International Atomic Energy Agency

IWGFR/6

INTERNATIONAL WORKING GROUP ON FAST REACTORS

EIGHTH ANNUAL MEETING

Vienna, Austria

15-18 April 1975

SUMMARY REPORT

Part III

Reproduced by the IAEA in Austria
January 1976

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FOREWORD

The Eighth Annual Meeting of the IAEA International Working Group on Fast Reactors was held at the IAEA Headquarters in Vienna, Austria, from 15 to 18 April 1975.

The Summary Report (Part I) contains the Minutes of the Meeting.

The Summary Report (Part II) contains the papers which review the national programmes in the field of LMFBR's and other presentations at the Meeting.

The Summary Report (Part III) contains the discussions on the review of the national programmes.

Contents

		Page
1.	Discussion of Prof. Pierantoni's Presentation	1
2.	Discussion of Dr. Smith's Presentation	2
3.	Discussion of Dr. Engelmann's Presentation	4
4.	Discussion of Mr. Vautrey's Presentation	6
5.	Discussion of Dr. Wensch's Presentation	9
6.	Discussion of Mr. Allgeier's Presentation	14
7.	Discussion of Dr. Krasnojarov's Presentation	14
8.	Discussion of Dr. Tomabechi's Presentation	17

1. Discussion of Prof. Pierantoni's Presentation

Dr. TOMABECHI: It has been the practice of this working group to mention the approximate figures of the number of the personnel and also the figures of the budget of each Member State and I would like to ask a question. What would be those figures for the Italian fast breeder programme for 1975?.

Prof. PIERANTONI: For the 1975 budget we have something of the order of 36 billion lire and something of the order of 400 technical and scientific staff with no support staff included, not including people working for the fuel manufacturing line. Because the CNEN now is organized in five different large departments, one for fast reactor, one for thermal reactor, the third for the fuel cycle, the fourth for biological safety environmental research and the fifth for the advance of technology research like fussion and basic technological research, and all the activity related to the fuel manufacturing are carried on in a department which is not the fast reactor department but the fuel cycle department. But the reorganization of CNEN was approved just ten days ago, - it took us about seven months of discussion.

Dr. DÄUNERT: Prof. Pierantoni, I would like to ask you if you can give us a feeling about the proposed time schedule for the construction or finishing the construction of your PEC reactor.

Prof. PIERANTONI: In November 1974 we agreed with the CEA to have planning for the PEC reactor by which the facility will be in operation during 1979. Now, we don't know how the six month delay, due to the reorganization of CNEN, will affect the time schedule and I think we may probably have something from a six month to twelve month delay.

Dr. WENSCH: My question is relevant to Dr. Däunert's but slanted somewhat differently. Do you have government authorization now to proceed with the PEC from beginning to end in terms of the funds?.

Prof. PIERANTONI: As I said here last year, the Inter Ministry Economic Planning Committee is approving a programme and they give their decision to do, for example, given work in a given area. For example, this year the Minister of Industry, who is the man in charge to give the first approval to the CNEN budget, approved without modification, the request made by CNEN managing committee of allocating money and, I think, the total request approved by the Minister of Industry is 132 million lire compared with about 70 billion lire of the 1974 budget. But, while the situation in Italy may be quite fine from the point of view of money, as you know, the energy crisis is probably pushing the Government to make very large reorganization of all the body involved in energy production, and I am not referring to CNEN only but also to ENE and ENEL. So probably we will be facing some other delay due to the fact that the Government is now thinking of very large restoration of all the energy sector,which is now divided in at least 3 or 4 bodies,and ENE is referring to the Minister of Government participation while ENEL and CNEN are referring to the Minister of Industry,so the bodies working on energy field are referring to different ministers.

Dr. KRASNOJAROV: I have heard that the few control rods have been reconstructed. Is this correct?. Was it only due to release of helium or was it for the sodium flow?. Could you give the motives for this?. And if not, what was the reason to reconstruct them for venting type?

Prof. PIERANTONI: This was due to helium production in control rods.

Dr. SMITH: I would like to ask a little about the trial of the full sized pump rotor. What exactly are these tests to be?. What accidents are you simulating - Is it just the seismic condition?.

Prof. PIERANTONI: Just the seismic condition and we are planning to see what will happen during a seismic and what happens to the rotor after the given size of the earthquake.

Dr. SMITH: By the rotor do you include the shaft?.

Prof. PIERANTONI: Yes, we include the shaft, the shaft without any sodium flow.

Dr. SMITH: Are you looking for vibrations?.

Prof. PIERANTONI: Yes, we are looking for the vibration problem. And we are simulating an earthquake of .3G with quite complex spectrum, and .3G is the maximum intensity.

Dr. KRASNOJAROV: I would like to continue this question. Are these experiments being carried out from the point of view of the capability for work of the pump after the earthquake or do you see in this particular question problems for the reactor's safety?. Would you like to clarify this for me?.

Prof. PIERANTONI: In Italy we cannot avoid having a large power station designed for earthquake conditions because we may have quite strong earthquakes everywhere in Italy. The boiling water plants built in Italy are designed for .3G, I think, and this limit will put some condition to all large power stations built in Italy and I also think that in other European countries, due to this reason, we are putting some effort in order to see what happens to a component like the pump shaft during and after an earthquake.

Dr. KRASNOJAROV: Would you like to explain?. You are speaking about earthquakes. Do you think that during earthquakes the reactor will continue to work or not?.

Prof. PIERANTONI: We are now putting a lot of control devices in order to shut down the reactor during an earthquake and also we are looking for the possibility to shut down the reactor before the earthquake, if it is possible, but we must see what happens to the long shaft of the pump after an earthquake.

Dr. WENSCH: May I make a comment on this potential problem - it is a real problem in our view. In the United States all our plants have seismic scram switches and I presume that this is the situation throughout most of the world. In the event of an earthquake the plant will be scrammed. During the coast-down of the pumps you will lose some pressure in your hydrostatic bearings. If the pump journal moves back and forth and then touches other bearing surfaces, you can do a great deal of damage to the pump and we have seen this happen, not under earthquake conditions but under conditions in which a hydraulic instability permitted the journal to move around and touch the bearing.

Dr. TOMABECHI: I would like to continue this discussion. Perhaps I have not understood correctly what Prof. Pierantoni has said. Did you say that you are not going to use the sodium for this test?. Are you going to shake whole of the loop?.

Prof. PIERANTONI: We are going to use the sodium, and shake part of the loop. We had a lot of trouble finding a place in Bresimona because we should avoid destroying the mountian when we save the pump.

Dr. TOMABECHI: And what is the capacity of the facility for this test?.

Prof. PIERANTONI: I don't know how the facility was designed because I didn't follow personally the facility but it is really designed. If you wish I can send you some information on the facility profile. We now have a new organization in charge of preparing such work but I will send you the information as soon as possible. Everything will be quite clearly explained. I don't know exactly what method they use but I think it is a mechanical one. Mr. Vautrey, do you know exactly?. I think the mechanical method is the only one they have.

Mr. VAUTREY: I don't know exactly the details of the method by which seismic shocks are simulated but it is by some mechanical method, I believe. I should like to add, more generally speaking, with regard to this experiment and to seismic conditions, that the aim of these tests is to check all the revolving parts of the pump, the shaft in its real size with the rotor functions in conformity with the design of the project. With regard to sodium, as Prof. Pierantoni has said, there is no circulation of sodium. It is the shaft that is tested and that corresponding to the rotor or the pump, and a mass equivalent to the rotor of the pump is used so there is no real outflow in the test, but the shaft and the rotors are fed with sodium, as usual, and in these seismic tests the idea is to check that things do happen the way they were conceived in the design. And all of the Phenix and Super Phenix installation pumps and cooling systems are meant to resist earthquakes, and the aim of the tests is merely to see that they in fact do so. That doesn't mean, of course, that in the case of an earthquake you shouldn't scram it or shut down the reactor.

2. Discussion of Dr. Smith's Presentation

Mr. POLLIART: I would just like to ask you - on Page 3 you mentioned in your report, Dr. Smith, about the experience of separation, including recovery of a damaged sub-assembly. Could you tell us what was the reason for it?.

Dr. SMITH: As I remember the story, this was done at a fairly early stage of commissioning of the reactor when the interlock system was not fully in operation. Somebody managed to load a fuel element into the hole which already contained a fuel element and damaged the handling arrangement at the top of the subassembly. This had to be recovered using a special device.

Dr. WENSCH: My question goes to the steam generator problem on page 3 of Dr. Smith's report. Is there any way you can relate the leaks to manufacturing and inspection of tubes?.

Dr. SMITH: The only steam generator which is accessible at the moment for examination is the evaporator. The evaporator head was removed and the tube bundle taken right out and the weld was examined. I believe this was a welding defect of some sort and there is no systematic corrosion or anything of this sort in the evaporator. I believe it is a straightforward weld defect which was not detected during construction or test.

Dr. WENSCH: My question was did you have X-ray radiographs made of the tubes during fabrication and can you not now look at the records and see whether they looked suspicious then or was the leak completely unexpected?.

Dr. SMITH: I think it is an unexpected leak. All welds were radiographed, and I am sure this particular radiograph has been looked at and as far as I am aware no leak was visible on the radiograph. Not only were they radiographed - the units were helium-leak tested in situ before they were sodium-filled and it must be a considerable worry that despite these testing methods leaks have occurred. I should add I think the helium test was not carried out at the full steam pressure and it may be necessary to do that, I believe. Even then, of course, you have the problem of temperature.

Mr. VAUTREY: Could you tell us please - give us an idea - of the present situation with regard to these steam generator leaks and repairs already done?.

Dr. SMITH: There are 3 steam generators - one of these has a leak in it. The bundle was taken out into the reactor hall because of some of the tests that were done to locate the leak by pressurizing sodium which had forced some sodium through the leak and there was some sodium in the steam side of the tubes. This had to be removed by washing and of course the tubes had then to be inspected for any damage that might have occurred as a result of the sodium getting inside the tubes. The intention would be to plug any tubes which show any signs of damage. Apart from the original leaking tube which has been plugged, I understand all the tubes are believed to be in a first class order. There are some with some marks on them which are probably only colouration and there is discussion going on as to whether or not to plug a few of these as a safety measure. But, I think in fact that the evaporator is almost at the point of being put back into its container and reused. As I said, the condition on the super heaters is more difficult because, whereas in the evaporator you can lift the top and inspect the whole tube plate, on the super heaters, there are annular rings of a semi-circular cross section.

On this semi-circular section there are some little blind tubes that project. These can be cut open and access to the tube plate is down these tubes and round a corner unless you are lucky and the tube you want to look at is directly underneath. This makes inspection very difficult on the super-heaters and also on the re-heaters. So at the moment at Dounreay they are examining the tube plate via these access holes. They have decided also to raise one bundle out of one of the super heaters to investigate the condition of the tube to tube plate welds. They have also cut out a specimen by remote control of the weld which, we believe, was the prime leak in one of the two super heaters and this will be inspected metallurgically to try to discover how this leak has occurred. There may be more information available in Paris - these investigations are going on at this point in time.

Dr. WENSCH: Coming back to the leaks in the super heater, can you tell us how many leaks have been found because it says in your report small leaks have been detected in two or three super heaters - does this mean one leak in each one or several leaks in each one?

Dr. SMITH: The leaks were originally small and the leaks enlarged with time. Under closer examination some small leaks were found in adjacent tubes so there could be some suggestion of a spreading of the leak to adjacent tubes. But, I think, if you would take what I say as being very preliminary since it is not at all clear yet what the situation on the super heaters is. The original intention was to plug the tubes and any surrounding tubes. But they now feel that they would really like to make quite sure exactly what happened before they do this.

