UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

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UNITED STATES OF AMERICA

ATOMIC ENERGY COMMISSION

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In the matter of: :

CONSOLIDATED EDISON COMPANY OF NEW YORK, INC. : DOCKET NO. 50-247

(Indian Point Station, Unit No. 2) :

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Ground Floor Hearing Room
8120 Woodmont Avenue
Bethesda, Maryland

Wednesday, 25 April 1973

Hearing in the above-entitled matter was reconvened, pursuant to adjournment, at 10 a.m.

BEFORE:

SAMUEL W. JENSCH, Esq., Chairman,
Atomic Safety and Licensing Board.

DR. JOHN C. GEYER, Member.

MR. R. B. BRIGGS, Member.

APPEARANCES:

(As heretofore noted.)

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CONTENTS

EXHIBITS

("Fuel Densification - Indian Point Nuclear Generating Station Unit No. 2, January 1973," and "Addendum to Fuel Densification - Indian Point Nuclear Generating Station Unit No. 2,"omitted from hearing record for 24 April 1973, is included herein following page 11,262.)

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P R O C E E D I N G S

CHAIRMAN JENSCH: Please come to order.

Is the Applicant ready to proceed with cross-examination?

MR. TROSTEN: I am, Mr. Chairman.

MR. KARMAN: Mr. Chairman, possibly at this time it might be better if we could have Mr. Kniel of the Regulatory Staff respond to the questions which were propounded by Mr. Briggs yesterday afternoon, and possibly we can terminate that radiological aspect and Mr. Kniel can be excused.

CHAIRMAN JENSCH: Is Mr. Kniel here?

MR. KARMAN: Yes.

CHAIRMAN JENSCH: Any objection?

Hearing no objection, we will proceed.

Have you been sworn in this proceeding?

Whereupon,

KARL KNIEL

was called as a witness on behalf of the Regulatory Staff and, having been first duly sworn, was examined and testified as follows:

THE WITNESS: Yes, I received the questions propounded by Mr. Briggs from Mr. Karman. I will try to restate the question and answer, each one separately.

CHAIRMAN JENSCH: Proceed.

EXAMINATION

THE WITNESS: Mr. Briggs asked about a reference to any definition of prepressurized fuel in Section 5.3. Section 5.3 only requires that the reloaded fuel be similar in design to the initial core.

He is correct. There is no reference to any requirement for prepressurization specifically using those words. The intention of the "similar in design" is to cover the area of prepressurization. In my opinion, unpressurized fuel would not be similar in design; and the "similar in design" covers, I think, all of the aspects of the fuel design that are required to be met rather than just prepressurization, that part.

For example, a fuel could be prepressurized at somewhat lower pressure and not meet the requirement for cladding collapse during any cycle.

MR. BRIGGS: Well, how is that taken care of then? It seems to me "similar" may imply that it has to be prepressurized, but it doesn't put any limits on prepressurization, doesn't put limits on pellet density, anything else.

"Similar" is a very broad adjective to use there.

THE WITNESS: That is correct. We don't have any rigid specifications on the core itself.

The -- years ago the Regulatory Staff required technical specifications that put rigid design limits on every item that was safety-related in the plant. It was prior to 1966. It turned out that any -- almost any change that was made by the Applicant required a tech spec change; and any small nut or bolt, or anything, usually required some kind of a tech spec change.

To get away from that aspect of it, and to make the tech specs more reflect the boundary of operating conditions, we pretty much minimized any kind of specific input regarding to -- with regard to what the design should be.

In other words, we don't have specific information on -- what the accumulator is or what the safety injection pumps are or the -- what remains in Section 5 is a more general description of what the plant is.

MR. BRIGGS: Section 5 does mention the enrichment that must be used, does it not?

THE WITNESS: Beg pardon?

MR. BRIGGS: Section 5 does mention the enrichment?

THE WITNESS: Yes, sir. There are numbers and limits mentioned in Section 5.

MR. BRIGGS: Well, it just seemed that the pre-pressurization was such an important requirement, and the amount of prepressurization, since it does determine to some extent at least the lifetime which can be allowed for the fuel and other conditions, that this would be an appropriate condition to put into the tech specs; but you explained why you haven't done it.

THE WITNESS: Yes, I think it is -- certainly wouldn't have any objection to putting it into pre-pressurization in this section.

CHAIRMAN JENSCH: You wouldn't have any?

THE WITNESS: No.

CHAIRMAN JENSCH: Excuse me for interrupting, but could you suggest to the Board what specifics you would include if that phase of it were to be included in your technical specifications, both as to pellet densification and the amount of pressure and whatever aspects of specifics would be important?

THE WITNESS: Well, the only thing we would add would be that fuel should be prepressurized. I wouldn't want to add a particular pressure or any particular density or anything like that. Those variables can be evaluated in combination with various combinations; and you wouldn't want to care to specify that at the moment.

CHAIRMAN JENSCH: You say at the moment. Why not?

THE WITNESS: Well, it is -- again restricting the plant fuel in the future more tightly than we feel is necessary to insure public safety.

CHAIRMAN JENSCH: In other words, you assume that the fuel densification problem is fully and finally settled, and you need no further research in that regard? Is that your suggestion?

THE WITNESS: No, I don't suggest that.

CHAIRMAN JENSCH: Well, you say it is not necessary at the moment. Do I infer correctly that you don't know what the full specifics are of the densification?

THE WITNESS: We evaluated proposed fuel design; and we feel that that is satisfactory. We don't feel that any specific identification of the fuel in great detail is required in ascension.

CHAIRMAN JENSCH: You give us your conclusion in that regard. I am trying to find out what the specifics are for your conclusion. Is it because your research is not yet completed on fuel densification? Or it is concluded and you arrived at the conclusion and those specifics should be included in the technical specifications?

Which is it?

THE WITNESS: We feel the design specifications for the plant should not be included in the technical specifications. This is what I tried to express in providing you with a history of what the technical specifications are.

Section 5 is the vestigial remains of the technical specifications as they were once written, which involves all the design features of the plant.

The technical specifications used to include in essence the FSAR and the design of the plant; and the technical specifications now are directed principally at the way in which the plant is operated within limits; and to define those limits.

CHAIRMAN JENSCH: Well, I understand your position in that regard, but as Mr. Briggs points out, you have left fuel enrichment in there. This fuel densification, or problem, is of recent origin, and whether that is of sufficient reason to modify your trend toward elimination of some specifics about core design, I don't know. You said that you are changing -- the trend is one way. Now you have learned of this obstacle of Ginna fuel and the question is a matter of policy. Shouldn't the specifics be in there so that there are some parameters right at the outset regarding the kind of fuel?

Can you give us any comments in that regard?

THE WITNESS: Well, you mentioned the enrichment is left in as a specific. It is left in, but only as a limit.

In other words, it shows the reload fuel be no more than 3.4 percent. It doesn't specify what the enrichment shall be in each region or each reload region.

MR. BRIGGS: Well, in the same sense, one might indicate what the limits on pressurization are, but I guess you have to add to them. You have to have an upper limit and a lower limit in that case, do you not?

THE WITNESS: That's correct, and it wouldn't mean too much without getting information on what the densities are also. It would be kind of a parametric values of density and fuel clad thickness and modality. There would be a lot of parameters that would be covered in any meaningful specification.

MR. BRIGGS: It seems to me the Staff has said in the past, at least in some places, that one should take care of the fuel densification problem in the tech specs of -- for each particular reactor; and it just appears that -- at least appeared to us -- that the -- really it wasn't taken care of in these tech specs except -- well, even in the -- it didn't mention that the fuel was prepressurized in the spec; and that possibly if one -- if you say, well, we want to refer it to the design as it is, that one ought to put in something more specific than that the fuel should be similar to the -- in designing the fuel that is used in the plant at the present time.

I don't see any objection to the Applicant having to come in a year from now, or two or three years from now to change the pressure limits, if you wish, or to indicate that the design is different because if the design were different, I suppose you'd have to change possibly peaking

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factors; and you'd have to change the relationship between power and peaking factor that could be used for different axial offsets and things like this.

It just seems that the tech specs should reflect better the consideration that went into accounting for the fuel densification problem in this particular reactor. That, I think, was a major concern.

(Mr. Karman and the witness conferring.)

(The Board conferring.)

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CHAIRMAN JENSCH: I don't know how long this session of the hearing will go, but I wonder if the Staff would have an opportunity to give some further consideration to the suggestions of the Board this morning; and if parameters were to be included in reference to prepressurized fuel, what would be the suggestions from the Staff with respect to parameters? Would you have an opportunity to give some thought to that in the next couple of days and perhaps come back and give us a report in that regard?

THE WITNESS: Yes.

CHAIRMAN JENSCH: My thought is whatever the Staff thought should be included for prepressurized fuel, what would be the specifics thereof?

Would you do that?

THE WITNESS: Yes, sir.

CHAIRMAN JENSCH: Very well. Thank you.

MR. BRIGGS: I believe there was another question.

THE WITNESS: Yes.

You had -- the second question was with reference to the specifics requiring 21,000 effective full power hour limit on the Region 1 fuel.

The reason we have this particular specification in there was that you may recall that the core consists of three regions: Region 1 has 65 assemblies and the other two have 64 assemblies.

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The Applicant is considering using some of the Region 1 assemblies in subsequent cycles because there is an odd requirement. It is not clear, perfectly clear, that 21,000 effective full power hours would cover two cycles; so that in order to restrict the Applicant to an exposure of 21,000 effective full power hours in any single assemblies that are used in subsequent cycles, you put in the specifics.

MR. BRIGGS: And if the Applicant changes fuel, then he's going to have to come in and he may want to come in and ask for a change in this number also, is that correct?

THE WITNESS: He may want to ask, provide some basis for greater exposure per individual assemblies in Region 1, which he reinserts in subsequent cycles.

MR. BRIGGS: Is there any reason why one shouldn't have said that the fuel in the reactor, including Regions 2 and 3, shouldn't be radiated to more than 21,000 megawatt days -- 21,000 effective full power hours?

THE WITNESS: Yes, there is.

The Applicant's valuation for clad flattening for Region 2 was, I believe, 29,000 effective full power hours; and this more than covers any anticipated exposure for two cycles; and the Applicant's evaluation for Region 3 was greater than three effective full power hours.

MR. BRIGGS: Why is it that the --

THE WITNESS: I beg your pardon. That should read

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three cycles.

MR. BRIGGS: Why is it that the fuel in which the exposure is limited -- what requires that limit?

THE WITNESS: Well, what requires it is that the full exposure for two cycles might possibly exceed 21,000 effective full power hours.

MR. BRIGGS: I am sorry. Is it low density of the pellets or some other factor?

THE WITNESS: Well, it is a combination of factors which lead to the prediction that the fuel might flatten in 21,000 effective full power hour exposure.

MR. BRIGGS: I believe there was one other question

THE WITNESS: Yes, sir.

The last question had to do with the Section 2.1 of the technical specifications; and Figures 2.1-1 and Figures 2.1-2.

These figures describe the safety limits for operation of four and three loops for Indian Point Unit 2; and the bases indicated that these limits were derived with using F sub Q of 3.12 and -- I guess it is. If sub delta H of --

MR. BRIGGS: 1.75.

THE WITNESS: Correct. 1.75.

Both of these numbers have been subsequently changed to allow for operation with an evaluated fuel

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densification model.

It turns out that if you recalculate these curves using the revised peaking factors, you get less conservative numbers.

In other words, the safety limits would not be as severe using the new peaking factors; so the Applicant has chosen to remain with the curves as presently depicted, calculate it with the old peaking factors, and these are actually more conservative, in other words more restrictive on the safety limits.

So the Staff has no objection to that.

MR. BRIGGS: I don't believe there were any other questions that I had.

Thank you.

CHAIRMAN JENSCH: Thank you.

We will hear further from you later.

Will you proceed?

Anything further, Regulatory Staff counsel?

MR. KARMAN: Not with respect to radiological matters, Mr. Chairman.

CHAIRMAN JENSCH: Very well. Applicant ready to proceed?

