhaven that I know who does that.

DR. DUNHAM: Of course, one of the things is that ORENS itself has no other mission than this sort of thing.

Sometimes they work at cross purposes but generally they come through ORENS to ORENS, through him back and forth and Brookhaven has no such mechanism; maybe they should develop something of that sort either at the AUI level or at the Brookhaven level itself.

DR. WARREN: I should think AUI would be and while we are making tremendous efforts to reach people in Thailand and in Ceylon, let's not forget Maine and New

[redacted]

[redacted]

Hampshire. (Laughter)

DR. FAILLA: Any other item?

DR. DUNHAM: No, I have no other items to call your attention to.

DR. FAILLA: Any other items to come up?

DR. DUNHAM: I might mention, you recall, we were at one time going to have this meeting in Idaho and were advised by the Division that it might not be possible because of certain activities there.

I talked to Lou Rogers Thursday, and he assures me that the place will be open to the Committee for the next meeting if they decide to go there. The date would normally be the 9th and 10th of March.

As you recall the reason it was postponed was that the ANP would not have been available for visiting if we had time.

DR. FAILLA: Is that satisfactory to everybody?

[redacted]

[redacted]

DR. DOISY: What are the dates?

DR. FAILLA: 9th and 10th.

DR. GLASS: How long does it take?

DR. DUNHAM: It depends on whether Western Airlines are still on strike; they were on strike yesterday and then you have to go by train if you can't go by plane, which means that you have to go to Chicago and then take the Portland streamliner to the City of Portland and they will meet you then at Pocatello.

You can fly to Salt Lake City overnight from the East and take an early morning flight to Idaho Falls proper.

One item I forgot and that is that we do have a letter here from the Chairman thanking us for my note of the 5th, inviting him to this meeting and also inviting him to suggest items for the agenda.

He indicated that he had no items to add and he wished it were possible for him to attend.

And Chuck, as you already know had to

[redacted]

[redacted]

decline at the last minute and couldn't be here.

DR. FAILLA: Well, in that case, this session is adjourned and we will have an executive session.

(Whereupon the meeting was adjourned.)

[redacted]