Mr. VAUTREY: Do you think that these leaks in the steam generators are due to errors in construction that could have been avoided by a more efficient control of the construction phase or what conclusions do you draw from the appearance of these leaks?. Do you think that it is something inevitable in such an installation or do you feel that it is rather a result of poorly made equipment?.

Dr. SMITH: Well, at this stage I don't think we are drawing the conclusions from what has happened in the indications. All these units were carefully inspected and they were carefully tested both radiographically and by helium testing. Nevertheless these leaks have occurred.

In the case of the super heater it is apparent that the leaks did in fact enlarge before the super heater was taken off line when we had the problem of determining exactly what form the original leak took. So the implications of this are clear that either the testing we have done isn't good enough, or steam generators have to be designed to accept leaks of this sort, or in some way the manufacturing technique is to be improved so that they do not occur. I think that what I am saying is really speculation at this stage. We don't know the original cause of the super-heater leaks, - we don't know how badly the super heaters are affected. The steam generator situation is, perhaps, slightly more encouraging and since that there were not so many leaks and no consequent damage, but I think our experience has done nothing to allay the fears that one has always had with the reliability of these steam generators. I think we have to do a little more investigation before we can decide what the situation really is.

Dr. ENGELMANN: When do you expect the super heater leaks can be repaired and when do you expect the PFR will go to full power?.

Dr. SMITH: Hopefully, it is only a matter of plugging these tubes, and we have in fact already plugged some of them. The tubes are plugged by an explosive welding technique which seems to work very satisfactorily. Assuming that the inspection of the super heater does not show anything unpleasant, even to take the bundle out does mean cutting some fairly large pipes and this will take something, I believe, of the order of 2 months. So it will be 6 weeks or 2 months before one could get up to power. At that time there should be nothing stopping the reactor going up to full power as I say, unless some fresh or worse difficulty is found.

Dr. ENGELMANN: This would mean in early summer?.

Dr. SMITH: Yes, early summer.

Dr. ENGELMANN: I have another question, not on this steam generator - maybe there are other questions on steam generator problems?.

Dr. KRASNOJAROV: Is it your intention to continue with large steam generators or do you think that it will be more useful to have a small modular type in future?.

Dr. SMITH: At present, we envisage continuing with large steam generators. There are eight sets proposed for the current 1200 MWe CFR design so they are something like the same size as the PFR generators. I think as a result of our experience we would want to pay a lot of attention to accessibility of tube bundles for repair but I don't think we would consider at the moment going to small modular units because they are more expensive and you get into a very great degree of complexity over all the instruments you require for such a large number of units.

Dr. KRASNOJAROV: But, to have such a reactor not in operation costs money too - maybe it would be better to spend money on more instrumentation.

Dr. SMITH: I think it is not only the cost of the instrumentation - it is also the reliability of very many circuits and I feel that one would only go to a very large number of modules if there was no alternative. Perhaps I should also ask the USSR who also has had some unfortunate experience with steam generators, whether you think you continue with large units or small modular units - What is your view on this?.

Dr. KRASNOJAROV: Some people in our country think about large steam generators and others think about modular type. For example, we used Czechoslovakian modular type for BOR-60 and we are very satisfied and now we have discussions with Czechoslovakia about installation of at least one Czechoslovakian modular type for BN-350 and BN-350 has again a large steam generator. Maybe a modular type of steam generator will be installed for BN-600 too. I am not sure what the future will be. The reliability of the whole installation would be better when you could cut one section and continue operation with others. You could lose some percent of power but continue operation and have time to repair or to extract.

Dr. DAUNERT: Maybe you remember in the course of detailing the SNR-300 we changed the design of the intermedia heat exchanger to modular design. This means instead of 1 intermediate heat exchanger we have now 3 modulars. And I want to give you a feeling what Mr. Krasnojarov mentioned that you have to spend more. For the construction of the special case of SNR-300 we spent an amount of 30 million DM for this change. On the other hand, if you have the loss of power from 300 to 1000 MWe you have to spend about 100 million per month or so on the losses of producing electricity.

Dr. ENGELMANN: I would switch now to another subject if this discussion is over. On page 6 of your report you mentioned that you have achieved about 16% peak burnup without pin failure even with materials like Nimonic PE 16. Now, to my knowledge this material is a very low ductility material so I am very much surprised that this would withstand such high burnup. What is the reason?. Do you have a very low density of fuel?.

Dr. SMITH: The density of fuel?. I'm not sure, I think it would be the standard 80% although some of the fuel irradiated is only 70% dense. This is of course only 1 pin in a trefoil.

Dr. ENGELMANN: But you agree that you would not expect from a calculation that the pin would withstand the pressure of the fuel?

Dr. SMITH: I'm not sure that I could answer that question directly because I haven't sufficient data about this particular experiment at my fingertips. Putting it another way, I don't think one is enormously surprised that one has achieved it at least with 1 pin, even a PE 16. The general situation is that, though we have had quite a large number of failures, most of these failures have been due to some defect or other, that we have been able to identify, and we believe that very few of the failures actually are complete end of life failures. If everything was perfect you would nevertheless expect any pin to fail eventually and as it said in the next paragraph there were these 2 pins out of the 77 pins or so in a sub-assembly which had failed. It looked as if one could find no reasonable excuse except that they had just come to the end of their life. There is even some question that there might have been something odd about these 2 pins. The implication of this is that if you can make all your pins perfectly you will get 10% burnup.

Dr. ENGELMANN: Well, we thought loss of ductility would be one of the limiting factors and I would expect with nimonic PE 16 this would occur earlier in the burnup life than with the other materials. So, if even with this material one can achieve 16% burnup maybe you might never reach the burnup limit on other materials.

Dr. SMITH: But, I think you need to know the fuller details.

Dr. ENGELMANN: Yes, the temperatures and so on. I have another question about your physics experiments. Did you get any indication on the tendency for breeding ratio?. Is it still decreasing due to nuclear data changes or is it coming up again?

Dr. SMITH: I think recently it came up a little. It certainly has not decreased any further. Using calculations for CFR it depends what you assume about the composition of the plutonium that you were using. SGHWR I think produce quite good plutonium from the point of view of breeding. The breeding varies quite a lot with atomic composition. We have certainly got no worse since last year in either our doubling time or our breeding gain.

Dr. KRASNOJAROV: My question concerns fuel loading system of CFR, the discharge of spent fuel. Do you suppose to do it in a gas atmosphere or are you going to have gas cooling and to use a special container in sodium?. After what time of reactor shut-down do you use gas atmosphere?.

Dr. SMITH: The PFR refuelling system is exactly the same as it was before. The sub-assembly is lifted at the core, under sodium, it is transported into the rotor under sodium and it stays under sodium for some period of time - between 10 and 20 days. It is then in a cylindrical can which is filled with sodium and it is pulled out in this can. I don't quite know where the gas-cooling is referred to. Bottom of page 4 refers to the cooling of the can. This is the CFR design. It would be a similar arrangement but you have to remove the heat from the can when you take it out.

Dr. KRASNOJAROV: Not through the sub-assembly?

Dr. SMITH: The sub-assembly is in a pot with sodium and you have to cool the pot. The present CFR design does not have an in-core store. Therefore, you require to handle sub-assemblies with about 50 kW of heat, per sub-assembly.

Dr. WENSCH: That's a lot of heat, 50 kW, to remove - our designs stay below 20.

Dr. SMITH: Yes, that is the present concept. It is fairly easy to remove 50 kW of heat. The problem arises in the accident condition if the cooling fails.

Dr. TOMABECHI: On page 4 you have mentioned that the alternative system is operated by sodium pressure from beneath the core in the design of CFR. This means that the control rod will be operated hydrodynamically with sodium?.

Dr. SMITH: Well, it is held by a sodium pressure. It can be pulled up into position.

Dr. TOMABECHI: And at the scram condition you just lose the pressure and it falls down by gravity.

Dr. SMITH: Yes, that's right.

3. Discussion of Dr. Engelmann's Presentation

Dr. WENSCH: On the slide which you showed indicated the construction of the SNR 300 indicated some milestones. It appeared to me that you started construction before you had licensing authority. Is this right?

Dr. ENGELMANN: Are you referring to SNR 2 or SNR 300?.

Dr. WENSCH: It was SNR 300.

Dr. ENGELMANN: We would not be allowed to start anything before completion of the licensing. And certainly we will not start construction of SNR 2 before SNR 300 is in full power operation.

Dr. SMITH: Could I ask on your last paragraph on plutonium losses and cooling time?. I would very much support what you say here. This is a thing we have also decided is very important. What sort of recycle time do you consider to be practical and what are the most difficult limiting features in this cycle?.

Dr. ENGELMANN: In our study we have assumed, after enquiring from the re-processing and transport people and the designers and operators, a two year out of pile period until the year 2000 and 1.2 years after that time. The largest time span out of this 1.2 years is about 1 year for cooling. We have assumed 3% losses of plutonium until the year 2000 and we also assume that we can bring this to 1.5% after that time. These are not losses which go into the open air of course but represent Pu which cannot be fed back into the cycle. The influence of these losses and the out-of-pile time is tremendous if you look at the fuel conservation and the speed of installing fast reactors. I think to reduce the out-of-pile time and to reduce the losses is about equivalent to going from oxide to carbide fuel. If you also consider the environmental aspects there are strong incentives to reduce out-of-pile plutonium, that means out-of-pile time, because the longer the out-of-pile time is the more plutonium we have outside reactors. So you have real reason to look a little bit more into the chemistry.

Dr. SMITH: In some ways you are more optimistic and in some ways less optimistic than us. We tend to assume 4% plutonium losses reducing to 2% in the longer term, although I think these are essentially estimates probably on both our parts. Our recycling time, however, we hope ultimately to reduce to three-quarters of a year or 9 months rather than 1.2 years. And looking at the recycling, some of the key issues seem to be the transport problem and the number of sub-assemblies you can take per flask, and in the reprocessing plant , the iodine problem. If you attempt to do the reprocessing before 150 days you get a very large iodine content which must be regarded as a hazard in the event of an accident in the reprocessing plant. I don't know if other countries would like to comment on recycling time and re-processing losses.

Dr. WENSCH: I think most of us recognized these problems for many years but only recently have the problems really come home. For example, light-water reactor industrial reprocessing has turned out to be difficult on a commercial basis. One can do it on a pilot plant basis but on a commercial one it has not been achieved. In recognition of what must be done in a light-water reactor programme, as well as in this liquid metal fast breeder reactor programme, ERDA is now approaching this in two ways. We are going to assist the light-water reactor programme by providing support for the reprocessing of the light-water reactor fuel, and when it comes to LMFBR fuels we are reorganizing and setting up a stronger organization to tackle these problems more directly under people who are truly responsible. Now I am only speaking about the mixed oxides. Certainly when one goes to the carbides we are faced with the difficulties of chemistry and one must then consider burning the carbides or going to some other means. When it comes to cycle times we have had sensitivity analysis performed and we have shown a range of values but I believe that for LMFBRs a cooling off time of 150 days seems to be reasonable, and from losses in conventional laboratory scale systems one can hold the loss to about 1%. In the larger commercial operations this has yet to be demonstrated.

Dr. KRASNOJAROV: 150 days for what do you mean?.

Dr. WENSCH: 150 days cooling off time before the fuel goes into the head end of the reprocessing plant.