MR. TROSTEN: Yes, Mr. Chairman.

(Witness excused.)

MR. TROSTEN: Mr. Clark --

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Whereupon,

JOHN R. CLARK

was recalled as a witness on behalf of the Hudson River Fishermen's Association, and, having been previously duly sworn, was examined and testified further as follows:

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FURTHER CROSS-EXAMINATION

BY MR. TROSTEN:

Q Mr. Clark, I would like to question you with regard to your testimony of April 23, 1973.

Have you been responsible for the operation of a hatchery?

A No.

Q Or for the stocking of fish?

A No.

Q Have you personally participated in either of these operations?

A Not on any large scale.

Q When you say not on any large scale, were you referring to your testimony on Transcript Page 11,050, in which you say, "I have investigated the attempt at stocking -- excuse me. Page 11,0501, where you say, "I was involved earlier, in fact in 1969, with an investigation of this particular stocking"?

You were referring to the stocking of the Choctawatchee River?

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A Yes.

CHAIRMAN JENSCH: You want to turn to your transcript first to get the context of the discussion?

BY MR. TROSTEN:

Q Was the answer to my question yes, that is what you were referring to?

CHAIRMAN JENSCH: Let him review the transcript first, please.

MR. TROSTEN: Yes.

THE WITNESS: Yes, that and experience that I had at Sandy Hook with Moncks Corner stock, trying to raise the fish in an aquarium.

BY MR. TROSTEN:

Q I am sorry. I didn't hear.

A Trying to raise the fish in the laboratory aquarium.

Q I am sorry. I didn't hear the last answer.

A Laboratory aquariums.

Q I gather at Sandy Hook when you were at the Sandy Hook laboratory, they attempted to raise the Moncks Corner yolk sac larvae to fingerlings, is that what happened?

A Yes, I went to Moncks Corner myself and investigated the hatchery and then had some of the -- of the -- of the larva stages returned to the Sandy Hook laboratory for experimentation purposes to see if we could be successful in

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raising them. We did attempt to stock the small lake in New Jersey, all without success.

Q What was the nature of your investigation of the Panama City, Florida, stocking that you refer to on page 11,051? What did you personally do?

A I was sent down there to evaluate the scientific program of the Panama City laboratory, which included several components at that time, one of which was the stocking program for Choctawatchee Bay.

Q How long were you down in Florida?

A I was there on two occasions for a total of perhaps two weeks altogether.

Q Mr. Clark, on page 3 of your April 23 testimony, there is a statement in the second sentence as follows: "The requirements for Hudson stocking would be 10 million fish or more."

What are the assumptions upon which you base your estimate that the requirements for Hudson stocking could be 10 million fish or more?

A Based upon the -- that is a nice, round number, based upon the kinds of estimates that we have been exchanging in the last few months.

Q Is there nothing more specific than that?

A Excuse me.

I haven't seen any figure produced which is an

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exact figure at this point as to what the stocking requirement would be.

Q With respect to your statement on the same page, "Nor does this appear practical in dealing with such large numbers of breeders (5,000 fish of about 10 pounds average)," what are the assumptions upon which you base your estimate that there would be a requirement of 5000 fish of about 10 pounds average?

A Excuse me. I have to refer to something.

Q Yes.

(Pause.)

THE WITNESS: This is -- I was just trying to check to make sure I have the right thing. This is partly based upon Dr. Goodyear's estimates in his testimony of April 23, the same day.

BY MR. TROSTEN:

Q What else is it based on?

A It is based on scaling that down a little bit.

Q Scaling it down a little bit?

A Yes. He was of the opinion that 15 million fish would be required and that 7000 or 8000 stock would also be required. I was trying to just take a conservative shot at that.

Q Would you point me to the particular page?

A Certainly. I think it is page 11.

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Yes, where it says in order to produce 15 million young-of-the-year striped bass, and so forth, and goes over to page 12 where he ends the paragraph by saying it is likely to require an excess of 7000 to 8000 fish.

Q Did you discuss this with Dr. Goodyear prior to preparing your testimony?

A Yes.

Q Mr. Clark, on page 2 of your April 23 testimony, you state the following: "For example, when Mr. Bibko was doing experiments for the Applicant on striped bass eggs and larvae last year, he was completely unsuccessful in getting material from the Hudson." Did you personally speak to Mr. Bibko and ask him about his attempts to collect striped bass, female fish, in the Hudson River?

A Yes, I tried to secure assistance for him in doing this from some of the fishermen on the river.

Q No. That wasn't my question. I asked you whether you had personally spoken to Mr. Bibko about his attempts to secure striped bass females in the Hudson River.

A Yes, I did that. And in -- during our conversations about this, I attempted to get him some help in finding places where he might be able to catch fish in the river and referred him to some fishermen's organizations. We didn't succeed in helping him.

Q When did you speak to Mr. Bibko?

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A This would have been in late May.

Q Of 1972?

A 1972, or very early June, perhaps. In that period of time.

Q Have you spoken to Mr. Bibko about his attempts to secure striped bass since May or June of 1972?

A Yes. After he came back from Moncks Corner where he worked doing experiments with temperature and pressure and so forth, I discussed the whole situation -- problem with him at a little laboratory he had set up on the reservoir.

Q When was that?

A I don't remember the specific date.

Q Was it last summer?

A Yes, last summer. I can't remember whether it was late June or early July.

Q Now is it your understanding on the basis of your communications with Mr. Bibko that he did not secure any female striped bass?

A That he wasn't successful in getting any quantity of material sufficient to do the experiments they wanted to do.

Q Let me see if I understand this.

When you say getting material from the Hudson, you are not suggesting, are you, that he did not capture female striped bass, are you?

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Q Mr. Clark, could you describe to me the efforts that you made to assist Mr. Bibko in securing female striped bass?

A Yes. I contacted Bob Boyle, Robert H. Boyle at the Hudson River Fishermen's Association, my client in this case, and attempted to have them find a gill net fisherman or gill net fishermen or other water men along the river who had the scale of knowledge and experience in methods of capture, to find a way to collect several dozen fish at the -- as near as possible to the time of natural ovulation of the eggs so he could be successful in mass culture.

The extent of my effort was to ask Mr. Boyle to encourage him to do the best he could do and to find -- and find out how that was going, whether in fact he had been able to find the fish, because there seemed to be a great urgency about it.

Q Now is it your understanding that in previous years female striped bass have been captured in the Hudson River and have been spawned?

CHAIRMAN JENSCH: Mr. Counsel, would you move your microphone closer in?

MR. TROSTEN: I am sorry.

CHAIRMAN JENSCH: Okay.

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A I don't know what the actual number of fish was that he captured or attempted to modulate or spawn.

Our conversation was along the lines of the adequacy of any attempt he could make to capture certain spawners for his purposes, his experimental purposes. One thing he told me he wanted to do was get sufficient fish which might mean several million fry that he could see them in the river and determine what happened to them in the region of the plant.

Q Is it your understanding that he did successfully capture female striped bass and spawn them?

A I think, as I recall, although we didn't talk much about this, he did get a few.

Q And that he spawned them?

A I think he spawned them. I am not sure. I am not sure on this. We only talked about the fact that it was not possible to get the quantity that he wanted in spite of the efforts we tried to give him.

What his actual score was, I don't know.

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(Pause.)

BY MR. TROSTEN:

Q Is it your understanding in previous years female striped bass have been captured in the Hudson River and that these bass have been spawned?

A I think it might be easier if you would cite the case or cases you are talking about.

Q I was simply asking you a question on the basis of your knowledge of the situation.

A I know of no one who in any year has been able to collect a large number of spawning females and been able to successfully hatch out large quantities of them. I think that I would know if this had happened.

It could easily have escaped my attention if somebody had made an abortive attempt or number of abortive attempts.

Q You mentioned large numbers. I simply wanted -- I wanted to ask you, have there been female striped bass captured and have they been spawned apart from the numbers of females that have been captured or the amount of eggs that have been removed? Do you know of instances where this has occurred?

A I am not on a well informed -- if there have been such efforts, I am not well informed on them. I know of the Rhode Island people who did get some fish there and did go back to Rhode Island and were able to arrange a few for a

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while; and I -- if my memory serves me right, this would have been Dr. Saila's group up there.

I am not specifically familiar with the success they got. There was a fellow who had a kind of floating lab down in the Narragansett Bay area some place where they were trying to raise these fish. They had some smell of success, but it was only a limited number of fish, as I remember it.

Q You are not familiar with anyone operating on the Hudson River who has done this?

(Pause.)

A My memory produces nothing.

CHAIRMAN JENSCH: The premise of your question indicated that there was.

Can you identify that, perhaps?

MR. TROSTEN: I cannot at this point. I would have to confer with our witnesses, Mr. Chairman, to see if I can identify a specific premise. If this becomes important --

CHAIRMAN JENSCH: I understood the premise of your question was the existence of such an activity; and if it is not such an activity, then I presume the premise of the question is unfounded.

MR. TROSTEN: Well, the premise of my question was -- I am not really sure there was a premise. My question was, was he aware of such activity; and I understood from the witness' answer that he was familiar with a Rhode Island group that had done this, but that he was not familiar with a group operating on the Hudson River.

CHAIRMAN JENSCH: I heard the answer. The problem I had was I inferred there was a foundation for your question and I wondered if you could establish it.

You are asking him something -- aren't you familiar that this is happening? If it isn't happening, of course, it is an unfair question.

MR. TROSTEN: I will connect it up, Mr. Chairman.

CHAIRMAN JENSCH: Very well.

(Pause.)

MR. TROSTEN: I have no further questions for Mr. Clark, Mr. Chairman.

CHAIRMAN JENSCH: Does the Regulatory Staff have any questions?

MR. KARMAN: No questions, Mr. Chairman.

CHAIRMAN JENSCH: Any redirect?

MR. MACBETH: Could I have just a moment?

CHAIRMAN JENSCH: Yes.

(Pause.)

MR. MACBETH: I have just one question.

CHAIRMAN JENSCH: Proceed.

FURTHER DIRECT EXAMINATION

BY MR. MACBETH:

Q Mr. Clark, is there a typographical error on the last full line of page 5 of your testimony?

A Yes.

There is a misspelling there which could interfere with the meaning of the sentence and that is the word "plan" should be changed to "plant."

CHAIRMAN JENSCH: Would you speak a little louder, please?

THE WITNESS: Next to last line, page 5, the word "plan" should read "plant."

CHAIRMAN JENSCH: Thank you.

THE WITNESS: Thank you.

MR. TROSTEN: Mr. Chairman, I have an additional question I would like to pose.

CHAIRMAN JENSCH: Sure.

FURTHER CROSS-EXAMINATION

BY MR. TROSTEN:

Q With regard to the attempts to capture and spawn striped bass or -- I guess -- which were made by the Rhode Island grop, where did these Rhode Island -- the fish -- come from that were taken by this group of people from Rhode Island?

Which group of people was that, by the way? I am not too clear to which group you were referring.

A There are two parties of interest in that area. One group is a private group with Bob Pond, who is the president of the Adam Flood Company up in North Adam Borough, Massachusetts. They are on the Narragansett Bay. He has gone to the river -- he is there today, as a matter of fact -- for a number of years in the past and attempted to spawn and raise, back in the Narragansett Bay area, the fish that he has taken from eggs that he has taken in the Nanticoke.

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This is not the group I was referring to. I was referring to the -- in the University of Rhode Island, in their marine research department, there is a Professor Salem whose student or assistant at one time several years ago, I think 1970, and/or 1969, or maybe since then, have gotten material from the Hudson River and -- to use in experimental rearing activities at the University of Rhode Island.

Q When you say got material, what do you mean? I guess I was having the same difficulty with that phrase as it was used in your testimony.

A Now here I am going back a number of years. I think they did both. They attempted to secure striped bass, females, in the spring in the area of Constitution Island, in the Hudson, and as I recall, had no success.

Later they took fingerlings from the river and were able to rear them with some degree of success. A small number. That is about the best I can dredge out of my memory right now.

MR. TROSTEN: That concludes my cross-examination of Mr. Clark.

CHAIRMAN JENSCH: Very well. Mr. Briggs has some questions of Mr. Clark.

MR. BRIGGS: The first question has to do with what fraction of the females that are aged four years and older are spawners. Are all the females age four years and older spawners in a particular year, or is it only a small fraction, a large fraction?

THE WITNESS: As I recall it, and I don't have my papers with me this morning, it is about 75 percent you would expect to be maturing and spawning of four year old females; and nearly 100 percent of the five year olds.

MR. BRIGGS: All right. So five years and older, 100 percent of the females are considered to be spawners?

THE WITNESS: Yes, sir.

MR. BRIGGS: Taking the data one has in the previous testimony, the number of average number of eggs per female and the number of mates that are required in the Hudson, you can in fact calculate the number of females?

I haven't done that calculation, but I made some values.

THE WITNESS: Yes, you can do that. The only problem that you have to be aware of is -- in doing it, is that there is something less than 100 percent viability in eggs that are spawned out of the female.

MR. BRIGGS: Yes.

THE WITNESS: The hatchery method has percentages that should be lower than in the natural system.

MR. BRIGGS: In your testimony of October 30th, 1972, the final testimony, you give a derivation of population estimate based on the Carlson-McCann data of 1966, and '67, I believe.

As I read the testimony, you start out with about 1.3 billion viable fertilized eggs and indicate that this is as good an estimate as is now possible, but probably a low estimate; and you end up with an estimated population at the end of 16 weeks of 2.4 million juveniles. That, I believe, is on old page 20, and the following pages.

Later juveniles, an estimate of 1.8 million at the end of the 34th week.

If that's the entire population of stage five juveniles in the Hudson at the end of the 34th week, how does one get that you must place 10 million or more fish in the Hudson to compensate for this?

THE WITNESS: I have unfortunately not made it clear enough there that those are relative numbers, dependent entirely upon -- for any extrapolation to absolute numbers.

You have to have a figure about the year efficiency, efficiency you are sampling.

Here we have recently, it seems like -- most of us thinking about this are thinking that the population in that river is considerably higher than those relative numbers would indicate; and perhaps by an order of magnitude.

MR. BRIGGS: Well, we have talked some about the efficiency of the gear. I don't believe I have heard numbers. I have heard numbers like 10 percent, including the efficiency of the gear. Is this a reasonable number?

THE WITNESS: That would -- I think I testified -- I testified earlier this year that my experience and knowledge about the selectivity of otter trawls would indicate that they might be only 10 percent efficient in censusing the population of young fish in the river; and that I could easily imagine that figure, instead of around 2 million, being around 20 million. That high, if you try to get the absolute numbers from those relative numbers.

MR. BRIGGS: So you think the population of the juveniles could be as high as 20 million then?

This goes back to the question about the impingement on the screens. This would make the impingement fraction on the screens much less than you had indicated in your previous testimony, is that right?

THE WITNESS: Yes, exactly. We are on the horns of a dilemma there.

MR. BRIGGS: Yes, I know.

Let's see.

There is a statement on page 5 of the April 23rd testimony. It says at a minimum,with extraordinary success, it could take more than half the expected life of a plant to determine whether stocking was working.

If it takes so long to determine whether the stocking is working, does this mean that the plant, under that circumstance, the plant wouldn't be having much effect on the population of the striped bass in the river?

THE WITNESS: In relation to natural background fluctuations?

MR. BRIGGS: Well, possibly in relation to the natural background fluctuations, but if one can't determine an effect of a measure that's being used to alleviate a condition, it seems that normally this means that the disturbing factor, like the plant, isn't having much effect on the condition.

In other words, if you can't tell whether your stocking program is doing any good, then it -- I wonder whether one can make the counterconclusion that under that circumstance the plant just isn't having much effect that you can tell about? Is that a bad assumption?

THE WITNESS: It doesn't follow, from my thinking, about this, what I had in mind when I made that statement. My thinking was that you would not know with any degree of certainty that substituting artificial for natural spawning, whether it was working until you have been able to cycle back through, until you had a several returned generations of fish to the river; and then you could determine whether ultimately you get to the point of the population being restored and the breeding stock being restored in the river.

MR. BRIGGS: I think that's helpful to understand what you were thinking about at the time.

I got the impression from some of the Applicant's previous testimony, Dr. Stevens' testimony, that the efficiency of raising fry in a hatchery from eggs was -- could be expected to be considerably greater than the efficiency in the river; and I believe you here indicate that your conclusion is different from that.

Are there factors that would cause the efficiency to be lower in the natural environment than in the hatchery, or higher in the natural environment than in the hatchery that you can see?

THE WITNESS: I think that you could make an effective argument for either case depending on your point of view and your experience and so on. You could make probably a fairly good case, very good point of view. I think that -- I don't see any way of resolving that particular point short of actually going ahead and trying it on a full scale basis, because there is so much theory involved in all of this, and so much hypothesis, and so much hope, rather than solid experience, that I don't know how you resolve that.

It could be argued either way.

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MR. BRIGGS: In your testimony of October 30, 1972, you start out with 1.3 billion viable fertilized eggs, and you end up with 1.8 million -- or let's say 2.4 million fry at the end of 16 weeks.

Although these are relative numbers, don't they give you an efficiency for the Hudson River?

THE WITNESS: Yes, they would be interpreted that way.

MR. BRIGGS: So one could take the efficiency of the Hudson River as being like two tenths of a percent and going from viable eggs, not from the original eggs, but from fertilized eggs?

THE WITNESS: Yes.

MR. BRIGGS: I don't believe I have any further questions.

Thank you.

DR. GEYER: This may not be in the form of a question, but a general commentary on the situation we are faced with here. The problem is to be sure what is -- when is -- what is happening is due to the lack of the stocking of fish, or the effect of the plant, or reduction in pollution, or any of a number of causes that could be operated in this system.

We need some discussion, somewhere along the line, before the case closes, it seems to me, of how you are going to tell what you are doing and be sure that the conclusion you draw is the correct one rather than just a possible one.

Now, I don't know whether you wish to speak to this problem or not, but it certainly is one that we have to deal with.

THE WITNESS: One thing that occurs to me and has all the way through, when I thought about hatching, is that of all the experiences that I have read about, and what I have learned about hatching of marine estuarian fishes shows a trail of unproved efforts at hatching and the failure of closing of hatcheries, all along down the coast, from Gloucester, Massachusetts, down the Chesapeake Bay, to the coast and San Francisco Bay area. Many of these had to do with striped bass.

In San Francisco, in the early part of this century, it was tried and dropped. In the Chesapeake Bay, it was tried again and again dropped. In the Hudson River, shad hatching was tried and stopped. Lobsters in Massachusetts, they are still doing it.

Even today with modern methods, they have no idea whether the lobsters they are putting in are contributing to the stock. Winter flounder, she used to hatch them by the truckloads at Woods Hole and bring them down to Long Island Sound. They used to hatch cod; they hatched just about everything they could manage with the techniques to hatch them, return to estuarian coastal waters. There is no indication any place along the line that this has been useful in improving the populations; and therefore, all these marine fish hatcheries that I know of have gone out of existence.

Down in North Carolina, they are still hatching and returning striped bass into the system, but people I have talked to down there like Don Baker tell me that they, for instance, have been stocking a river down there with striped bass; and all the time they have been stocking over about the last five years, the striped bass fishery has gone down, down, down and finally, right in the face of all the heavy stocking in that river of striped bass, the fisheries have just collapsed; and today it doesn't exist in a significant number.

There is no place anywhere in the literature of these efforts where I have seen any kind of demonstrated success. I think about the Hudson River and say how then can we have any hope that we can prove that it would do -- if it were to go ahead, what hope do we have that in this one situation we would be able to measure its effect?

We haven't succeeded over the last several generations.

DR. GEYER: Thank you.

MR. BRIGGS: Just another question concerning your last discussion. You mentioned the collapse of the striped bass fishery in a river in North Carolina, was that it?

THE WITNESS: Yes.

MR. BRIGGS: What river was that?

THE WITNESS: I would have given the name of that if I could have remembered it. I can look that up and let you know.

MR. BRIGGS: Maybe you can tell me what the collapses -- what caused the collapse of the fishery. Is that known?

THE WITNESS: Dr. Baker was not sure of that fact that it was a pollution induced factor, either facilitation or some kind of toxic -- ingestion of some sort of toxic chemicals. I can supply you with a reference on this if it would be of value to you.

MR. BRIGGS: It would be helpful, yes.

CHAIRMAN JENSCH: Let me ask you a few questions if I may, especially in the light of the questions -- an answer you gave, perhaps, to Mr. Briggs. I think you commented on some testimony of Dr. Stevens. If I recall your answer correctly, you said well, you could make an effective argument either way about this; and the only way you could really tell was to put on a fullscale endeavor, I take it, on the Hudson River to see whether you could restock the Hudson River through a hatchery operation. Was that the substance of your answer?

THE WITNESS: Yes.

CHAIRMAN JENSCH: Now in view of this recent discussion, however, with Dr. Geyer, this recital of the several endeavors that have been undertaken from Gloucester, Massachusetts way down to the Nanticoke Bay, I take it you don't hold much confidence that a fullscale operation on the Hudson River would be fruitful, is that your suggestion?

THE WITNESS: I certainly wouldn't discount it. We have very, very able people now like Dr. Stevens who have advanced the practice of hatchery work as opposed to the success of field introduction; have developed and improved the methods of hatching these critters and getting them out of hatcheries. I certainly won't discount that it could work; but I am saying that you are operating with this idea: bring it out in a background of total negative evidence against estuarian or nearly total evidence against estuarian and marine success.

I would think that one would need to do a considerable amount of experimentation over a number of years and develop and advance the methods before one could offer this as the remedy to the entrainment kills in the Hudson estuary. It's a breath of hope, but to me it's certainly far from any thing that we could accept as a proven technique.

CHAIRMAN JENSCH: It's a hope springeth eternal aspect that keeps you striving to see if at sometime somewhere it might work. But so far you know of no actual data that suggests it will work, is that your thought?

THE WITNESS: That's my thought, yes, sir.

CHAIRMAN JENSCH: Now, you said it would have to be a full-scale hatchery operation. What do you mean by that? How many ponds and where would you have it? I mean, as I understand some of this testimony, the fish go up the river to some location which is more desirable than perhaps others where they spawn and then the fish come down the river. Where would you put the hatchery operation? Up in the spawning area or in the maturing area? Or catching area? Or how many ponds would you have and how far out in the river would you have it?

Aside from the moment of whether it's an adverse impact on the environment -- I mean any other place in the river.

Could you tell us what kind of a full-scale basis you were talking about?

THE WITNESS: It would go beyond my particular expertise to describe in detail the kind of hatchery used, but I can at lease discuss design requirements or performance requirements; and I would certainly think that in view of the hazards intendant to moving fish around, you gain greatly by having placed on the river, very near the natural area of greatest abundance of the larval stages of the fish.

Introduce them, these developed fish, into the river in some rational way which would spread them out through the water and give them the best opportunity immediately to succeed rather than if you just put a bunch of them in one place. Those fish there would immediately consume the available food and then they would be in trouble because they have to eat a considerable amount. You can probably expect three to four to five copepods

copepods

an hour for an active live, healthy larvae. They have to cover a lot of water. You have to have a plan for introducing them into the river that would relate to the location; so the location should be as near to the area of the salt water-fresh water interface as possible. The problem with this is depending upon the river outflow rate, discharge rate, the boundary may be ten miles down or ten miles up the river. You have that factor to contend with.

CHAIRMAN JENSCH: You are not suggesting you move the ponds up and down the river according to this salt water-fresh water interface, are you?

THE WITNESS: No, because if you did this all on barges, the number of barges to raise these fish to fingerling size would fill up a good deal of the length of that river. It would take a good deal of water -- of water capacity, surface area tanks, to raise these fish even at the very optimum rate of 8000 sub adults per acre.