Dr. SMITH: Mr. Vautrey, would you like to comment on this problem as far as the French programme is concerned?.

Mr. VAUTREY: Yes, I should merely like to add what I feel, without being too sure of what the situation is with regard to these matters in France. I think that the aim is also to achieve only one per cent losses, but I feel that perhaps the more valid figure would be about 2 or 3% at the present state of affairs. Of course, cooling time should be as short as possible. This is one of the aims to improve the economic value of fast breeder reactors of this generation. I don't have any figures to give you all the details on this matter, however.

Dr. SMITH: Dr. Krasnojarov, would you like to comment?.

Dr. KRASNOJAROV: Chemists look upon the process of reprocessing when there are good enough results and this means that the first few primary fuels must not be expected in this way, and be enough for economic efficiency. Then for the first core it is necessary to wait. But I suppose the first fuel will be reprocessed at least after one or two years of heat decay to do it cheaper. We consider that it will be possible to have less than one year's time and losses no more than 1% in future, and experiments on laboratory scale show that it is possible. Apart from this it is possible to have nonaquoid process where it is possible to decrease the time even more. But in this case there could be a transportation problem. Perhaps in this case the decision would be to have a small reprocessing plant near the reactor. We have a lot of different experiments now and no decision about future reprocessing.

Dr. ENGELMANN: I think one or two years ago I would also have said that we are aiming at cooling times smaller than one year and plutonium losses lower than 2%. It seems with the bad experience in the light-water reactor reprocessing our chemists are a little bit hesitant to claim low cooling times and smaller plutonium losses. Perhaps after they have learned a little bit and have regained a little bit of their self consciousness they will set their goals again a little bit higher. At the moment they say we have to face 3% losses and in the beginning at least we need for the whole external cycle in the order of two years.

Dr. SMITH: Yes, I think also in the UK there is this pessimism which results from a rather closer look at the problem and certainly our pessimism seems to lie more in the field of plutonium losses, economically recovering plutonium than in the cycle time, though I agree there are difficulties in even getting down to a year at the cycle time. It certainly wouldn't be done in the early stages of a fast reactor programme because it

would not be necessary. Mr. Pierantoni, is there anything you would like to add on this?

Prof. PIERANTONI: The plutonium losses are not an economic problem only but it is an environmental problem and tons of plutonium will be piling up considering the few percent of losses. Somebody has just calculated if we proceed with a nuclear power capacity, according to our plans, 11 kilogrammes of plutonium per second will be generated in the year 1985. Now calculate a few per cent losses!

Dr. WENSCH: I think perhaps we are discounting the large amount of experience in reprocessing of the nuclear fuels which is being done by the various governments. We know what the losses are based upon - many years of experience and that they can be held to 1%. In the laboratory, by using ion exchange columns, one could hold the losses down to essentially zero but it would not be a practical thing to do. I think what we are striving for is to develop a commercial industry where one must balance off permissible losses against additional costs for efficient operations. So the losses would never become zero because the costs would then become too high.

Dr. SMITH: Well, I think my comments did refer to a commercial operation. It is clear that if you spend enough money you get nearly all the plutonium back. But one of the areas in which we don't have, I think, anywhere much experience is the head end treatment, the first treatment of the dismantling of the sub-assembly and the separation of the fuel from the client or the dissolution of, or whatever you have to do, which can be a very difficult process and could also give rise to considerable waste.

Could I ask another question? You talk on top of page 6 of control of plutonium particle size in the fuel. What aspect of the plutonium particle size is considered to be important?

Dr. ENGELMANN: I cannot give you the exact answer. This is a point which concerns the fuel manufacturer, and the fuel quality depends on the control of particle size in the fuel. I think it is connected to the migration of plutonium and uranium in a temperature gradient. The grain size has some effect on the separation of Pu and U.

Dr. WENSCH: In addition to the fuel, we have made the same observation on the effect of grain size in the swelling behaviour of stainless steels. This group may recall that two years ago in this room I had a model showing the stainless steel swelling - the year before I had a model which was on previous data. We find today that the curve is becoming flatter so that the exposure times to neutron fluences, before embrittlement, is becoming longer. In other words, by careful control of the chemistry of the stainless steel and precisely determined cold working, the swelling is being delayed.

Dr. ENGELMANN: May I ask a question on this? Did anybody else find a difference in swelling, if you have nominally the same material but different charges? We experienced swelling on different charges. The same material in one case showed about twice as much swelling as in another case.

Mr. VAUTREY: Yes, I agree with Dr. Engelmann entirely and I can confirm that we also in France have obtained different results using stainless steels, various steels supposed to be identical which would seem to indicate that in the composition of these steels and their effect on swelling, there are facts which have not yet been completely understood. Now, I should like to add that when we speak of swelling we should of course discern between real swelling due to distortion in the cladding, which is due to void on one hand and to distortion due to stress on the other. I am speaking of the overall global effect measured on cladding, and the parameters which influence these distortions don't as yet appear to be quite clear.

Dr. KRASNOJAROV: Some of our results are such that we have very poor statistics and we try to extrapolate. But different steels have different swellings, and it seems there is no stabilization factor - it is exponential. There are different results between electromicroscope and measurement of density. Electromicroscope results are less than measurements of density. When you measure increasing of the volume by measuring of density, figures are more than from electromicroscope. Such results are somewhat systematic. Sometimes they are very close but as a rule electromicroscope results, after calculation of void, are less. Experts say that there is no problem for 10% burnups. Use of materials with less swelling decrease the problem. But in our case we have the lesser dose than will be in large reactors. Again, some kind of extrapolation.

Dr. SMITH: Certainly in the UK we find the results are variable. In fact, there is a reference on the bottom of page 6 in my presentation to variations of swelling round the diameter of a tube which we have observed and it has been ascribed to variations in hardness around the tube. But the problem seems to be that the swelling is very sensitive to quite small changes in the condition of the steel.

Dr. WENSCH: I would like to participate in this discussion too because we have observed this for many years. Starting five years ago we began to characterize our stainless steel very carefully. It is purchased from one steel maker in sufficiently large lots so that the tubes and claddings can be made from one heat. After fabrication is completed, the steel is analyzed very carefully before it goes into the irradiation rigs. This has enabled us to prepare a standard for cladding which has extended its life-time considerably.

Dr. TOMABECHI: We have only a limited experience of irradiation, but we have also noticed differences in the behaviour of steel. The same kind of steel behaves in a different way - perhaps it is due to a small amount of impurities which are not detailed properly in the specification, or some procedures of making a cladding tube, thus eventually giving us a different behaviour of the cladding tubes.

Dr. SMITH: Could I just pick on one other point out of the presentation. This is at the bottom of your page 7 and it is the question of fuel sodium interactions, and the situation which as you say is supported by observations in several countries, of getting fragmentation of UO_2 with only small pressure pulses. If this can be substantiated, this is of very great importance for fast reactor safety. But from experience in the UK, knowing that for example with metal - water you can get very large explosions, one would need to have a reason which you could sustain which would explain why you don't get this with UO_2 and sodium, and I wonder if anybody has such possible explanations?

Dr. ENGELMANN: Well, you know Fauske's theory which tries to explain the phenomena and to explain why, for instance, after putting some aluminium or whatever in sodium you will get an explosion and with uranium oxide you don't get an explosion. According to our experience you will get a substantial pressure increase only if you inject liquid sodium into molten fuel but not the other way round. But we, of course, have no good theory to explain it except what Fauske's theory proposes.

Dr. SMITH: This would start a long argument but your explanation is that you believe the Fauske's theory.

Dr. ENGELMANN: So far we don't see anything which is not in agreement with Fauske's observations but of course we have to look more into the matter.

4. Discussion of Mr. Vautrey's Presentation

Dr. TOMABECHI: I would like to ask a question. I think that you have mentioned about some sodium leak experiences with Phenix and perhaps I have misunderstood what you have said. I heard that you had some leaks of steam generators. What is the situation now and how did you repair the leaks?.

Mr. VAUTREY: Yes, there were probably some mistakes in the translation with regard to the sodium leaks, so it is necessary that I should come back to this point and make it quite clear. Here is where the sodium enters the reheater and here you have a valve and a leak took place. The leak was here near the weld of the valve in the secondary circuit and not in a steam generator.

Dr. DÄUNERT: Mr. Vautrey, on your Fig. 3, one can find a breeding ratio of 1.12 for the Phenix 250. Can you explain a little bit more to what this breeding ratio is concerned. Is it breeding ratio of a pure plutonium core or is it calculated with adjusted codes from experiments?.

Mr. VAUTREY: I shouln't like to answer that question too precisely because I know that on these questions of breeding ratio specialists have often evolved in their opinions and I myself couldn't give any precise description of the conditions under which the figures given here are estimated. What I can say is that these are the most realistic figures, due consideration being given to the latest results of the most reliable theoretical constant values obtained so far. Of course, experience will have to justify these figures for Phenix, for example. I don't think that anything allows us yet to have an experimental result. This is the forecast result. But, I do feel it is an extremely honest, realistic and good estimate.

Dr. ENGELMANN: Was this a mixed oxide core or the uranium fuelled core?. There was the second part of the question.

Mr. VAUTREY: I am not absolutely certain but I think that the figures expressed here, that some assemblies use plutonium oxide, some assemblies contain enriched uranium oxide - this is a provisional core because we lacked the necessary plutonium at the time when the assemblies were entered into the core. So this is a transitional situation and it is not taken into account when working out these calculations.

Dr. WENSCH: I found this presentation to be very refreshing. I have heard many problems in the fast reactor programme going back to when I entered it in 1953. The French achievement of building Phenix in five years, essentially within the budget, and have it operate almost without any problems at all, represents engineering and management of the highest order. I wish to congratulate my French colleagues. It is a marvellous undertaking and, as time goes on, experience should reveal interesting data that should be useful to all of us.

Dr. KRASNOJAROV: I would like to ask especially about page 14. There, we were speaking about the important elements and the work on the programme of the Super-Phenix. It was translated - the calorifuge of argon. Now, I am not quite sure what this means. And the first and second points are also not clear to me - Could you perhaps give me some explanation?

Mr. VAUTREY: Perhaps a drawing will make this clear. Here you have the tank of the reactor with the pumps. And here you have this top concrete cover to the tank and the argon covering is underneath this top concrete cover. Calorifuge means thermal insulation in English.

Dr. KRASNOJAROV: And you want to improve it?.

Mr. VAUTREY: I think that the reason for these extensive tests is justified because when we conceived and designed the Phenix project we thought of this solution and didn't use it because we felt that our tests were not sufficient. So, in Phenix the tank of the reactor is like this. Here, you have the turning plug, here you have the pumps and the intermediate heat exchanges (IHX) and here you have this concrete cover but the tank here is shaped somewhat like this and the top part of the tank here, which we call the roof in English, gave us some problems with regard to thermal constraints behaviour and that is why we thought of this other solution. But, to be sure that it is the right dimensions, and that it does have all its heat resistent qualities between hot sodium and this cold roof, this insulating material must be carefully tested and the problem of insulating material in the PFR also gave some trouble, I believe.

Dr. SMITH: I mentioned this trouble in my presentation. We have a system like the system you will adopt for Super-Phenix with stainless steel insulation below the roof. It turned out that the cooling provided was not adequate and we have increased the gas cooling on the roof and that system is now in operation. But, also we were concerned about thermal expansions in this roof, possibly eventually leading to some failure and we have, therefore, put in some monitors which enable us to actually measure the movement of the roof. I think I am correct in saying, movement is less than we had feared at one stage. Nevertheless, this is a very difficult design area.