You can see if you release 15 or 10 million fish, how many acres of land you are going to have to have. I haven't calculated it out, but quickly it seems to me it's many, many thousands of acres.

You could at one stage introduce them into a number of barges in the river, the finning stage, and take them in that fashion up or down the river.

Now this is discussing everything from introducing them as fry, tiny fry into the river, which you can do efficiently and effectively to growing them up to sub-adult size and introducing them when they are four or five inches long.

I would not yet be willing to make a judgment as to which would be most successful.

I think you will find that throughout the area of experience with striped bass stocking, that people would prefer nothing under two, and larger if possible, four or five inches, as a minimum size to be stocked.

CHAIRMAN JENSCH: Is it the substance of your presentation that you couldn't take -- I use the term "fish fry", or larvae, from another area like this Moncks Corner thing, and move it to Hudson River? Or is it your thought that you would have to -- if you are going to try to restock the Hudson, you have to restock it in the Hudson area?

THE WITNESS: Yes.

CHAIRMAN JENSCH: And then take watever you can develop out of some full-scale hatchery and put it into the Hudson River? Is that your thought?

THE WITNESS: I think you face difficulty, I won't be willing to say that it absolutely won't work; but I think that here again you are into a matter of great difficulty, because the evidence that seems to be coming out of what they are doing in Mobile Bay and Choctawatchee Bay would suggest that this is an ecological raise of fish which has a different set of adaptabilities and different set of environmental requirements, and a different pattern of habits of its behavior in life; and that if this is true, then Moncks Corner fish wouldn't be at all acceptable.

The other way around, the Hudson River wouldn't be at all acceptable for these fish because they are used to a whole different way of life. If they could live in that river and grow up successfully there, the evidence suggests they wouldn't go out in the ocean. Well, if they don't leave the Hudson River and go out in the ocean, we are going to be short four or five million fish a year out there according to our previous determinations.

Well, half of that.

I don't think it's any kind of acceptable objective to raise fish to stock in a rive that never go out, because these -- that seems to be the major function of the Hudson, as a nursery ground and spawning area. The active fishing goes on in the middle Atlantic area. These fish won't go out there; we are not going to help the situation by putting them in the Hudson.

CHAIRMAN JENSCH: Perhaps, I didn't detect it, but did Dr. Stevens project -- is he proposing to set up a fish hatchery on the Hudson River?

THE WITNESS: I have not seen a definite proposal. I haven't seen a single specific plan developed yet. Its' been talked about flying the fish or driving them somehow down to Florida and back, raising them down there and bringing them back. I think there is also an alternate proposal for a hatchery to be right on the river.

CHAIRMAN JENSCH: Well, I take it that with Dr. Stevens and his experience, you are willing to say he may be the first among thousands to have a successful hatchery. You don't think there is a likely probability, within -- I use the terms probability, numbers, whatever it is, how you would calculate it -- it is never going to work on the Hudson River?

THE WITNESS: Yes, the spectacular success Dr. Stevens has had has nothing to do with estuarian fish at all. It has to do with fresh water and reservoirs. His fish that they have taken other places have not taken hold any place else.

(The Board conferring.)

MR. KARMAN: Mr. Chairman, I have one question, if I may have Dr. Clark.

CHAIRMAN JENSCH: Sure.

FURTHER CROSS-EXAMINATION

BY MR. KARMAN:

Q Dr. Clark, you mentioned in your conversations with Dr. Baker, with reference to the collapse of a striped bass fishery in the North Carolina River.

A Yes.

Q Do you happen to know where the brood stock, with respect to that stocking, was obtained from?

A Well, I believe --

Q I beg your pardon.

A I think Weldon, the Weldon Hatchery in North Carolina.

Q Was it from the same river? Was the brood stock obtained from the same river that they were -- that you mentioned with respect to its collapse?

A No.

MR. KARMAN: Thank you.

CHAIRMAN JENSCH: Just one question.

On page five of your April 23rd statement, you changed the word "plan" to "plant"; and the plant to which you referred was Indian Point No. 2 plant, is that correct?

THE WITNESS: Of the plant we are discussing, Indian Point No. 2. Yes, sir.

CHAIRMAN JENSCH: Thank you.

Any further questions?

MR. TROSTEN: No, Mr. Chairman.

CHAIRMAN JENSCH: I infer from this further discussion that you were seeking information in your questions that you need not connect up. Whether there is any hatchery on the Hudson River.

I had the inference from your intonation of the words -- aren't you familiar with the hatchery or the hatchery on the Hudson River -- I assumed there was one. I know now there is none.

There being no further questions, thank you, Mr. Clark. You are excused.

(Witness excused.)

CHAIRMAN JENSCH: Before we proceed with another witness, I did receive a document, written statement, from the regulatory staff counsel. It is available for review by all parties. It suggests I call the office.

If you don't mind, I will call a recess. We will reconvene at 11:25.

(Recess.)

end

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CHAIRMAN JENSCH: Please come to order.

No smoking in the room, please.

Is the Applicant ready to proceed with further cross-examination?

MR. TROSTEN: We have no -- excuse me, Mr. Chairman.

We have a few -- very few questions, for Dr. Goodyear. However, I have been talking to Mr. Macbeth, and I informed him that we did have some redirect testimony for Dr. Stevens in light of the testimony that's been received; and I think the best thing to do would be for us to go ahead with our redirect testimony of Dr. Stevens and then we can -- I will cross-examine Dr. Goodyear later.

CHAIRMAN JENSCH: Very well, proceed.

Whereupon,

DR. ROBERT STEVENS

was recalled as a witness on behalf of the Applicant, and, having been previously duly sworn, was examined and testified further as follows:

MR. TROSTEN: Mr. Chairman, at this time I would like to direct to the Board and to the parties a document which has been prepared jointly by Dr. Lawler and Dr. Stevens which will be the subject of redirect testimony. This was prepared last night and this morning on the basis of the testimony that was received yesterday, Mr. Chairman.

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CHAIRMAN JENSCH: Proceed.

Dr. Stevens, having been previously sworn, you need not be sworn again.

xxxx

REDIRECT EXAMINATION

BY MR. TROSTEN:

Q Dr. Stevens, at the last hearing session, you testified concerning the recoveries of striped bass which have been stocked in the Choctawatchee Bay by Dr. Barkaloo.

Do you have any supplemental information from Dr. Barkaloo on this subject?

A Yes, I do. I called Dr. Barkaloo last night because of the several different figures that seem to be floating around; and he -- Dr. Barkaloo said that he did indeed have 200 recoveries preserved in alcohol or formalin, but that he also had 600 to 700 documented reports that fisherman had caught striped bass, that is they would go to a landing, and the landing operator would say that on Thursday, two fishermen caught three fish. They have got this documented.

That's -- also, they had a creel census there, I think on a short period of time in '71, where they caught a few fish at several points in the system; and by accepted statistical analysis, had projected this to a figure that 1200 fish were probably caught in a three-month period in 1971. As I say, it was a statistical analysis.

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He also stated --

CHAIRMAN JENSCH: May I interrupt?

What was the original figure? You said they caught a few fish. What did you start with to get the 1200? Three, five?

THE WITNESS: I think it was 16. When you talk over the phone like this, sometimes things get garbled.

He also said that of the ~~1.8~~ 1.6 million fingerlings stocked to date, that only about one-half were large enough to where they could be recaptured by the gear they use, either gill nets or by fishermen. In other words, there is a 15-inch size limit there. It doesn't mean a fisherman doesn't catch fish smaller than that, but he has to throw them back, which would affect the record.

At any rate, 1.6 million are not available for recapture, but at this time about half are, the others being too small to recapture.

CHAIRMAN JENSCH: Thank you.

Proceed.

THE WITNESS: When I called him the first time, I don't know what happened, but I got the impression that he said he had received about two percent returns on the number of fish that had been stocked and were available for capture; and he disclaimed saying this.

So my previous testimony of two percent is

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apparently in error; and that's essentially all I wish to say on that point.

MR. TROSTEN: Dr. Stevens was referring to his statement on transcript page 10,380, and was offering that correction to his previous testimony.

CHAIRMAN JENSCH: Thank you.

MR. TROSTEN: Mr. Chairman, at this time I would like to offer in evidence on behalf of the Applicant the document which I have just distributed to the Board and to the parties. It is entitled, "Comparison Between Number of Mature Females Required for Hatchery Replenishment Computed Using the Applicant's and Staff's Models." It is undated.

I will provide sufficient copies for the reporter, Mr. Chairman. This document will serve as a basis for further testimony by Dr. Stevens.

CHAIRMAN JENSCH: You request it be incorporated in the transcript as if orally read?

MR. TROSTEN: Yes, sir.

CHAIRMAN JENSCH: Any objection?

MR. KARMAN: No objection.

CHAIRMAN JENSCH: Hudson River Fishermen's Association?

MR. MACBETH: No objection.

CHAIRMAN JENSCH: The statement identified by Applicant's counsel may be physically incorporated in the

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transcript as if orally given and shall constitute evidence on behalf of the Applicant.

(The document follows.)

COMPARISON BETWEEN NUMBER OF MATURE FEMALES REQUIRED FOR HATCHERY REPLENISHMENT COMPUTED USING THE APPLICANT'S AND STAFF'S MODELS*

	Applicant's Model#		
Conditions	Impact Parameters Used by the Applicant	Impact Parameters Approx. Staff's Input Conditions	Staff's Model##
Indian Point Units 1 & 2	2 to 5	14	56 to 78# & 150 to 225**
Indian Point, Bowline & Roseton	9 to 10	28 to 29	102# to 117

*Assumptions
- Total number of eggs/female = 600,000
- Total number of viable eggs/female = 270,000
- Hatchery efficiency = 25% of viable eggs reach approximately 2" (end of the J_I stage)
- Replenishment occurs at the end of the J_I stage (59.5 days old).

These values represent estimates based upon hatchery replenishment estimates given in Table 7 of Applicant's Hatchery testimony of April 20, 1973.

The high end of the range corresponds to the set of conditions used by the Staff (1955 River conditions) to obtain the max. effect estimates, i.e., 5.2 million & 7.8 million juveniles corresponding to Indian Point & multiplant effects simulated by the Staff respectively.

**Corresponds to the 10 to 15 million juvenile bass given in the Staff's April 23, 1973 testimony.

Note
54% of Table values would be needed using the Staff's estimate of 500,000 viable eggs per female.

1mil

BY MR. TROSTEN:

Q Dr. Stevens, if you assume that a range of 150,000 to 15 million fingerlings at the end of the juvenile one stage, as used in Dr. Lawler's model -- that is fingerlings which are about two inches in length, were required for stocking, how many female striped bass would be needed for brood stock?

A I am a little confused about which is Bowline and Roseton.

Q The Bowline and Roseton plants, Dr. Stevens, are additional fossil fuel power plants which are going to be constructed -- which are constructed on the river and which have been analyzed in the Applicant's and Staff's testimony.

These plants provide additional impact on the river and they have been analyzed in Dr. Lawler's testimony.

A Oh, all right.

Well --

MR. MACBETH: I am afraid I have lost the question. Could we have it reread?

(The reporter read the record as requested.)

MR. TROSTEN: Mr. Chairman, may we have a brief recess?

THE WITNESS: I think I have it now.

CHAIRMAN JENSCH: Yes.

(Recess.)

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THE WITNESS: The projections are based on approximate products per 10-pound fish of about 67,000 juvenile, two-inch juvenile fish; so it would be about two fish for 150,000; and some 225 for 15 million.

CHAIRMAN JENSCH: I wonder if the answer could be read again, please.

(The reporter read the record as requested.)

MR. TROSTEN: For the assistance of the Board, Mr. Chairman, I would like to associate the 150,000 fish, with the two, in the first column, in Indian Point 1 and 2, two to five fish; and the 15 million fingerlings, with the number 225, in the last column in the first row.

CHAIRMAN JENSCH: Very well.