Dr. ENGELMANN: I have a question on this. Is your main concern about the overall homogeneous heating and the expansion of the roof structure or is it a transient heating - local heating?. What is the main problem?.

Dr. SMITH: The main problem in the PFR is the question of the penetrations. It isn't in fact a simple sheet across the top, because there are penetrations which go through this to take the pumps and the heat exchangers where these fix into the sheet.

Dr. ENGELMANN: Differential expansions?.

Dr. SMITH: Yes, you get difficult stress concentrations.

Mr. VAUTREY: I should like to add a few words to what has just been said. The model that we are building at the moment is 6 m. diameter, as I said. It is so big that we can reproduce a certain number of penetrations in the roof in the top. It is not a simple study of the insulating material. Those tests are now finished. Now we are studying the real situation of the top of a reactor, the insulating material and a certain number of penetrations which are necessary to make through it.

Dr. KRASNOJAROV: Not everything is clear. What is the main problem - higher temperature near concrete or not uniform distribution of the temperature due to heat penetration in some places?. Maybe it is radiation penetrating through thermal insulation and heat is produced inside these parts. Then increasing thermal insulation is not the decision to be taken.

Dr. SMITH: I don't believe the trouble is due to gamma radiation heating. Are you thinking of gamma radiation heating?.

Dr. KRASNOJAROV: Yes.

Dr. SMITH: No, that is not the main trouble. It is simple thermal heating. Then the expansion of a rather complicated structure.

Dr. KRASNOJAROV: And you decided to increase cooling and want to increase thermal insulation in this case.

Dr. SMITH: We have no possibility of altering the design of PFR now. We might not do it the same way if we did it again.

Mr. VAUTREY: As far as we are concerned, it isn't so much to increase the thermal insulation but to check by a sufficiently representative test that this thermal insulation is what we have represented as being in the calculations, because you have the concrete roof above the insulating material which is cold, and you have to know this area thermally extremely well. And, I can say that a number of the tests we undertake is the general philosophy to check by testing that all that is introduced in the project and in the calculations is indeed correct. It is not really even a problem to be solved. This is what we call confirmation testing, to check on a sufficiently representative model that all we had supposed and had introduced in our calculations was indeed correct and corresponds to reality.

Dr. KRASNOJAROV: And the next question concerns the purification system of Phenix - "L'ensemble du système de purification" - What do you mean?.

Mr. VAUTREY: I think the answer is quite simple here. It is the purification system which includes essentially the cold trap but with all that accompanies a purification system: an electromagnetic circulating pump, plugging indicators, sensors to control the purity of the sodium, and economizer in conjunction with the cooling trap, which is customary. And all this within a vertical tube, shroud, which would be in the main tank and which can be had access to vertically. So in Phenix the purification system of primary sodium was the only important part containing primary radioactive sodium which was outside the main tank. Is that clear?.

Dr. KRASNOJAROV: It is impossible to understand the meaning.

Dr. DÄUNERT: Just to explain this to Dr. Krasnojarov. I think the problem if very simple. In the case of Phenix you have a pool-type reactor. You have to have a system for purifying the primary sodium inside the pool. If you have a loop type reactor like BN-350 you can install this equipment along the piping.

Mr. VAUTREY: In Phenix all the primary sodium is contained here except for the purifying circuit with the cold-trap. In super-Phenix the purifying circuit will be doubled and it will be like this up here.

Dr. SMITH: On page 13 you are talking of later reactors and you are talking of reactors of possibly 1800 MWe or more. It seems to me there are serious difficulties in these larger reactors. Firstly, because of the worse sodium void coefficient in a larger reactor. Secondly, because of the longer refuelling times unless you go to a larger sub-assembly which doesn't seem possible for transport reasons, and thirdly because if you do any maintenance you are loosing more MWs of power during the time the reactor is shut-down for maintenance. It seems that these disadvantages might more then cancel the advantages going to a larger scale. Could you comment?.

Mr. VAUTREY: Yes, on this point I would for the time being limit myself to a very simple answer. Firstly, because these studies are being undertaken at present and we have no results as yet, and secondly, because it is not at all sure a priori that we intend at a later stage to build any plants with a capacity of 1800 or 2000 MW. But, we are obliged to give due consideration to the ideas or requests of our clients, especially Electricité d'France - the French electricity authority - and such a capacity of 1800 MW electric is a capacity which has been considered. I won't say it has been proposed as being possible by the Electricité d'France. We are studying today the problems which would arise from such a power capacity and even if inconveniences should arise I can say that we haven't for the time being seen any impossibility or any real reason why we could'nt propose a plant of this capacity, if our clients so requested us to do so.

Dr. ENGELMANN: On page 10 you mentioned for Super Phenix that you only have a single pot or reactor vessel just with one wall, not a double wall. Is this correct?.

Mr. VAUTREY: No, there is not only one. As in Phenix, there is a double envelope. It is a double walled vessel with sufficiently small space so that in the case of a sodium leak, cooling would continue as normal. And, the decrease of cooling would not be too great. There is an additional concrete vessel with a double water-cooling circuit which is quite analogous with that which was done for Phenix.

Dr. ENGELMANN: But for Phenix you had another steel vessel with your water pipes in it. And now you have concrete?.

Mr. VAUTREY: No, I said that steel liner is against the concrete with two water-cooling circuits in parallel for emergency shut-down cooling.

Dr. SMITH: Could I also enquire on page 10 at the bottom of nearly the last paragraph, where you are talking about the introduction of new fuel elements - "utilisent un sas rotatif" - What is a sas?.

Mr. VAUTREY: You would probably say something like air-lock in English. It means that for Phenix the assemblies are extracted on a ramp in a sodium pot and here you have an air-lock with a balancing movement to take it to stocking. And in Super Phenix down here you will have a similar ramp but up here you will have a sort of air-lock which rotates and which will have two openings here and here. In other words, instead of having a balancing movement you will have a rotating movement. And this latter system is especially conceived so that we can at the same time bring in a new assembly, fresh fuel, here into the lock and at the same time extract an irradiated assembly. The irradiated assembly is sent to stocking and the fresh assembly is introduced to the core. At the same time, the two operations can take place in this way - the extraction and introduction.

Dr. KRASNOJAROV: On page 9 you mentioned about the detection of hydrogen that corresponded to about 350 mg/h. What does it mean?.

Mr. VAUTREY: It is not quite that. The value determined is the figure you will find a bit further down on the page. A concentration of hydrogen in sodium is approximately 0.090 ppm. That hydrogen comes from the diffusion of hydrogen through the walls of the steam generator tubes and we have determined by calculation that these rates of diffusion were at the beginning about 500 mg/h. That is the rate of hydrogen diffusion in the steam generator.

Dr. KRASNOJAROV: Can you tell us a few words about the source of hydrogen?.

Mr. VAUTREY: The source is obviously the water of the steam generator. I am not a specialist in these phenomena but I believe that it is known, especially when steam generators are started up, that there is hydrogen diffusion due to fixation of oxygen which lasts throughout the period, especially when a stable layer of martinsite is formed in the piping of the steam generators and this decreased thereafterwards gradually. And, I think this is a more or less common phenomenon frequently observed.

Dr. SMITH: This was observed in PFR start-up.

Dr. KRASNOJAROV: Then it is possible to observe hydrogen in steam.

Dr. SMITH: Well, it might be but we don't have hydrogen detectors on the steam. Theoretically there should be hydrogen in the steam but nobody looks.

Dr. KRASNOJAROV: It is possible to see hydrogen in the steam. There is a process of corrosion when oxygen from water goes into steel and makes magnitite and it is possible to see hydrogen in this case. In our case, during start-up of steam generator after shut-down, it is possible to see some level of hydrogen which then decreased. Do you mean these sources of hydrogen penetrate into sodium?.

Mr. VAUTREY: Yes, but I am referring to the hydrogen in sodium due to the diffusion of this hydrogen through the walls of the piping and tubes.

Dr. WENSCH: I can confirm the same thing for EBR 2 and others during start-up. The hydrogen levels are very high in the sodium because of the occlusion of moisture on the surfaces where rust was formed. This would last for several weeks or more until it was eliminated through cold trapping or had escaped through the steam generator. The other source of hydrogen, which would be very low, would be tertiary fission tritium, which would eventually leak out through the steam generators, but it is really minute.

Dr. SMITH: Do you know whether the tritium would go to the steam generators or to the cold trap?.

Dr. WENSCH: It is a free radical in a heavy ionized environment. Our studies show that it would go out through the steam generators, because the diffusion and movement through IHX's and Steam Generators would be faster than absorption in the cold trap. It is small compared to that formed and released by light-water reactors.

Dr. TOMABECHI: A few years ago I read an article published in the AN'S meeting and this article discussed tritium content, both in primary and secondary circuit of EBR-II. And, if I recall correctly, the ratio between the concentration of tritium in primary and secondary circuits is about 4:1, which means that the concentration in the secondary circuit is about a quarter of that of the primary, and the paper quoted over 60% tritium produced in the core trapped in a cold trap, and this gives us some idea how much tritium produced in the core will go to the steam side of the steam generator.

Dr. SMITH: Mr. Vautrey, you mentioned that Super-Phenix would have a circular secondary containment against design to resist an aircraft crash. Could I ask what sort of aircraft you assume for this design and how thick the building has to be. I think you gave us the diameter and height. How thick does it have to be?.

Mr. VAUTREY: I am not sure that I can answer your question exactly, but I think that the walls are to be about lm. thick, and designed to resist a relatively small aeroplane crash. I think it was supposed to be a plane with a wing span of approximately 7m. but I am not sure. I don't have any figures with me.

Dr. DÄUNERT: I would like to comment on this. The SNR had to be protected against the crash of a Phantom which I think is the strongest penetrating aircraft at the moment - this means 7000 Mtn/sq.m.

Dr. WENSCH: I have one general question which concerns all large part reactors - just how does one design the pool concept to withstand seismic disturbances?

Mr. VAUTREY: Well, I can try to formulate an answer, but it will have to be somewhat vague because this is really a specialist's problem. I know that in France we do have a number of companies which specialize in such calculations and it is not so much the CEA's work, and all parts of the installation are submitted to these sort of calculations on resistance to earthquakes and seismic disturbances. I cannot really say much more about it. The technical requirements specified for each part, and these specifications are worked out before the definitive version of the project.

Dr. SMITH: We have considered some of these problems but not to any great depth. You can certainly put round the tank at some low level a snubber to stop the tank's movement becoming too large in case of horizontal acceleration, and in the design in which the core support structure is taken out to the side of the tank, this will also prevent movement of the reactor itself. From that point onwards, your problems are probably very similar to those in a loop design, though you may concern yourself with the flow of sodium in the tank, but this is another problem. I don't know whether you had any specific facet in mind?.

Dr. WENSCH: No, not really, Dr. Smith, I was just thinking of having such a large massive body suspended where you have longitudinal, as well as vertical movements, and was interested in learning of the seismic criteria.

Dr. SMITH: I think the CFR will be designed to meet seismic criteria, but I think the level in the UK is lower than elsewhere - lower than in France.

Dr. TOMABECHI: The problem for us in the designing against earthquakes is escalation of magnitute of design base earthquake to be adopted. There is such a trend and people are talking about a higher number than in the past. Situation may become difficult, if such a trend continues.

5. Discussion of Dr. Wensch's Presentation

Dr. SMITH: I think under safety was this reference to SAREF. In view of the fact that we have a paper on view of testing facilities I wonder if you would just say what the situation on SAREF is?.

Dr. WENSCH: I can't say much about SAREF at this time. SAREF is also known as the Safety Test Facility. For many years we had funds put aside to build a safety test facility at Jackass Flats, Nevada. It was to expose fast reactor cores to very high excursion rates. This facility was terminated because no one knew how to design it then. Since that time many US scientists have been exploring test facilities to see if any could help answer the question of what happens when a reactor is exposed to a very high ramp rate or loss of coolant. This has lead to the Safety Test Facility now known as SAREF for Safety Research and Experimental Facility which is now in the conceptual stage. Its functions are now being delineated. Facility cost is estimated to be about $ 350-600 million.

Dr. SMITH: It is as noted on page 57.

Dr. WENSCH: Well, I have news for you. Originally, for budgetary reasons, we estimated $ 400 million but this was only a planning figure.

Dr. SMITH: Could you say anything about the actual status - I mean to say, what is the probability that the money would be available?. Do you envisage as entirely USERDA or would you try to make it as an international project?.

Dr. WENSCH: I can make a general comment. In the area of safety we believe there is no such thing as commercialism. Many years ago Professor Lejpunskij made a remark regarding fast reactors, which I have used many times, namely, "we are all in the same boat", and, particularly, this is true in the area of the safety. In speaking for ERDA, I would believe that serious consideration would be given by our officials in having SAREF help assist in meeting other needs outside of the US.

Mr. ALLGEIER: On page 29 I discovered a new item, at least an item I have not seen before in budgeting. It is Impact of Market-place and it is $ 35 million. Could you perhaps explain what this covers?.

Dr. WENSCH: The Impact of Market Place represents recent cost increases in raw material and fabricated product industries over and above the average rate of escalation during the 1972-1974 period. It is substantially in excess of that experienced by the economy in general. Current heavy industry orders by the nuclear community impacting estimated costs by reducing the competitive desire for new business and by lengthening procurement lead times for components and equipment.

Dr. TOMABECHI: On page 36 you have mentioned that the ERDA is now considering expanding the capability of SCTI and you said you are now considering increasing it to 75 or 150 MW (th). Could you please elaborate on this?.

Dr. WENSCH: There are two facilities involved. The SCTI is a very old facility going back to 1958 and it is being modified by replacing some pieces of equipment which may no longer be fully dependable. Plans are being made to increase its capability, to 75 MW(th). The Plant Component Test Facility will be a brand new facility and, indeed, it may be more than 1 facility. It is in the conceptual stage now and it will be in the order of 150-200 MW(th).

Dr. ENGELMANN: I have a question on your Fig. 10 on page 25, in connection with one of the last slides you showed. On this slide you explained by a proper initial operation of fuel pins that you can increase the fuel pin rating by 20%. In this Fig. 10, I think you do not give any fuel ratings for FFTF, CRBR and the Commercial Prototype. Can you add these figures?. Do you have them in mind?. And do these figures already make use of the 20% increase, which seems to be possible?.

Dr. WENSCH: I don't have the figures in my mind but we have them in the document on operating data of parameters. They will not take into account the 20% improvement.

Dr. ENGELMANN: Well, I can look this up but in this document the commercial prototype is not listed. Do you plan to take this into account for a commercial prototype?.

Dr. WENSCH: We will if it is confirmed by operational experience.

Dr. SMITH: I would like to ask Dr. Wensch - this initial mode of bringing up to power is in some way connected with melting or recrystallizing the fuel. What sort of treatment does this consist of and how quickly can you bring it up to full power?.

Dr. WENSCH: This observation was first discovered at HEDL by Dr. Ersel Evans and his colleagues, and I believe that it depends on bringing new fuel elements up to full power over a period of about two days.

Prof. PIERANTONI: On page 13, Fig. 4. you are referring to research and development programme. What is the difference between Cooperative Power Reactor Demonstration and the Liquid Metal Fast Breeder Reactor?.

Dr. WENSCH: It is simply a way of defining different kinds of money. In this case the Cooperative Power Reactor Demonstration would be money used for CRBR. You could not take that money and use it for other kinds of LMFBR activities. It can only be used for this project.

Prof. PIERANTONI: On page 58 you are referring to a 19 pin advanced carbide fuel sub-assembly. These pins have a sodium bonding or a gas bonding?.

Dr. WENSCH: They use a helium bond.

Dr. ENGELMANN: On the same page and the same paragraph you mention that 75,000 MWD/T correspond to 50% of the burn-up expected in a commercial LMFBR and you also mention on another page these high burn-ups. Do you consider these high burn-ups as realistic targets for the present kind of cladding, and wrapper materials?.

Dr. WENSCH: Yes, for an optimized commercial plant.

Dr. ENGELMANN: To what amount of swelling would this lead? How many vol.%?.

Dr. WENSCH: The target of the advanced alloy programme is to achieve 5 percent volume increase at $2.5 \times 10^{23} n/cm^2$.

Dr. ENGELMANN: I am asking the question because our materials' experts say, that from the present experience which goes up to $8\ 10^{22} \div 10^{23} n/cm^2$ above .1 MeV, it is very difficult to extrapolate to such high doses because a burn-up of 150 000 MWD/T in a large LMFBR would correspond to, say, $3 \div 4 \times 10^{23} n/cm^2$. This means a factor of four in extrapolation. Now the experts say that from their present knowledge it is not clear if they can just make a straight extrapolation and how the exponent of the volume increase behaves. There are indications that the exponent might increase with dose becoming larger than 1, and then perhaps the swelling would be prohibitive for such large burn-ups, but from discussions here and also from the presentation of Dr. Smith, I got the feeling that in your countries you are not so much worried as we are.

Dr. WENSCH: Indeed, we are. I will rephrase the answer to help clarify this important point. This is part of our advanced fuels programme. 150 000 MWD/T is based on the assumption that as part of the advanced alloy and fuels programme we can achieve very high burn-ups. Now in stainless steel - 316, cold worked,we have achieved the order of 7 or $8 \times 10^{22} n/cm^2$ with no significant problems. I am not sure, if this satisfied your question, Dr. Engelmann?.

Dr. ENGELMANN: Yes, if you refer to your advanced alloys programme then maybe it might be possible.

Prof. PIERANTONI: What type of reprocessing do you plan to use for advanced carbide fuel?.

Dr. WENSCH: There are some preliminary processes underway at Oak Ridge. You may have read about where they burn-off the carbon, then take the residue and treat it aqueously.

Dr. KRASNOJAROV: On page 42 you mention that clad breach occurs predominantly as hairline cracks or very small pin holes with attendant fission gas release, and without solid fission product release. And I believe you extract your failed fuel element immediately without solid fission product release during some minutes, some hours after first indication about failure. Have you any experience in finding out failed fuel element without solid fission product release during operation with failed fuel element to extract failed fuel element immediately?

Dr. WENSCH: First of all, the cover gas is being monitored continuously for traces of radioactive fission product gases, and they are detected very quickly after fuel element leaks. EBR-2 has metallic fuels for drivers. In certain parts of the system we also have delayed neutron sensors. So, we can usually tell-if the driver fuel has failed we pick up two signals, but if it is one of our other fuels, we don't get any delayed neutron signal; we get the fission gas release immediately. As you may recall, all the experimental pins are tagged with Xennon so that another signal may be obtained from a leaking pin.

Dr. KRASNOJAROV: After what space of time do you usually stop?.

Dr. WENSCH: Very quickly, depending on the size of the leak.

Dr. ENGELMANN: Do you shut down the reactor immediately?.

Dr. WENSCH: Yes, it is a standard operating principle.

Dr. KRASNOJAROV: I want to say that solid fission product released and you have some experience in the USA about quantities. I have read proceedings of one of the conferences on the quantity of different fission products released and it is possible to have 30% ÷ 50% caesium from pin inside sodium later - but of course immediately after fuel failure. The first indication should be gas, later - delayed neutrons.

Dr. WENSCH: What we are talking about here are the fission products which are volatile at these temperatures. So, some iodines and other fission products which are volatile will go out like xennon. But, there is very little caesium at all - I don't recall that there is any caesium in EBR-2's primary sodium.

Dr. ENGELMANN: The reason is the different way of operation. If you continue to operate with defect fuel pins in EBR-2 until the cracks become wider open, then you will have caesium, but you stop before.

Dr. KRASNOJAROV: We have some operating experience with failed pins on BR-5 and BOR-60. It was observed that delayed neutrons appeared very soon after or sometimes together with fission gas on BR-5. Now, more often we see delayed neutrons later than fission gas - sometimes some days ago but solid fission release.

Mr. VAUTREY: I think if you are observing fission products in the sodium in a very short lapse of time very rapidly, it is probably a quite large clad breach. In Rapsodie I must say that most of the breaches in the cladding were hairline cracks or very small pin holes which don't grow and which only allows volatile products to escape.

Dr. WENSCH: I recall in one case we detected a leak and found the sub-assembly and went through each pin. We knew that the sub-assembly had a leak in it. This was a 19-pin test bundle, but they could never find a leak in the clad. It was so minute that when the fuel element had cooled down, they just could'nt find it.

Dr. DÄUNERT: On page 29, Dr. Wensch, in your cost estimate for CLINCH RIVER project, you mention escalation from 1974-1987 accounts for $ 498 million. What inflation rates did you anticipate for these years?. It is a very difficult thing and you reach a very exact figure.

Dr. WENSCH: We assume an average escalation rate of 8 percent. We have asked for a revised authorization from Congress, which will include everything for building the plant, but escalation is going to be treated as a separate item. Each year we will go back to Congress and state what the escalation for the year has been, so there can never be any question that escalation represents an overrun, because of either poor planning or management.

Dr. DÄUNERT: But nevertheless, Dr. Wensch, you proposed further escalation here and you can easily be wrong in the percentage of 10-20% just due to different figures you take for the inflation rate. This is only a remark.

Dr. WENSCH: We took 12% for last year, which was very high for the US. In our calculations we had based it on an average of 8 percent, which was too low.