BY MR. TROSTEN:

Q Dr. Stevens, in Table 1 of the document which is entitled, "Staff Analysis of Artificial Propagation to Replace Hudson River Fishes Killed by Power Plant Operation," prepared by Dr. Goodyear, dated April 23, 1973, Dr. Goodyear presents an estimate of the efficiency of hatchery production based on published data.

Do you agree with these estimates?

A No. I think those estimates are unrealistically low.

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In my ~~fif~~th testimony, I testified that this whole program had its beginning in South Carolina about -- in 1961;

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and that in the early years of my operation of the hatchery there, I was essentially unsuccessful because we had not worked out the techniques which would enable us to be successful; also that in the rearing attempts which were first tried at Edenton National Fish Hatchery, likewise the success was not very good, again because we just didn't know how to do the thing and didn't have the techniques worked out, not that they are completely worked out now or that the -- that it is a science, it is still an art; but we in the 1970s, especially in the hatcheries -- egg hatcheries and rearing hatcheries where they have had experience, we are, I think, very successful in the production of striped bass.

The reason I think that Dr. Goodyear's testimony is unrealistic that he has limited his references to those early years. There's some 15 different references he uses, all but three of which go back into the '60s and even into the early '60s.

He uses Moncks Corner data from 1962 and 1964; and this is 1973.

These other data are available. As a matter of fact, the success of the effort in the Southeast in both egg hatcheries and rearing hatcheries are in a little mimeographed report that is public information, available to anybody that wants it from the striped bass committee.

CHAIRMAN JENSCH: From where?

4mil

THE WITNESS: The striped bass committee. It is a committee of the Southern Division of the American Fishery Society where the striped bass people of the several southern states and the federal government meet twice each year; and in a sense they compare notes and write short reports describing their success or failure.

It is -- this type of information is available; and there is a lot of other type of information either published or in scientific reports that are available from state and federal agencies or -- a colleague, a scientist can call individual biologists, as I have called Dr. Barkaloo, and Dr. Shell.

And these data -- if these data were used in addition to this earlier data, then the picture would be much brighter.

CHAIRMAN JENSCH: Would you prepare something that would bring it up to date, then? Perhaps you have some redirect evidence that -- rather than -- saying that it will be better, would you make the calculation of what you think it shows? Give us the sources, some of these manuscripts and other things that might not be easy to identify, which you would be able to do.

THE WITNESS: In my last testimony I chose to pick the most -- the data from the most experienced and the most successful hatcheries to indicate what can be done.

5mil

Of course, that is in a sense biased data, too.

But I feel that it is more logical to testify as to what can be done in 1973, as for instance on any experimental program.

If I were going to address myself to describing our success in the space program, I would talk about our walking on the moon and what we plan to do with the next shot. I wouldn't go back and talk about when we were putting monkeys up in the air; and I also would pick the best success, as for instance what the U.S. can do in relation, perhaps, to what France can do.

The -- in 1973, we have hatcheries, both state and federal, which they have not had good success for one reason or another; but in most instances, it is because they are just getting into the program and their personnel do not have the expertise or the background or the experience to make a success out of the thing.

CHAIRMAN JENSCH: Thank you.

THE WITNESS: I think that the project shown here of .8 percent survival is certainly an unrealistic one in our hatchery systems.

CHAIRMAN JENSCH: What do you mean by successful? Within the last two years, three years, four years?

When do you think you started to have success in this program?

6mil

THE WITNESS: I think I can answer that by the next thing I am going to say.

CHAIRMAN JENSCH: Excuse me. Proceed.

THE WITNESS: All right.

Also there is another document here, by Mr. Goodyear, entitled, "Response to Interrogatory of the Hudson River Fishermen's Association on Supplemental Staff Testimony on the Feasibility of a Fish Hatchery and Replacement by Stocking of Fish in the Hudson River;" and he takes a report published by the Edenton National Fish Hatchery in 1970 --

MR. KARMAN: What page are you reading from?

MR. TROSTEN: Mr. Karman, Dr. Stevens is reading from the single page attachment to your letter to Mr. Macbeth dated April 20, 1973.

THE WITNESS: Dr. Goodyear lists from this report -- it is a federal report -- and states the fact that 125,726 -- approximately 125,000 juvenile striped bass were produced from 169 pounds of striped bass, and goes on to -- with calculations -- calculating that only 744 juveniles per pound of female were produced.

What he fails to mention is that total production at that hatchery that year was not 125,000, but some 930,000, 804,000 of which came from Moncks Corner fish and was the result of standard -- at that time -- standard operational procedures; whereas, 125,000 fingerlings he talks about

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were the results of experiments, tangential experiments.

In the one case they were trying to produce their own brood stock. They had them on station for some four years; and they ovulated them successfully, but the survival in the ponds was relatively low. There are a number of reasons for this.

The other case was they were trying fish from the Nanticoke River, where they spawned these fish and the eggs proved to be so buoyant that they couldn't even -- wouldn't even stay in a hatching jar and they had to try to devise new hatching techniques to handle this egg, these buoyant eggs; and the result was that the percent hatch was small, even though the survival of the fingerlings -- the fry in the pond was relatively good.

Well, this is a very optimistic report and it is dated 1970; and he chooses to lift from this report the least successful segments, the most experimental segments and to completely ignore the standard techniques wherein they were able to achieve a 36 percent survival of all larvae introduced into the pond survived. He ignores this. Naturally the -- when you do this, you throw out the good data and bring in the poor data. You are going to come up with very low estimates.

So I think that for this reason his estimates that the hatchery survivability of fry and larvae in -- and

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fingerlings in a hatchery situation are no better than that in nature is certainly -- has no basis in fact.

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MR. TROSTEN: Mr. Chairman, at this time I would like to offer in evidence on behalf of the Applicant the document upon which Dr. Goodyear relied in preparing his response to the interrogatory for the Hudson River Fishermen's Association.

It is U. S. Department of the Interior Fish and Wildlife Service document entitled "Striped Bass - Morone saxatilis (Walbaum) - 1970 Report on the Development of Essential Requirements for Production." I would like to offer this in evidence as Applicant's Exhibit 6.

(The document referred to was marked Applicant's Exhibit 6, for identification.)

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CHAIRMAN JENSCH: The document just identified by Applicant's counsel may be marked for identification.

Having thus been marked and offered, is there any objection?

MR. MACBETH: I haven't seen the document.

MR. TROSTEN: I asked Mr. Karman and Dr. Goodyear yesterday the basis upon which Dr. Goodyear prepared his testimony, and was advised that he relied on this document.

Is that correct, Mr. Karman?

DR. GOODYEAR: Which piece of testimony?

MR. TROSTEN: This is the response to the interrogatory.

MR. KARMAN: Yes.

MR. TROSTEN: I have sufficient copies to provide to the parties.

MR. MACBETH: I have no objection.

CHAIRMAN JENSCH: Regulatory Staff?

MR. KARMAN: No objection.

CHAIRMAN JENSCH: Applicant's Exhibit No. 6 is received in evidence.

(The document heretofore marked Applicant's Exhibit 6, for identification, was received in evidence.)

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CHAIRMAN JENSCH: Is this the document to which Dr. Stevens just referred?

MR. TROSTEN: Yes, sir, it is.

CHAIRMAN JENSCH: Thank you.

Proceed.

BY MR. TROSTEN:

Q Dr. Stevens, what is your opinion of Dr. Goodyear's conclusion that 2700 10-pound adult female striped bass would be needed to replace Hudson River striped bass taken by the Indian Point 1 and 2 plants? This conclusion being expressed in the response to the interrogatory to which you referred earlier.

A Well, since I feel his survivability is low, it

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follows that the number of adult females he needs would be high. I think it is very high and exaggerated.

I can tell you, very frankly, that it would take Cox's army to capture, maintain, inject, ovulate, and spawn 2700 striped bass.

If we had needed to work with these numbers of fish in the southeast in our program, which has been going since 1961, we would have never gotten to first base because the public would not stand for this, the fishermen would not stand for this. The biologists would not have the time or there would not be enough of them to handle this magnitude of fish; and the administrators don't have the money for this type of effort.

So if I thought this was anywhere in the ball park, I would never attempt to testify as to the feasibility of striped bass culture for stocking purposes.

It is just as far exaggerated, overexaggerated, as the survival figures are underexaggerated.

Q Just to round the record out, Dr. Stevens, do you have any comments to offer with regard to Mr. Clark's conclusion that 5000 fish of about 10 pounds average would be needed?

A Of course, that's even adding insult to injury. It's essentially -- I would not say it is impossible. I would say the cost and public relations aspect would be --

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make it essentially impossible.

Q Dr. Stevens, Mr. Briggs asked the question earlier concerning the river in North Carolina on which stocking was attempted. Do you have any information to supply to the Board with regard to that river?

A Yes. I also have talked to Don Baker, who is chief of fisheries of the North Carolina Wildlife Resources Department; and this concerns the Tar River, in which they historically had a small population of striped bass and a small commercial fishery, that is small in relation to that which obtains in the Roanoke River or Albemarle Sound. This population, for unknown reasons, has declined in recent years, as has the populations in some of the other estuaries, Choctawatchee Bay, Mobile Bay, Mississippi Bay; and in this particular instance, since the North Carolina Department had -- owns and operates annually the Weldon Hatchery, and have -- has several millions of surplus larvae a year, they concluded that they would stock the Tar River with these larvae in hopes to build up this population, presumably on the basis that the adult fish there were not -- either not spawning or not spawning sufficiently. They have seen no effect of it, which is not surprising. As I testified the last time, we found in South Carolina very early that stocking ~~striking~~ of larval striped bass was very, very inefficient. We stocked some 60 million larval striped bass in a 13,000

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acre reservoir; and while we had a few survivors, we did not produce a fishery or -- we concluded that these -- when you stock these small larvae or eggs, there's millions of organisms in the water waiting there ready to gobble them down. It goes the whole gamut of the animal kingdom. There are zooplankton that will eat fish larvae. There's insect life that will eat fish larvae. And, of course, all kinds of fish will eat the fish larvae and eggs; and so for every 10,000 of fry you release, larvae you release, very, very few of them survive. This is borne out in the models by Mr. Clark and Mr. Lawler; and this was what was done in the Tar River.

I think Mr. Baker concluded that larvae stocking was a very inefficient, ineffectual way to establish -- re-establish striped bass population, perhaps a little later than the rest of us did. He continued ~~intended~~ to stock them into recent years.

It is the -- and the whole crux of this matter on this larval stocking, nobody that I know of is advocating we release larval fish. With all these millions of organisms sitting there ready to eat any larvae that comes down, you have to overwhelm this system with -- by -- as the natural females do; and they put 2 billion, 10 billion eggs in the river. And a few of them escape because the system is essentially overwhelmed.

When you put a million in there, then there's

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more than enogh predators to take up these million eggs or million larvae, and nothing survives.

It is almost inconceivable that a hatchery could produce a billion or 2 billion eggs or larvae. The point I want to emphasize is that by protecting the eggs and the larvae in the hatchery, in the hatchery jar, nothing is there eating them; there is no loss except an occasional malfunction with a pump or somebody knocks a jar over on -- of eggs over on the floor; but in the hatchery there is essentially -- if an egg is viable, and you are pretty sure -- most of them are going to hatch into larvae.

Likewise, you take the larvae to a protected pond area where you have completely wiped out all predators, and you have provided vast quantities of food in terms of zooplankton, and you release these larvae into a body of water which has no enemies, and which has abundant food, and the survival there is infinitely greater than it would be in nature; and when you get around to the point where you want to stock your fingerlings into nature, then the number of predators that are there which can and do take the two or three-inch fingerlings are much, much fewer than that which is going to take eggs and larvae; and, therefore, your success or your survival goes up a hundred -- I don't know how much. Forget that. It goes up a great deal.

CHAIRMAN JENSCH: You want to put a number on it?

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On that? Would we get into numbers and probabilities?

THE WITNESS: I don't have any numbers to put on it.

We stock fingerlings throughout the South in reservoirs and estuaries and streams; but we do not have any studies going there which would quantify our survival. It is qualitative data.