Dr. ENGELMANN: In your report you mentioned the result of some reviews and you pointed out that they stressed very much the importance of low-doubling times. Now we have recently made a similar assessment study in preparation for the justification of the SNR-2 project, and we have found out that starting from new predicitions of power needs in the future the picture drastically changes. Several years ago everybody, and especially the utilities, predicted continuing large growth rates of electric power demand. Now, since the oil price was increased two years ago, we see that the energy consumption goes down and that it is not clear that we will continue these growth rates over long periods, and the new assessment of energy needs in the future shows lower growth figures. If we base our estimates on these new predicitons, then we come to the conclusion that even with poorly breeding oxide fuel in LMFBRs we can drastically reduce the uranium consumption. I would like to draw a little picture (fig. 1) which shows the annual U_3O_8 consumption and is based on the area of Germany, Belgium and the Netherlands. The figures correspond roughly to 10% of the world's figures at the moment. Up to 1995 we have the expansion of light-water reactors which need quite a bit of uranium. The U_3O_8 consumption would continue to increase like the upper curve if we had only light-water reactors. If we have breeders then we can reach a maximum in the annual uranium ore demand which,in our case, is about 20 thousand tons U_3O_8 per year round the year 2010. But, this now depends on the kind of breeder, as you see in the figure. If we have carbide breeders we will reduce this demand at an earlier time and we will have a lower maximum than with oxide breeders. We have also made evaluations for the gas-cooled breeder. They show a higher consumption at the first stage because the inventories of these reactors tend to be higher. Due to the better breeding, later on the U_3O_8 consumption of GCFR's, however, will be a bit lower than that of the oxide breeder. If you draw the picture of the cumulative amount (fig.2), that means the total amount, cumulated to the year, then of course with light-water reactors you would go up but with the breeder you will reach saturation at an amount of U_3O_8 which again depends on the kind of breeder. But, as long as you have real breeding, that means a breeding ratio $\geq$ 1.15 in order to cope with all the losses, the figure for the German, Belgian, Netherlands-zone will not go up more than 10^6 tons of uranium ore, and we are quite satisfied with this. Our study also shows that the doubling time is not the real important figure. There are other figures which are figures of merit for breeder reactors. It may be helpful to have a plot to assess the quality of the breeder system (fig. 3). If you plot here the total system inventory, that means the fissile material in the reactor and in the external circuit of the breeder, versus the net amount of fissile plutonium produced per Gw-year, you will find that with increased system inventory one tends to be on the upper side of the U_3O_8 consumption and again with low plutonium production one tends to be on the upper side. So this means that the lower right corner is the best one and the upper left corner is the worst one from a strategic point of view. You see where the oxide breeders, the carbide breeders, and the gas-cooled breeders are (OBR, KBR, GBR). All the breeders do the job and of course a carbide breeder would do a better job but the question is whether we need it. Maybe we don't need it now. Our argument is the following. We feel we should develop the carbide breeders for two reasons in spite of the additional money required. Firstly, we are not certain that the present predictions on the power demand are on the conservative side - Maybe we will come to higher consumption again. Secondly, we feel that the breeders can be used to produce not only plutonium but also uranium 233 to supply fuel for high temperature reactors, which could be used to produce not so much electricity but process heat for coal gasification and perhaps later on for hydrogen production. That means for various applications of process heat. If you add the additional demand for the high temperature reactors, which are not breeders but convertors, that means that you use part of the breeding gain to produce uranium 233 instead of plutonium, in the radial blanket. Then you have to go to a little bit higher breeding ratios to have at least a net breeding of one for the breeders themselves. In summary, we feel that there is no need to reach doubling times of the order of ten years and that in the beginning the doubling time has no effect due to the plutonium produced by light-water reactors. Later on there is a competition of the total system inventory and the net plutonium production.

Dr. SMITH: Certainly the studies in the UK would very much support the kind of thing that you have been saying and this perhaps explains our policy. We certainly see the most important thing is to get the fast reactor established and at a comparatively low breeding gain. The doubling time for these fast reactors is not to be considered very important. All this sort of study which we do tends to take just a reactor with a fixed doubling time for the whole of 50 or 70 years or we assume that at some time we change to a better reactor which for study purposes we usually assume to be a carbide reactor. We certainly support the importance of inventory in the early days, at least, because we start off from the plutonium accumulated from thermal reactor systems and this very much affects the number of fast reactors that you install. Nevertheless, our predictions do show a need for a better doubling time than we have at present. I would put the need as probably being in the range of 10 to 15 years and probably, not as Dr. Engelmann says, a need or a strong need for a doubling time less than 10 years. But these things are so dependent on the assumptions of power growth rate that it is very difficult to be positive on this need. So our policy, therefore, is to get the reactor established with oxide fuel because that is advanced now. Then our advanced fuel will be carbide which should give a better doubling time. I think all one has to do at this stage is to avoid any sort of commitments which would preclude you from going to a shorter doubling time at a later date though it is not too easy to see what these are. While we are on doubling time could I just ask one or two questions?. On page 59 of your paper at the end - you talk about a 16 year simple doubling time. My belief is no doubling times are simple. This, I presume, is a linear doubling time as opposed to an exponential doubling time and again raises the question of what out of pile reprocessing losses are assumed. There are a number of references to doubling time in your report. Is there a general assumption to what these out of pile losses are?.

Dr. WENSCH: They are about 1%. I would like to make a rejoiner to both you and Dr. Engelmann in that I was careful in saying that it was only recommended doubling time. Some U.S. experts imposed a goal on us and we too feel that it is much more important to establish a successful operating reactor that works well and then consider reducing the doubling time as a longer range effort.

Dr. ENGELMANN: I very much share your view and also the view of Dr. Smith. The most important thing now is to get the cheapest and most reliable system irrespective of the doubling time. A low doubling time is a question of tomorrow. This can be improved in a step-wise approach. But, if you don't get the breeders started then you will never make use of low doubling times.

Dr. SMITH: I will just make one other detailed comment on the references to breeding ratios. It is worth noting that the breeding ratios can change quite a lot, depending on what quality plutonium you are using in terms of isotopic composition. Some of these calculations can be done for sort of equilibrium plutonium you get with a fast reactor system. But, in fact it will be many years before you get to this and one needs to be rather careful to specify to what sort of plutonium these figures refer. Another point on the breeding ratio is the following. We, as you know, tend to use this breeding gain term because this takes account of the different values of fissile isotopes, whereas the normal breeding ratio doesn't. That is a further source of inaccuracy.

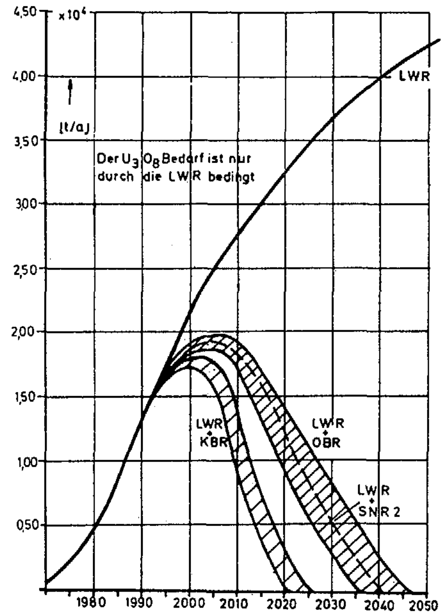

Figure 1. Annual U_3O_8 consumption.

OBR = oxide breeder
KBR = carbide breeder

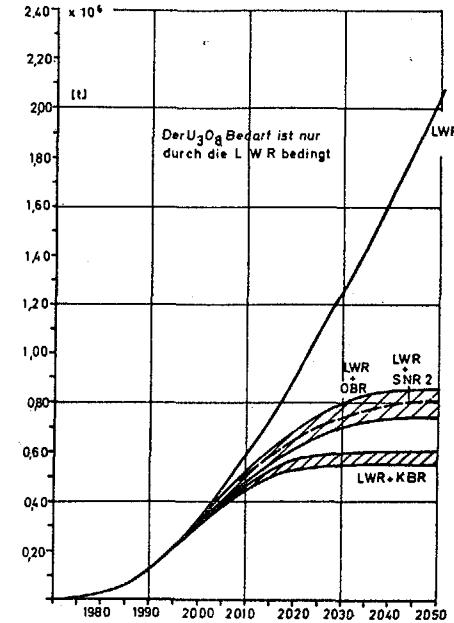

Figure 2. Cumulative U_3O_8 consumption.

(these figures refer to Germany, Belgium and the Netherlands)

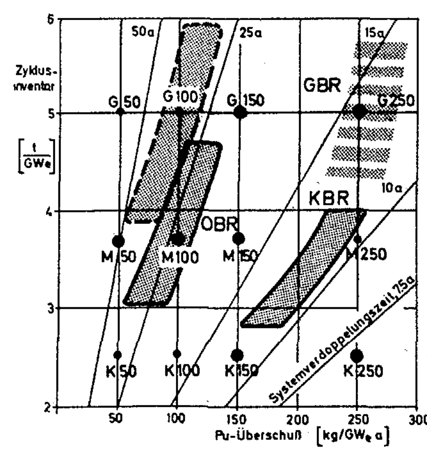

Figure 3. Parameter Range of FBR's:

GBR Gas Cooled Fast Breeder
OBR LMFBR with oxide fuel
KBR LMFBR with carbide fuel

The total Pu inventory depends mainly on excore time, the net Pu production depends partly on the doubling time.

Mr. ALLGEIER: I have a question along similar lines. I notice the budget figures for Molten Salt Breeders and Gas-Cooled Breeders on page 13. They are small but they are not negligible and at least for the GCFBRs they seem to grow rather fast. So, I would like to know what is current ERDA philosophy in regard to alternate breeder development, both for the near term and, if possible, for the long term?

Dr. WENSCH: Before ERDA came into existence the philosophy on the future of the gas-cooled thermal reactors was coupled closely with the operation of the Fort St. Vrain Nuclear Generating Station. If that reactor will operate and operate well it would then encourage the introduction of improved gas-cooled thermal reactors for process heat and so on. When it comes to gas cooled fast breeders our effort is exploratory, doing criticals and also piggy-backing fuel development for gas-cooled fast breeders within the LMFBR programme. Have I answered your questions, Mr. Allgeier?.

Mr. ALLGEIER: Yes and no. I would have liked to know is it a back-up philosophy or are you maintaining an existing effort at a time when it would be premature to abandon it completely?.

Dr. WENSCH: I don't believe you could say it is a back-up effort because the difference in support is so great. If you look at gas-cooled breeder effort at 6 million as compared to 261 million for LMFBRs it represents back-up effort - it is exploratory. Our officials believe you shouldn't put all your eggs in one basket. If we had to go to the gas-cooled fast breeder significantly more funds would be required, probably in the order of what is going into the LMFBRs.

Dr. ENGELMANN: Maybe I can add to the answer to the question. At least from the fuel conservation point of view, our studies have shown that the GCBR, in spite of its higher breeding ratio, does not bring anything which the LMFBR couldn't bring. Of course, there might be differences in the technology and here again it is very important to come into operation and to examine how the HTR development as a whole goes. Because I believe without the HTR development there will be no gas breeder - that is for certain.

Dr. SMITH: You left out ZPPR in your presentation. You did state somewhere that this is the largest plutonium zero power reactor. I believe I am correct in saying that you haven't really used its full capacity and now that you are no longer talking of a second 300 MW reactor and after the CRBR you are going into a 1200 MWe reactor. Does this mean that you will alter the programme of ZPPR so as to study large cores at a rather earlier date than you had previously planned?.

Dr. WENSCH: Plans are under way to mock-up 1200 MWe systems. There are also some studies under way on 2000 MWe systems. These are being done at Argonne. I don't know of any firm plans to do mock-ups of reactor core of this large size. Our plans are to do the CRBR and follow with the near commercial plant which should be of the order of 750 MWe.

Dr. SMITH: I think the physics results of a 1000 MW mock-up would be of interest to many countries, and 'though I believe the physics predictions are really very good for that size reactor, confirmation of these would be welcome.

Dr. TOMABECHI: I would like to ask one question regarding the one slide shown to us. You mentioned the Laser technique being used for welding the pins when you put the tag gas into the pins. Would you explain a little bit why you use the Laser technique?.

Dr. WENSCH: Well, the attributes of the Laser technique is the very precise positioning of directing the heat exactly where you want it within a closed container. You don't have any fumes or sparks from welding devices. It has more attributes than disadvantages compared to electron-beam welding or inert arc electrode welding.

Dr. TOMABECHI: In other words, if I understand correctly, you have said you are moving to the Laser welding technique for production of future commercial fuel?.

Dr. WENSCH: No, only tagged sub-assemblies will be made this way, and all assemblies in FFTF will be tagged.

Dr. ENGELMANN: Maybe there is a misunderstanding. This Laser technique is not used for welding end caps. It is only for this special tagging with this xennon-krypton mixtures. I have seen it at Hanford. You take a full bundle of one FFTF sub-assembly for instance and at the upper end of the fuel pin there is already incorporated a little gas container. Due to gravity or something a piece of metal with a pin drops and makes a little hole through which then the gas enters into the pin. With the Laser technique you can close a very tiny hole by complete remote operation. That is a very fine technique. This is not an end cap welding. It closes the hole of a millimeter diameter.

Mr. VAUTREY: I should like to ask Dr. Wensch if he could give us a few details with regard to page 33, point 8. What is this instrumented sub-assembly for charterizing reactor environment?.

Dr. WENSCH: I can tell you what the instrument is used for. It is being used to determine the temperature distribution and the neutron flux throughout the core. From time to time each user of the EBR-II receives a three-dimensional spatial map of the neutron flux at various points. So when he puts his experiment into the EBR-II he knows what the neutron flux will be.

Dr. SMITH: You referred to in your presentation the stretch capacity of the Clinch River Breeder Reactor at one point. Could you just explain a bit more?.

Dr. WENSCH: We know that Clinch River components are going to have some stretch. Very fortuitously the stretch capability may equal the improvements that we hope to get from the fuel's improved performance. Normally in U.S. utility practice allowance for about 10 percent stretch is incorporated in design. On a generator of 500 MW(e) one usually expects a 10% stretch.

Mr. HOSAKA: On page 28, figure 11, you mention that the cost of the Clinch River project is carried in this cooperative power reactor demonstration programme. Is this only the 1.2 billion for plant investment or does it also include the 434 million for development costs?. Or whether the second part is financed directly through ERDA to various laboratories and so on?.

Dr. WENSCH: The 434 million is for the development, systems engineering prototypes and testing involved. There are other kinds of research and development which are not shown here. That is the part I mentioned on the base programme. Some work being done for FFTF in safety would have application to CRBR.

Dr. SMITH: Perhaps another way of putting the question is on page 13 of figure 4 there are some numbers. Which number on this one does the 434 come in?. Where does the 434 million fit into these numbers?.

Dr. WENSCH: The 434 would fit with the 35.4.

Dr. SMITH: That puts it straight.

6. Discussion of Mr. Allgeier's Presentation

Dr. SMITH: Could I just ask about the term hot fuel and cold fuel. Is this for gas-cooled reactors or liquid metal reactors, and what do you mean by these two concepts?

Mr. ALLGEIER: It is for liquid metal reactors and these two concepts mean sodium bonded and helium bonded. The main problem of these carbide fuels is swelling and there is a theory that at certain temperatures you have a critical temperature in regard to swelling.

Dr. SMITH: I understand now but I didn't understand the term hot and cold fuel.

Dr. ENGELMANN: In your last sentence you mention that the Commission does support extension of any suitable activity. Does it mean supporting by money or by more ideal means?. What kind of support are you thinking of?.

Mr. ALLGEIER: It means merely that in our opinion there are areas, particularly in the safety field, where there is room for improving international collaboration beyond existing projects and beyond the Community. I am thinking in particular of code development work and things like this. And if there are suitable initiatives anywhere I think the Community would be willing to lend its offices and to make an effort to find money, if necessary.

Dr. TOMABECHI: Again a question of terminology. On the front page at the bottom there is stated a sub-group for Containment Loading and Response. What does it mean?.

Mr. ALLGEIER: The first sub-group on Whole Core Accident Codes goes as far as developing work considering codes to calculate the energy release in case of a whole core accident. The second sub-group then would consider codes to calculate the loading which is resulting from such an excursion and the structural response.

7. Discussion of Dr. Krasnojarov's Presentation

Dr. SMITH: BR-5 was to become 10 MW reactor and be called BR-10. It is now $7\frac{1}{2}$ MW. Is it BR-$7\frac{1}{2}$?

Dr. KRASJOJAROV: Just after the main vessel we have a main safety cylinder, which is with drive mechanism. And here is a small gap which is very sensitive to the temperature. When I was in Obninsk as a physicist who was connected closely to the BR-5 we investigated their thermal reactivity coefficient. We decreased the air fluid through the reflector and discovered that ability to move was limited at a higher temperature. This phenomenon was investigated and we decided not to increase the temperature. But after reconstruction they increased power and temperature that have effects on these cylinders. It was possible to orientate this cylinder and to increase power. They tried to do so but had not reached the expected result. They continue work trying to improve cooling. The whole system, heat exchanger and so on, was reconstructed for 10 MW. But this point does not permit to reach this power and, of course, some safety precautions. When it is necessary they operate at a full power during half an hour. But, now it is not the main installation for irradiation. We have BOR-60 and our task was not to stop operation BOR-60 in any case.

Dr. TOMABECHI: Could I have the figure for the diameter of the vessel of the reactor BR-5. Is it 330 mm?.

Dr. KRASNOJAROV: Our first core was 30 cm of height and a little less than 30 cm. in outer diameter. Later, the height of uranium oxide core was 40 cm. but carbide core was 30 cm. again and diameters were about 30 cm. Everything here was loaded with fuel. It seems we had a layer of reflector. Its efficiency is very high, of about 6%. We sometimes tried to compensate some effect of reactivity using it. So the diameter of the vessel is about 330 mm.

Dr. SMITH: And the dose was 4×10^{22}. Is that the dose?.

Dr. KRASNOJAROV: Yes, it was the estimated dose on the vessel.

Dr. ENGELMANN: Could you read the swelling formula on page 2 and check whether there are errors in it. Is this correct, $10^{-48.31}$ and what is in the brackets $(5.99 - 10^6)$ or (5.99×10^6)?. And what is T and T_2?.

Dr. KRASNOJAROV: This formula was written in a paper of Kochetkov, Orlov that was presented at Atlanta meeting of ANS last autumn. I have requested to write it correctly from my copy to this report. Maybe there are some additional indications about it in the report.

Dr. ENGELMANN: Can we ask you to correct this formula for the proceedings of this meeting and have the correct formula in it?.

Dr. KRASNOJAROV: I suppose you have some proceedings of the Atlanta Meeting.

Dr. WENSCH: Would you elaborate further on the nature of the particles and the size of those that fell into the low pressure plenum and what procedures were followed to assure yourselves that you did not have any clogged sub-assemblies or things like that.

Dr. KRASNOJAROV: Of course, I don't know exactly about sizes. Give me some spectacles to look through sodium.

Dr. WENSCH: That is no answer.

Dr. KRASNOJAROV: This is lower pressure plenum, this is higher pressure plenum. We cleared our fuel sub-assemblies from here.

Dr. WENSCH: Just like EBR-2.

Dr. KRASNOJAROV: Yes. The sodium from this place goes into the steel reflector. We have the same shape element from steel as fuel sub-assemblies. They are not solid, they have sodium inside which goes in such a way. It was arranged to make the temperature more uniform as you had in EBR-2 reactor. Of course very small quantity of sodium is necessary for such steel reflector element. To maintain some pressure here we have some special places where we have orifices. At first we discovered that we had more cold sodium outside the core than we expected. We decreased orifices and now the velocity here is too small to think about possibilities of any damage and especially because we feed from this low-pressure plenum of sodium only steel reflector elements.

Dr. WENSCH: What is down here?. Just pieces of steel from the fuel sub-assemblies or part of the control rod material too.

Dr. KRASNOJAROV: Four heads of the fuel elements and some pieces from control rods. There are 7 of them inside the tube. Only later will it be possible to try to connect everything and to know what kind of materials we have missed. But now we don't know about them. When we extracted one sub-assembly one of the pieces of the control rod had been extracted too. When we had extracted it we discovered a high level of radiation.

Dr. WENSCH: Did any of the poison rods get torn apart?. And also did any rods come apart?.

Dr. KRASNOJAROV: The lower part was inside this basket and it was extracted. The upper part was inserted into a special basket and extracted. Of course, maybe some boron carbide pellets had come apart?.

Dr. SMITH: Do you know why the rod came in half in the first place?.

Dr. KRASNOJAROV: We consider that the personnel missed some repairs. The efficiency of central control rod is high enough and, of course, in normal conditions it is very difficult to extract it without taking notice of it. At first, we used a Po-Be source to control sub-criticality and start-up. But its intensity decreased and it was necessary to have a new one. And we decided to use γ-Be source inside the core. We increased the intensity of the source and observed some reactivity change. But, of course, after shut-down the intensity of such source decreased and this operation to withdraw driver and to disconnect driver with control rod was done half-an-hour after shut-down. It was an ordinary operation. It was done and at that moment the intensity of the source decreased. The people in the control room said that nothing was noticed. But we checked and discovered that it could be so when you extracted a control rod.

Dr. SMITH: But, if for the moment we assume that it is real, that is the moment when the control rod was withdrawn. What does that bump mean?.

Dr. KRASNOJAROV: Maybe it wasn't disconnected. Maybe something happened when they disconnected it. We don't know.

Dr. SMITH: Then you rotated the core?.

Dr. KRASNOJAROV: Yes. They noticed that the temperature was not so uniform. There were some places where the alloy could freeze. But they could not suppose that the control rod was still latched.

Dr. WENSCH: Was the control rod still latched?. Was that what happened?.

Dr. KRASNOJAROV: They explained that everything was done as it was necessary. Then they decided to rotate the core. They met with some difficulties to move but they didn't interrupt the motion. It was just after irradiation, a special sub-assembly was irradiated for physicists. They were interested to have it as soon as possible. When they met such difficulties they decided that maybe an alloy is not in some places. When we stepped up part of our sub-assemblies and when we returned to the control rod we discovered that when we tried to move it down by the driver mechanism it stopped at an intermediate position. It was during the last days of July. And only then was it possible to enquire what was done by you and by you and what did you see at that moment.

Dr. DAUNERT: If I understood it right, you have now a lot of things in your coolant which should not be there. Maybe boron carbide pellets and also the broken parts of the heads of the fuel elements. Don't you think you could have an accident by having a blockage of a cooling channel like in the Enrico Fermi, for instance.

Dr. KRASNOJAROV: Before we made our decision to go to criticality and to go to power we had discussed where such places could be.

Dr. DAUNERT: This, I understood Mr. Krasnojarov, but what about these broken parts from the heads of the fuel elements?. In the case of small parts like broken particles they had been pushed into this lower pressure plenum. But the broken parts of the heads of the fuel elements are a little bit larger. Do you expect they are all in these lower pressure plenum?.

Dr. KRASNOJAROV: The gaps between sub-assemblies are very small compared to the size of the heads of the fuel elements. The place where the cut heads of the fuel elements could be, was checked by special devices and then by installation of a new sub-assembly. And we could see that this place was empty.

Prof. PIERANTONI: How many control rods did you break?.

Dr. KRASNOJAROV: What do you mean?.

Prof. PIERANTONI: As I understood, you have 5 sub-assembly heads which were cut. And you get one of these heads and four are still in the reactor and you say you have small pieces of boron carbide in the lower pressure plenum. Have you only one control rod damaged or more?.

Dr. KRASNOJAROV: One bundle was damaged and it consisted of seven rods.

Dr. ENGELMANN: You mention that the steam generators of BN 350 are under repair, that two units have been repaired and that others are being repaired right now. Does this mean you will replace or you will repair all five steam generators?.

Dr. KRASNOJAROV: The decision was to change the tubes in all steam generators except the sixth one. Usually we have only 5 steam generators in operation. Now it is possible to have three units in operation - others are being repaired.

Dr. ENGELMANN: And when do you expect the BN 350 will be on full power?.