CHAIRMAN JENSCH: We have had numbers going the other way. It seems to me you are on the crux of the situation there. Survival of fingerlings. Can you give a representative sample and extrapolate, run it through a computer, perhaps, come up with --

MR. TROSTEN: If you will give us a moment, Mr. Chairman, I think we can provide an answer or arrange to provide it.

CHAIRMAN JENSCH: You don't have to do it at the moment. Later, if you desire.

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: Have you concluded?

MR. TROSTEN: No. No. We have not.

(Pause.)

MR. TROSTEN: Mr. Chairman, may we take a five-minute recess, please?

CHAIRMAN JENSCH: Surely. At this time let's recess to reconvene in this room at 12:10.

(Recess.)

CHAIRMAN JENSCH: With everybody present and not having left the room, we recessed a little longer than we originally contemplated.

Are you ready to proceed?

MR. TROSTEN: Yes, sir. I am.

CHAIRMAN JENSCH: Proceed.

BY MR. TROSTEN:

Q Dr. Stevens, what is the survival rate of eggs to larvae and larvae to fingerlings in a hatchery?

A At Moncks Corner, the survival from eggs to larvae is approximately 40 percent.

Q 40?

A 40.

At the better rearing facilities, such as Edenton, the survivability from larvae to two-inch fingerlings is about 25 percent.

As I earlier testified, in 1970 it was more than that. It was 36 percent on Moncks Corner fish. The 25 percent, I would think, is a conservative estimate of the survivability from larvae to two-inch fingerlings throughout the Southeast in the hatcheries that know what they are doing, that have been in it long enough to know how to rear fish.

CHAIRMAN JENSCH: Why is it that you rejected the 36 percent? I thought you were the one that wanted to

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use the better data. You are going back to this 25 percent figure. Is that a cyclical operation type of thing?

THE WITNESS: No. For instance, this year I myself have a 28 percent survivability. A hatchery, in the state of Florida, had a 22 percent.

I don't want to go the other way and exaggerate. I would like to give you the best I can, the true facts of the matter as I see it. I think 25 percent is a conservative, realistic estimate.

CHAIRMAN JENSCH: Now I understand what you have just said, but I am trying to understand why you don't like the good figure of 36 percent that you seem to think that Dr. Goodyear should have used when you drew up his figures. You say, well, I think 25 percent is better.

Why do you think it? Because in the overall average, on the long projection, you are not going to get 36 percent all the time?

THE WITNESS: No. On this document of the striped bass committee, when I thumbed through there, the given states, 20, 25, 28 seemed to be the success to date of the average hatchery.

Of course, some states had complete water hauls. Texas didn't do any good; Virginia didn't do any good.

If you add these in, perhaps the overall success is less than 25 percent. That's the reason I tried to qualify that those hatcheries that have the experience to know what they are doing, you can reasonably expect 25 percent.

Now in my hatchery ponds at Palatka, Florida, I had up to 61 percent survival; and as low as 6 percent.

CHAIRMAN JENSCH: That's a wide range.

THE WITNESS: It is a wide range. That's why I said it is an art, not a science yet. We are still learning. After all, they have been in trout culture for over 100 years and we are still trying to figure out what is the best food for them. This is a never-ending process. We have made great advances.

As we sit now in 1973, we are producing a lot of fingerlings, stocking a lot of fingerlings in the Southeast. At the Edenton National Fish Hatchery, the best survival I ever heard of to date, as far as numbers, was 77 percent in one pond at Edenton. I think it also ranges all the way down to zero.

We are not in -- we can make predictions now, like the chicken industry, or somebody else. We are a whole lot further along than we were in the mid and late '60s, and we are learning every day. Every year we are improving; and so, I think, I would say 25 percent is prudent, a prudent estimate of what can be done at this day and time.

CHAIRMAN JENSCH: You have never taken all the figures and churned them up and worked out a mean average or any kind of average they have developed? This is kind of a visceral reaction you have, that 25 percent is pretty good?

THE WITNESS: That's correct. As I say, if I took all of the -- every bit of the data -- if I didn't throw out the work of the novices, I think it would bias the data in a way that's not very realistic. You have to give -- anybody that starts out to do this thing has to train his people and learn how. I think what I am trying to emphasize is not everybody can do this thing, that it needs a certain -- a lot of experience and know-how; and you can find instances where they fall flat on their face.

Last year, a number of different hatcheries in state and federal.

This, to me, is not because of the species, not because of the concept, not because of the technique, but it is because at the time they were trying to do it, those that failed were relatively incompetent, and they will gain competence as they gain experience.

CHAIRMAN JENSCH: Thank you.

Proceed.

BY MR. TROSTEN:

Q Dr. Stevens, how does this survival rate you have just testified to compare with the survival rate of fish in the Hudson River as postulated by Mr. Clark?

A Well, in Mr. Clark's Table 5 in the March 15 testimony, from sort of what he -- what he calls fertilized viable eggs until 51-day old juvenile, he projects a .8 percent survival, or .008 survival.

And so this is, of course, at least 10 times -- well, it depends on whether you are talking about eggs or viable eggs.

If he's -- I am projecting about a 10 or 11 percent survival from eggs to two-inch fingerlings.

If you talk about viable eggs, I can project almost double that; and he -- his premise is fertilized viable eggs.

So I could project almost double -- I mean, say 20 or 25 percent survival from fertilized viable eggs to juvenile at 51 days; whereas he projects about .8 percent.

So this is a magnitude there of about 32 times. If you divide 32 times into the 2000 females that they projected as needed on the basis of .8 percent, it comes out something like 60 some odd that I predict. It is all based on the difference between survivability of the juveniles, eggs, larvae, and juvenile fish in the hatchery, as compared to in nature.

It is a difference of opinion, as Mr. Clark testified to.

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Q Dr. Stevens, are there data on the comparative survival of hatchery fish, fingerling fish, once they are stocked in the river, versus fish that are normally in such a water body?

A No, not to my knowledge.

Q Dr. Stevens, in responding to my earlier question as to the range of brood stock that would be required to produce 150,000 fingerlings to 15 million fingerlings, would you -- you use certain assumptions. Would you please indicate for the Board what those assumptions were?

A Well, I just finished answering that. As I said, aout 40 percent of the eggs at Moncks Corner will survive to be fry; and when you stock these fry, about 25 percent will survive to become two-inch fingerlings. Those are the assumptions in figures that I used to project this.

(Pause.)

Q Dr. Stevens, I understand your testimony to be that you consider that the estimate of the number of brood stock required for replenishment of the striped bass which has been presented by Dr. Goodyear and by Mr. Clark is substantially exaggerated, is that correct?

A On the basis of our experience in the Southeast, that's correct.

Q Would you summarize, please, the basis for your opinion that these estimates are substantially exaggerated?

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In your opinion, what is the basic reason why these estimates are exaggerated?

A Well, I think we are going over material I have already covered.

I thought that Dr. Goodyear's estimates were biased because he used data that was not up to date. The current successes are much better than the data he used.

That's essentially the reason why I disagreed with his findings.

One thing I would like to say about Mr. Clark's testimony wherein he talked about the closing of hatcheries; and he was talking in general termas, but he did mention striped bass hatcheries.

I would like to testify that -- to the contrary, that not only has not -- has there not been any closing of striped bass hatcheries in the U.S. in the last decade, but rather there has been a great increase in the number. Moncks Corner was second -- was the second striped bass hatchery in existence. Weldon was the first.

Since that time, as I testified earlier, there's been some -- I don't remember the exact number, but say eight to 10 other hatcheries, striped bass hatcheries, established in that decade since -- between '61 and '73, including, I understand, one recent one in the state of California.

So I just want to say that there may have been other hatcheries closed, but not striped bass hatcheries.

MR. TROSTEN: Mr. Chairman, I would just like to clarify for the record, earlier in his testimony Dr. Stevens referred to certain estimates given by Mr. Clark of survivability. The Clark estimates are derived from Table 5 on page 24 of the testimony of Dr. James T. McFadden dated February 5, 1973. The Table 5 is entitled "Basic Data on Hudson River Striped Bass from Testimony of John Clark."

CHAIRMAN JENSCH: Have you concluded?

MR. TROSTEN: Yes, we have, Mr. Chairman.

CHAIRMAN JENSCH: Hudson River Fishermen's Association?

MR. MACBETH: Yes.

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RECROSS-EXAMINATION

BY MR. MACBETH:

Q Dr. Stevens, you just mentioned striped bass hatcheries, and their number in recent years.

How many of these striped bass hatcheries are producing stock for fresh water situations rather than for estuarine situations or marine situations?

A The -- let's address ourselves to the egg hatcheries. Moncks Corner has a policy of distributing larvae to any state or federal agency who requests them on the basis that having reared these larvae to fingerling or to juveniles, they will return as payment 25 percent of what they have reared; and they distribute these larvae throughout the country on this basis.

The majority of the frys are distributed to inland -- states that are interested in stocking them in inland reservoirs and rivers.

Now in the case of Choctawatchee Bay, Mobile Bay, and Mississippi Bay, these agencies choose to release theirs in estuaries.

Virginia -- the Vienna Hatchery Virginia has the same policy. They will give the fry to any qualified state or federal agency, and they in turn can release them wherever they want.

Does that answer your question?

CHAIRMAN JENSCH: I don't think it quite does. I think he asked what's the number. You said eight or 10 new striped bass hatcheries have been added in the last -- '61 to '73. How many of those are distributing to fresh water? Do you know?

THE WITNESS: All except the Mississippi hatchery. Unless Dr. Shell has a hatchery. I am not sure whether he has a hatchery or not. I know the Mississippi effort is more the estuary of the Mississippi Bay. The rest of them are fresh water.

CHAIRMAN JENSCH: How many striped bass hatcheries are there total, do you know? 15? 16?

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THE WITNESS: I would say -- I had that information at my fingertips in my last testimony. I don't have it now.

I would say it would be more like 10.

CHAIRMAN JENSCH: 10 striped bass hatcheries?

THE WITNESS: Yes.

CHAIRMAN JENSCH: Now all but one distribute their eggs to fresh water, is that correct?

THE WITNESS: No, sir.

The -- I think California -- I don't know that much about the West Coast. That situation out there, I think, is estuarine situation. They have a hatchery and they are releasing fish in the estuaries. I would say two now.

CHAIRMAN JENSCH: Thank you.

BY MR. MACBETH:

Q How recent is the establishment of hatcheries in Mississippi and California?

A The Mississippi hatchery, I think, is -- it is in its second year; and as a matter of fact, that's kind of a portable hatchery where they take it to Maryland and spawn the fish at Maryland and then return their fry to Mississippi for rearing to juveniles.

Now California experience is a rumor to me. I have no certain knowledge of it. I heard they put a big

ar18

hatchery up out there. Whether it is complete or not, I am not sure.

Q I think just to clarify something for the Board --

MR. MACBETH: It was my understanding of Mr. Clark's previous testimony that he was directing his remarks to estuarine and marine stocking of fish. I think he said that once or twice. I think that should be clear.

BY MR. MACBETH:

Q I would like to turn now to the material contained in the single page comparison between the number of mature females required using Applicant's model and a Staff model.

Let's take the situation where 225 mature females will be required for 15 million fingerlings.

How many fish -- striped bass -- do you think would have to be taken out of the Hudson River in order to obtain that 225 mature females?

CHAIRMAN JENSCH: Is there some problem with the clarity of the question?

MR. TROSTEN: The problem, really, Mr. Chairman, is this: The presentation is a joint one; and I think -- and it was also preapred as -- in great haste because of the sequence of events here.

I want to be sure that we have the right person who is answering the question.

CHAIRMAN JENSCH: Well, in view of his previous answers, I thought he was the right person because he was giving some calculations here a few minutes ago about how many female striped bass would be needed.

He had 15,000 -- 15 million -- some sort of thing. He's given a background.

Do you have any objection to the question?

MR. TROSTEN: I do not have any objection to the question.

CHAIRMAN JENSCH: Then let's let the witness answer it, then.