Dr. KRASNOJAROV: I don't know. It is necessary to finish the repair of the steam generators, to test them in operation. We are going to clean and change their tubing because of a lot of sodium water reaction products lodged there. We have also to take into account the possible effect or attack of some products lodged in the circuit. It is necessary to be careful.

Dr. SMITH: Did you completely change all the tubing in two of these heat exchangers - not just the faulty tubes, every internal tube?.

Dr. KRASNOJAROV: The decision was to change everywhere.

Dr. SMITH: And is it the same tube that you are putting back of improved quality or is it a different tubing?.

Dr. KRASNOJAROV: It seems to me that the thickness was a little more, amd the welding was checked more carefully. Some improvement was made.

Dr. SMITH: Is it the same material, the same steel?.

Dr. KRASNOJAROV: I don't know exactly. I think so.

Dr. SMITH: You said that you thought the original fault was in a weld right at the bottom of one of these sealed bayonet tubes. Is that right?.

Dr. KRASNOJAROV: You see here, there is the largest difference between sodium temperature and water temperature. But people calculated stresses and so on and proved that everything would be good.

Mr. VAUTREY: I should like to ask a small additional question on the same subject. You mentioned changing the tubes. What does that mean exactly from the point of view of the construction work?. Is it really a completely new apparatus or mechanism that has been designed and built or is it merely a repaired old apparatus or mechanism with new tubes?. In other words, did you retain anything from the old apparatus and mechanisms and only change them partially or is it a completely new mechanism?.

Dr. KRASNOJAROV: I don't know the details. I think that they changed the internal part of the evaporator. But, I think it will be possible to discuss all these details at a special meeting, together with your colleagues, on steam generators.

Dr. TOMABECHI: I have missed what you have said on the physics of BN 350. You said you have noticed some deviation from calculation in temperature and power coefficient. What is the cause of this?.

Dr. KRASNOJAROV: The difference between calculated and measured figures may be (10 ÷ 15)%.

It is possible to say that the experiment proves the calculation.

When people think about better coincidence they want to improve something in calculation. But, the difference is such that after improvement you could have some deviation in another direction on the next reactor. I consider that there is no need to pay attention to the deviation of (10 ÷ 15)%.

Dr. SMITH: You were talking on the use of failed fuel elements in BOR-60. Did I understand you to say that active caesium is the limitation on the use of failed fuel elements?. It is the most important contamination and that you are hoping to get rid of this by designing some special trap. And if you get rid of the caesium will you have problems from other materials or is the caesium by far the largest?.

Dr. KRASNOJAROV: If operating with failed fuel elements only some hours or some days then maybe caesium will be the main product that will be released from fuel into coolant. But we wanted to see whether it was possible to operate about a month or two months. We wanted to have experience and to get figures about quantity of different fission products that would be released from fuel into coolant. We got such figures and now we see that (80 ÷ 90)% of radiation level in the primary circuit is due to caesium. Therefore, the first task under this condition is to have possibilities to extract caesium. When such problem is solved it will be possible to think more actively about others. But, for example, barium, lanthanum and zirconium, niobium, have not too long a life-time. We increase and increase the quantity of caesium. In the case of other products it is possible to wait a little and to decrease the activity. And it is impossible to wait in the case of caesium.

Dr. WENSCH: This is a minor question dealing with the BOR 60. You said that on the surface of the sodium you saw oxides. There is no explanation why oxides, are there?.

Dr. KRASNOJAROV: We have movable plugs and there is a hole there. When we operate with a special machine we have argon everywhere and there is no penetration of oxygen inside. But, when we met with our problems this hole was opened. We put some rubber tube and gave argon here. But, of course, we inserted some devices there. Each time oxygen was on the surface of these devices which,contacted with sodium and the small quantity of oxygen, gave us some oxides.

8. Discussion of Dr. Tomabechi's Presentation

Dr. SMITH: Could I just ask one point - the number of technical people which is quoted here as being 440m.PNC; does that include virtually everybody in Japan on the fast reactors or are there other people working on the fast reactors at other organizations?.

Dr. TOMABECHI: It is the number of employees within the PNC, working on the fast reactors and this figure does not include the people who are engaged in the fuel fabrication. It is very difficult to figure out the number of people who are engaged in the fast reactor programme in industry and I can't even guess. Perhaps we have the order of 1000 or something like that. We have 4 manufacturing groups in industry. There is one more group but this group only deals with fuel.

Dr. SMITH: But these people are people essentially involved in manufacturing components as opposed to designing or R & D. You do have a substantial amount of R & D work in industry. Could you guess how many people there are in that sort of science or is it just too difficult?. I know it is difficult in the UK.

Dr. TOMABECHI: Sorry Mr. Chairman, I can't guess how many people are engaging in R & D work in industry.

Mr. VAUTREY: I should like to ask two questions. On the top of page 5 you speak of testing steam generator No. 2 and say that it is also of helical coil type. What then is the essential difference between the first and second steam generator?. And my second question on page 5 concerns studies on water leaks in steam generators. You speak of a 1/2.5 reduced scale model of the steam generator and add that the tubes of the simulated evaporator and superheater are of full size. What exactly are the specifications of this model then, please?.

Dr. TOMABECHI: Your first question is what is the difference between No. 1 and No. 2 steam generator. Designs are a little bit different but not very much. For instance, the size of the tubes are a little different, but the materials are the same. The top of the steam generators, for instance the tube sheet and the headers of the water pipes, is a little different. Our designers like to see which design is better. And the second question - The idea is to obtain the information about the propagation of pressure waves to various parts of the loops by simulating instantaneous rupture of tubing. We installed the tubes of full size in diameter, but shorter in the length. The size of the steam generator shell is of a 1/2.5 reduced scale. This means we are simulating a part of the steam generator, but we hope we can analyse properly experimental results for extrapolating to the size of the Monju steam generator.

Mr. VAUTREY: It is not exactly a scale-in fact it is a part of the steam generator.

Dr. WENSCH: The test of your reactor container is essentially the same as we have for FFTF and most of all the reactors being built and licensed in the United States.

Dr. TOMABECHI: Thank you very much. I would like to have these test procedures. I wish to add a few words. We have tested twice the leak rate of this containment already, first when we built the containment and also after putting concrete in it. The results of these tests showed us that the leak rate is less than .1% for 24 hours at design pressure. However, if we put the sodium in the loops we can't perhaps verify experimentally that the leak rate is below .1%. This is the main concern at the moment.

Dr. DAUNERT: Mr. Tomabechi, you told us that you had difficulties with siting of the Monju project. Is there some activity going on in order to get the licence for siting?. Are there negotiations of the PNC with the State government or what is going on today?.

Dr. TOMABECHI: We are trying to convince the local government by our own effort. For instance, we are trying to see local government or other government officials asking them to issue approval for boring the ground and so forth but we have not heard anything positive from them for more than one year.

Prof. PIERANTONI: I would just like to make a comment about the difficulty of siting in Italy. We had a lot of difficulty, not only for nuclear power stations but also for coil high power stations, and two weeks ago the Government decided to give about 2000 lire per electrical kilowatt installed for local authorities in order to reduce the problem. This is about $ 3 per every kilowatt installed and it seems that it works. Usually local authorities, in Italy at least, have a lot of expenditure due to roads which are destroyed by heavy trucks and so on. So this is just a contribution made by ENEL to local authorities in order to compensate them for special difficulties they must face during the construction of power stations. This means about 2 billion lire each large plant. We usually build from 2 to 4 large plants in each site. This means from 4 to 8 billion lire for each site and we have seen some difference in the local government behaviour.

Dr. TOMABECHI: Our Government has set up a law to give a special subsidy to local government last October, similar to that in Italy. It may take time before we can see some results of this kind of special subsidies given by the Government.

Dr. WENSCH: I would like to make some comments on siting in the United States. There was a very interesting article in a recent journal published by the Massachusetts Institute of Technology. The senior author's name is Bupp. This article reviewed the light-water reactor experience in the United States and the cost of plants on a capacity unit cost basis and normalized these costs to constant dollars. He then showed that by bringing in experience the unit cost should go down and the time for licencing should be reduced. But this wasn't happening at all. Indeed, the unit costs are going up, the licencing schedule is getting longer, and so he said let us examine the responsible factors. What is causing this?. - it is contrary to nature ! It turns out that the greatest factor influencing these two items is a result of environmentalists' pressures being put on the various agencies to make licencing longer, examining in great detail the reactor and its safety analysis, environmental report, and having public hearings and other relevant activities. The cost for doing this is significant - it adds to about two additional years in the United States.

Mr. VAUTREY: I don't mind saying a few words on what is going on in France. With regard to the Electricité de France's siting programme for the coming two years, it consists of 6 plants of about 1000 megawatts per year. As far as choice of site is concerned, for the last few months we have entered a somewhat difficult period because this acceleration in the nuclear programme has obviously excited discussion on the subject and there will be a debate in parliament soon on nuclear policy and the rhythm at which nuclear plants are being built. There are a number of people openly opposing or discussing this programme. With regard to siting, the local authorities, the municipal councils as they are called in France, were consulted by the Government recently and it appeared that according to the place in question, the response would be very different. In certain villages the mayors organized referendums. I remember quite recently, north of Normandy, there was a referendum in the department La Manche, very near the Channel, and the result was distinctly positive and a referendum held at the same time in another department in Landes, which is in south-west France on the Atlantic coast, gave a very negative result. In France there is no direct system of subsidies to local authorities, such as Prof. Pierantoni mentioned, but there is a system of taxes which does allow local authorities to profit financially from the presence of nuclear plants. Doubtless, this is an element contributing positively at least for a number of local authorities. It has appeared that in those places, localities which are situated in the neighbourhood of existing Electricité de France plants in the Loire Valley, since the presence of the nuclear plants have become so rich, these localities don't know what to do with their money. There is one very small village where they have built 3 big swimming pools for tourists. It was a little rural, backward village and now it is a place where the embellishments, improvements and modernization of the village are quite remarkable. The presence of nuclear plants has brought an enormous amount of money into the municipal coffers that they don't even know what to do with all the money they have got, as I already said.

Dr. SMITH: We will ask other countries to make a brief comment on the siting problem.

Dr. ENGELMANN: The siting problem in Germany is very serious in spite of the fact that the local authorities have the same profit by a tax system and the local authorities normally do not object. But, there is a group of professionals going from proposed site to proposed site, mobilizing the population and fighting against any installation, be it a nuclear power station or another power station, or refinery, or whatever it will be. They are fighting and telling the people that industrialization of their region will have a negative effect on their living. So, in spite of site approvals in one case the utility could not start building power plants. Since last fall people are occupying the site and just preventing building by their physical presence, but this will have to stop.

Dr. KRASNOJAROV: It seems to me that we don't have the same problems as you have. My opinion is that people in our country favour industrialization because near such a new power station or other plant will be new buildings, new houses, a place to work, and modern life, instead of as it was previously. Therefore, everybody welcomes this as a rule.

Dr. SMITH: In the UK we don't have difficulty in siting problems for fast reactors. We haven't one to site at the moment. In general, as I have said before, there is a great deal of difficulty in getting past the licensing authorities. I would guess, and it is sheer guesswork, that if we get a safety case accepted then we shall find a site. On the SGHWR there have been two sites which I think I named in my report. I have had no word of any opposition to those sites from any local body or any organization, and so long may it stay that way. So, I think we are relatively well off. I think our experience of the effect of nuclear power stations has been good. I don't think our local authorities profit very much directly, but certainly I know, for example, that where the CEGB is building at Dinorwic a pump storage scheme, the CEGB pays for widening roads which would normally be a responsibility of the local authorities.