THE WITNESS: You see, as I testified earlier, April 5, I don't know the viability of the eggs in the Hudson; and neither does anybody else.

They may not be 40 percent viability.

I doubt it being more than 40 percent. I can think of reasons why there would be less, much less.

MR. KARMAN: Did you say 40 percent survivability? That was the word I didn't hear.

THE WITNESS: Right.

In the Savannah River we find survivability is 5 percent. But if you assume they are going to be as good, as viable as Moncks Corner eggs, or Virginia eggs, that is 40 percent survivability, then you need 225 eligible females.

The other thing I don't know about is where the spawning grounds are, what percent of the females that are on the spawning grounds are indeed eligible for induced ovulation, and at what time of the year and so forth.

I know these things on other rivers. I do not know what goes on in the Hudson because I don't have any experience there.

I think maybe I can answer your question. For instance, at Moncks Corner, to get 225 eligible females which would give you good viable eggs, with a 40 percent survivorship, you would probably need 260 females. In other words, 25, 30 of those you caught wouldn't work out.

Is that -- does that answer your question?

BY MR. MACBETH:

Q I think it is coming close.

In other words, if you were standing on the banks of the Hudson River, looking for the 225 eligible females, it is your belief that you would have to remove only a total of 260 females in order to get 225 eligible females?

A Right. If I remember my experience down there, I was able to ovulate successfully some 88 percent. I think Mr. Bayless who is there now, at least that much, maybe even more.

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You see, the trick is -- and this is -- this is to find ~~the finest~~ spawning ground. Take the fish during the spawning season, about a six-week period.

ar21

If you go there out of the spawning season, then your fish are not going to -- not going to perform like you want them to.

Once you know what you are doing, and know where to do it, and the time to do it, then your success goes way up.

Q How long do you think it would take you to find out where to do it and when to do it on the Hudson River?

A I could find out a lot in the month of May.

Q So that you think in the following year you would only have to take 260 females from the river?

A You mean projected onto 15 million fingerlings?

Q Yes. Assuming that you want 225 for the females.

A I went to -- three strange rivers last year --

e7

1mil

CHAIRMAN JENSCH: Excuse me, Dr. Stevens. I know you are going to answer. But I think if you deal correctly with the question -- the last question was the next year you would only need to pick up 260 eligible females, is that correct, yes or no?

THE WITNESS: I don't know. I went to the Nassau River in Florida last year and I can tell you I couldn't find 260 eligible females there in working it 100 years.

I went to the Nanticoke River in Maryland. I could find 260 females, or 2600 females there for the next 40 years. It depends on the river. I don't know a damned thing about the Hudson.

(Laughter.)

BY MR. MACBETH:

Q Well, that is remarkably candid.

How many males would you need?

A Our technique on males is this: We typically use two males per spawning to assure that if one is sterile, the other one is not. We can use those two males again and again because they produce copious quantities of milt.

I would say it would be not quite one to one, maybe three-quarters male per female, something on that order

Q So that you need something on the order of 200 males?

A Correct.

2mil

Q What is the fate of those fish at the end of the spawning season, the roughly 500 -- excuse me, 460 fish that were used in tha hatchery?

MR. TROSTEN: Now let's be clear about this, Mr. Macbeth. You are hypothesizing in this question that if 460 fish were used using the extreme example of 15 million fish, how many would be required, is that correct?

In other words, --

MR. MACBETH: Well, we are past that.

Yes, we are assuming that it is a situation in which the experimentor or the operator of the hatchery wants 225 eligible females. Now to establish the value of need, you would have to take out 260 females and 200 males.

MR. TROSTEN: That's right. And that a determination had been made that you needed that many as opposed to two eligible females?

MR. MACBETH: We are assuming that you need the 225.

MR. TROSTEN: As opposed to the two?

MR. MACBETH: As opposed to the 500,000, anything.

MR. TROSTEN: Just as long as the record is clear.

MR. MACBETH: That is absolutely right. We are assuming you want 225 eligible females.

BY MR. MACBETH:

Q What would be the fate of the 460 fish at the end?

3mil

A In the early years at Moncks Corner, we used to cut the ovaries out of the fish and that would kill the fish, naturally. But after that, I started tranquilizing the fish to relax her so she would release her eggs. That way I could strip them out of her without killing the fish.

We would bring them out of the tranquilization and -- after they were swimming properly, release them into the rivers. This is the common standard technique at most hatcheries that I know of now, after having stripped the fish of her eggs, we release them into the river.

Q And that would -- does that mean 100 percent survival?

A You mean before release? Sometimes we were unable to get the fish out of the state of torpor; and so I was losing a small percentage. It would not be 100 percent.

Q It is somewhere in the 90s?

A Right. We would release to the river.

Q You said earlier on that you could think of many reasons why the viability of the eggs -- or the -- I think it was the viability of the eggs would be less than 40 percent; for instance, in the Savannah River, it was five percent. I am using those figures --

A It is in the ballpark.

Q Yes.

What are the reasons that you -- that it might well

4mil

be less than 40 percent?

A We don't know why the viability of the egg in the Savannah is low. We are looking into some sort of pollutant as an insecticide or heavy metal. I say looking into it; we are collecting eggs and ovaries; and we are collecting the flesh of the brooders and sending the material to federal labs in Columbia, Missouri, where they are running assays of the insecticide and heavy metal loading and trying to correlate that back to the survivability of the eggs or larvae.

We are doing this in the Chesapeake Bay. We are doing it throughout the -- up and down the East Coast. We did some of it last year and some of it -- we are going to do some more this year. But we don't know.

We think it might be something like insecticide or heavy metals. We have no data which shows that at this time.

Q Have any studies been made which compare the natural viability of eggs in a particular river system to the viability of eggs, fish taken from that river system, in a hatchery?

A I think you said -- you asked me if the surviv-ability of the eggs taken from a naturally ripe fish is the same as from a fish that is induced to ovulate? Maybe that is not your question.

Q Let me put it another way: What is the basis of this five percent figure for the Savannah figure? Is that

5mil

an induced figure of studying the natural conditions or is that fish taken from the Savannah River and hatched in a hatchery?

A Well, in all these hatcheries, the hatchery manager keeps his records as follows: He records every fish that comes in and the number of eggs that he gets from the fish. Then he records the number of fry that goes out; and at the end of the year, he divides the number of fry that he's produced by the number of eggs that he's brought in, and he gets a percentage of viability.

This -- if he has malfunctions and kills his eggs or drops them on the floor, he uses that anyway. 40 percent, 45 percent in Moncks Corner is all the pilot error, the malfunctions, everything.

This is what they do at the Richmond Hill hatchery on the Savannah River.

Q So that these are hatchery figures for Savannah River fish rather than figures for fish naturally in the Savannah River?

A Right.

Q Are there any figures of -- comparative figures for natural hatching in the Savannah River?

A Survivability?

Q Yes.

A No, not that I am aware of.

Q Are you aware of any other comparative sets of

6mil

figures of that sort where the same strain of fish, in a hatchery situation, can be compared with that strain of fish in a natural situation?

A In the -- there are data -- there is a study -- on the Santee-Cooper reservoir. On the estimated survival of naturally spawned eggs. This is the same brood stock we use in the hatchery.

Q Moncks Corner?

A Right.

There is data also at the Weldon Hatchery where they receive naturally ripe fish from fishermen and -- catch the fish in the river and bring them to the hatchery where they spawn.

In the same year they took green fish that were not ripe and induced them to ovulate with hormones; and they compared the survivorship of the naturally ripe fish with those induced to ovulate with hormones.

Q Is that the material in the article by Tatum, Bayless, McCoy & Smith? The reference is given on page 6 of Dr. Goodyear's testimony.

A What is the title?

Q Of April 23?

The title is "Preliminary Experiments in the Artificial Propagation of Striped Bass, 19th Annual Conference, Game and Fish Commissioners."

7mil

A I think it might be, but I am not sure; but that is available through -- I am sure you can get that information from Don Baker.

Q What were the results with the comparison between the Santee-Cooper reservoir and Moncks Corner hatchery?

A The -- I think the -- at the stage the eggs are coming down the river, they figured -- now this is -- I am going way back in my memory, too, on this -- I believe it was something like they figured 60 percent of the naturally spawned eggs were viable, or at least not dead at the time they collected them. As compared to 40 percent, which was entirely dead, they turned white. Very easy to tell a dead egg.

These eggs were, of course, anywhere from two hours to 24 hours -- had been drifting in the river that long at the time of capture.

Q Yes.

Apart from the information on the experiments in Florida, Alabama, and Mississippi, do you have any information on the survivability of two-inch hatchery reared fingerlings when they are put into a natural estuary situation?

A Well, as I testified earlier, McElwain ~~Mr. McCowan~~ of the Mississippi effort had a 2.4 percent return. How you quantify that, I don't know, as far as actual overall survival.

8mil

Of course -- well, unless you drain Mobile Bay and count them, this would be very difficult. You have to do it statistically. He's not concerned with that. Neither is anybody else down there that I know of, models as -- such as you have here on the Hudson River.

Q Beyond those three, you don't know of anything else? I am not suggesting there is anything else, but I just wanted to be sure that those were the three situations where there might be information on the survivability of hatchery fish in an estuarine?

A Unless California is doing something -- and they might well be -- I don't know of any.

Q All right. I was a little confused earlier on at one point where you were talking about the survivability from stage to stage. It seemed to me you said at one point that you went from -- in going from eggs to larvae, here the reference is to Moncks Corner, there was 40 percent survival; going from larvae to fingerling, with reference to Edenton, there is 20 percent survival. Does that mean there would be a total of 10 percent survival from eggs to fingerlings?

A Of eggs to fingerling. Now if you talk about viable eggs to fingerling, that is different.

Q That is what I was coming to next. By eggs, you meant simply eggs that are spawned, not fertilized eggs, or viable eggs?

9mil

A Right.

CHAIRMAN JENSCH: Did you answer --

Okay. Thank you.

BY MR. MACBETH:

Q You put considerable emphasis on the fact that this spawning is an art. How many practitioneers of the art are there in whom you have high confidence?

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A That have success, high success?

Q Yes.

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A Well, there's me, Jack Bayless, Bill Neal at Virginia Hatchery, Dave Bishop at the Tennessee Hatchery; there's Forrest Ware at the Florida Hatchery.

The Oklahoma success in the past year was encouraging, but not very great. I don't think they really -- it may be their brood stock -- maybe they don't have the experience. I don't know what it is. I think I named five. I would say those are the ones that are qualified to produce 40 percent that hatch.

Q Does that mean in turn that the success of any program which was attempted on the Hudson would depend on one of those men or someone of their caliber, which I take it to be a very small group, being in charge of it?

A Well, somebody trained up. The smallness of the group is based on the small effort. I mean, I don't say you can't take a competent fish biologist and train him. It is just

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plain experience. You can train him up to that.

But as it sits right now, I would say that those five are the ones that I would have in mind.

Q In the assumptions where you said that the total number of eggs per female would be 600,000; what was the size of the female in that assumption?

A That was Dr. Lauer's -- 10 pounds, is that correct?

Ten -- apparently that is the 10-pounder, Hudson River 10-pounder.

MR. MACBETH: I have no further questions of this gentleman, Mr. Chairman.

CHAIRMAN JENSCH: Regulatory Staff?

Do you want to wait until after lunch? It is 1:00 o'clock. Which is more convenient?

MR. KARMAN: I am not sure we have any, but I think that we may discuss this during the lunch hour.

CHAIRMAN JENSCH: Very well.

MR. KARMAN: We can find out if we have any additional questions we would require of Dr. Stevens.

CHAIRMAN JENSCH: Very well.

MR. TROSTEN: I have one question.

CHAIRMAN JENSCH: Proceed.

FURTHER REDIRECT EXAMINATION

BY MR. TROSTEN:

Q Dr. Stevens, you have testified that you do not

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presently have experience with regard to the Hudson River.

Is it correct that you are going to be acquiring experience with regard to the Hudson River as described in the document entitled, "Feasibility Study of Mitigating Power Plant Losses with Artificial Propagation of Striped Bass"?

A My company would like to contract to run a number of studies and provide a number of Hudson River juveniles for stocking in the river; and we have made such an offer.

The answer would be yes. We do have proposals.

MR. TROSTEN: That is all.

CHAIRMAN JENSCH: An hour, is that enough? Hour and 15 minutes?

MR. KARMAN: Fine.

CHAIRMAN JENSCH: Let's recess at this time, to reconvene in this room at 2:15.

(Whereupon, at 1:00 p.m., the hearing was recessed, to reconvene at 2:15 p.m., this same day.)

8

AFTERNOON SESSION

(2:15 p.m.)

CHAIRMAN JENSCH: Please come to order. Does the staff have questions of Dr. Stevens?

MR. KARMAN: I have one question, Mr. Chairman.

Whereupon,

ROBERT STEVENS

resumed the stand and, having been previously duly sworn, was examined and testified further as follows:

FURTHER RECROSS-EXAMINATION

BY MR. KARMAN:

Q Dr. Stevens, I call your attention to table 1 on the efficiency of hatchery production which we were looking at prior to the luncheon break today.

I ask you whether you could provide for us your statement of the average figures for currently operating hatcheries which you believe should be inserted in the mean column toward the bottom of this table?

CHAIRMAN JENSCH: Will you identify that document again please?

MR. KARMAN: Twenty-four is the document entitled, "Staff Analysis of Artificial Propogation to Replace Hudson Fishes Killed by Power Plant Operation, C. Philip Goodyear."

CHAIRMAN JENSCH: You are referring to table one thereof?

MR. KARMAN: That is correct.

CHAIRMAN JENSCH: Thank you.

THE WITNESS: How long will you give me to provide this?

BY MR. KARMAN:

Q I think it would be best if you told me how long it would take.

A Well, you mean for the current -- for 1972 season, is that correct? Is that what you want?

Q Yes.

A Yes, I could go back to my office and dig out this document and compile something within the next couple of days.

I do not have -- there are several states that did not respond or did not write their report, turn it in. I don't have that information.

But of those that did, I could compile that or I could send you the report and let you compile it if you would like.

Which would you prefer?

(Laughter.)

(Pause.)

Let me draw that back.

I don't know whether I could get your pounds of fish used in that data. I could certainly get the percent -- get the percent of the brood stock spawned; percent hatched of the eggs obtained; and the percent survival of the larvae stock in hatching ponds.

Q The only problem I had was I assumed that your estimates were based on figures which would allow you to do that, to fit those figures in at this time.

MR. TROSTEN: Well, Dr. Stevens does not have these data, you know, with him, in his head, or with him, Mr. Karman.

If the Board will permit, and the parties can stipulate, that Dr. Stevens will be permitted to prepare this document and it will be received in evidence without the necessity of a further evidentiary session on this, I think we could undertake to do that. He does not have this information available to him.

MR. KARMAN: That would be perfectly all right with me.

MR. TROSTEN: Is that acceptable to you, Mr. Macbeth?

If not we ought to let the record stand on the basis of what the witness has. We are trying to wrap this up.

MR. KARMAN: Or has not presented.

MR. TROSTEN: Whatever he has available at this moment.

MR. MACBETH: That's all right with me as long as there is a full discussion of the basis on which the numbers rest so we don't simply get a series of numbers without documentation, basis on which they are developed. If we can have that, it would be satisfactory to me.

THE WITNESS: The basis upon my testimony? Well, my -- what I prefer to do, want to do is testify on the basis of the success of the Moncks Corner Hatchery and the Edenton Hatchery. That is essentially where I get 45 percent and 25 percent.

Now, if you want a mean figure, it's going to be less than that, I can tell you that, as I testified before. When you add in the water hauls and the unsuccessful people, your percentage figures are going down.

My testimoney -- and I try to point this out in my April 5 testimony -- that based on what has happened in Moncks Corner and Edenton, these figures are reasonable because I think you can -- you could put yourself in a position on the Hudson River to duplicate or -- I made -- measure what success they have had in any successful hatcheries down there.

This is the best we can do to date, is what I was trying to testify to.

MR. TROSTEN: Well, Mr. Karman, I think --

MR. KARMAN: I said it would be all right with me.

MR. TROSTEN: It would be all right with you?

MR. KARMAN: Yes.

MR. MACBETH: Do you agree to giving the basis on which the numbers rest? That's all I want. I want to get a complete record rather than, you know, something that would demand more questioning. I prefer to wrap it up too but to do that -- the man is having a statement as to where the numbers come from.

MR. TROSTEN: There is no reason why Dr. Stevens cannot present the --

CHAIRMAN JENSCH: Well, let's leave it this way.

If you prepare something and you desire to interpose objections thereto, you may do that. Otherwise we will see if we can't work it out and receive it into evidence.

I think that as I understood the question, however, it was current years; and his response was you want it for one year, '72.

I wondered if you wanted to go back to '69, '73, whatever data they have?

MR. KARMAN: The latest information he has.

CHAIRMAN JENSCH: One year?

MR. KARMAN: One year would be sufficient.

CHAIRMAN JENSCH: One year would be sufficient?

You have enough data to work out a mean, do you, back in your office?

THE WITNESS: Yes, sir.

As I say, I don't have the pounds of fish. Some of this data I don't have. I don't know how many pounds of fish were involved of the brood stock used.

CHAIRMAN JENSCH: Maybe your records will reveal that?

THE WITNESS: It will not. We don't customarily keep this type of information.

CHAIRMAN JENSCH: How about these several documents that you were indicating were available in every public library or every hatchery office in the country, that you could get all these data?

THE WITNESS: That data does not include weight of the brood stock, total pounds of the brood stock.

CHAIRMAN JENSCH: Where did Dr. Goodyear get the foundation?

Can you tell us, Dr. Goodyear?

DR. GOODYEAR: Out of documents I had.

CHAIRMAN JENSCH: Maybe if you show the type of document you had, Dr. Stevens can find this and location figures too.

Would that be helpful to you, Dr. Stevens?

THE WITNESS: You see, two of these reports are authorized by me; and those reports, in them, I did include the weight of brood stock used at Moncks Corner in '62 and '64. But these reports that are being submitted to the striped bass committee do not -- they include how many brood stock, but they don't include the pounds per brood stock.

CHAIRMAN JENSCH: Well --

THE WITNESS: This would require a lot of personal correspondence and telephone calls I would assume.

MR. TROSTEN: Mr. Chairman, I will suggest we will prepare the best response we can in the light of the question; and if it's unsatisfactory or -- it doesn't have to go into evidence. We will do it the best we can.

CHAIRMAN JENSCH: Fine. Fine.

MR. KARMAN: No further cross.

CHAIRMAN JENSCH: I had some questions.

Did you have any questions?

Dr. Stevens, I was -- I would like to refer to Applicant's Exhibit No. 6, which your counsel handed to you.

As I understood your criticism of Dr. Goodyear, you said he picked out the poor figures and you thought he ought to use the good figures. I wonder if you could tell us where the poor figures are and where the good figures are?

THE WITNESS: Page 16, sir.

Under Harvest.

CHAIRMAN JENSCH: Yes, proceed.

THE WITNESS: The overall survival of the entire hatchery was 29.3 percent; and as I understand it, there were 3 different distinct brood stocks used, one of which produced 2.7 percent; the second of which produced 22.9 percent; and the best of which produced 35.7 percent.

Now he simply used the data from number one and two and not from number threee.

CHAIRMAN JENSCH: And that was the basis of your criticism of Dr. Goodyear, is that correct?

THE WITNESS: Right.

CHAIRMAN JENSCH: References were to table 1, to 35 percent, is that correct?

THE WITNESS: Is table 1 -- his table 1 includes -- see, there are two documents that I attempted to criticize. One was --

MR. TROSTEN: It's the response to the interrogatories of the Hudson River Fisherman's Association --

THE WITNESS: Plus --

MR. TROSTEN: That's it.

THE WITNESS: Plus his --

MR. TROSTEN: Plus table one in the April 23 testimoney of Dr. Goodyear.

CHAIRMAN JENSCH: Well, I am referring to this page 16 of Applicant's Exhibit No. 6.

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: Table one is used therein. I assume it's table one of Applicant's No. 6.

MR. TROSTEN: No, sir.

The table 1 that Dr. Stevens has been referring to is a table which appears in Dr. Goodyear's testimony of April 23.

Here it is, Mr. Chairman.

(Mr. Trosten hands the chairman the document.)

CHAIRMAN JENSCH: I understand that.

Here is a document which I understand was prepared by the U.S. Department of the Interior; and on page 16 thereof, they refer to a table one. I don't presume they knew of Dr. Goodyear's table 1 prepared in April of 1973. So I assume it's table one. I want to refer to table one.

THE WITNESS: That page is unnumbered but it's after 25 -- no, it is 26, excuse me. Page 26.

CHAIRMAN JENSCH: Thank you.

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DR. GEYER: There is also a Table 1 on page 32.

THE WITNESS: They are different sections.

MR. BRIGGS: And on page 19.

DR. GEYER: Plenty of Table Ones.

(Laughter.)

THE WITNESS: Yes.

CHAIRMAN JENSCH: I suppose they give you quite a range for the good figures.

(Laughter.)

CHAIRMAN JENSCH: Which one do you think they are talking about?

THE WITNESS: They are talking about --

MR. BRIGGS: Page 19, I think.

? THE WITNESS: Page 1 and -- page 1 on page 19.

CHAIRMAN JENSCH: All right. Now where is the 2 percent and the 25 and the 39?

THE WITNESS: The 2 percent is the first series of entries under source, ED standing for Edenton, ponds 1, 13, 14, and 15.

MR. TROSTEN: It is the first collection of data. Do you see that, Mr. Chairman?

CHAIRMAN JENSCH: There is a figure used on page 16 of Applicant's Exhibit No. 6, which is 2.7.

THE WITNESS: 2.7.

CHAIRMAN JENSCH: That's over under harvest data, under the column percentage of survival. Is that correct?

THE WITNESS: That's correct.

CHAIRMAN JENSCH: Now you go back to the fourth column of that general section, and it says date, 4-22-70. What does that date refer to? Can you tell me?

THE WITNESS: This is the date that the larvae were stocked in pond one.

CHAIRMAN JENSCH: Yes.

Now I thought -- maybe my notes aren't correct, but I thought you were criticizing Dr. Goodyear for taking figures, like the monkey into space, like 1960. Now where is the 1960 figure that you are complaining about?

THE WITNESS: Then we go back to his number one table, where I criticize that.

CHAIRMAN JENSCH: I see.

THE WITNESS: Have you got his Table Number 1?

CHAIRMAN JENSCH: Yes, sir, I do.

MR. TROSTEN: It is the figure in the second column, under the heading "year," Mr. Chairman.

CHAIRMAN JENSCH: That's after page --

MR. TROSTEN: It appears after page -- it is unnumbered, but -- our version doesn't have a number.

MR. MACBETH: Page 5.

MR. TROSTEN: After page 5, according to Mr. Macbeth.

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MR. MACBETH: It is page 5.

MR. TROSTEN: Does your copy have numbers?

MR. MACBETH: Well, there are written numbers on my copy, handwritten in the upper right-hand corner.

MR. TROSTEN: I see.

MR. KARMAN: It is the fifth page.

CHAIRMAN JENSCH: Yes. Thank you.

Well, he used 2.7; and that report uses 2.7. Now what does -- what is the criticism? I mean it doesn't make any difference what you use, you get the same figures, 2.7, don't you?

THE WITNESS: But that was only a portion of Table 1. Why did he pluck that out and use it? That's the very least successful of the three different fish.

CHAIRMAN JENSCH: That's the same figure that you have got in Applicant's Exhibit No. 6. It seems like they are consistently running 2.7.

THE WITNESS: All right. Go back to Table 1 in the Applicant's 6.

CHAIRMAN JENSCH: Yes, sir.

THE WITNESS: And drop down to the next percent survival, 22.87.

CHAIRMAN JENSCH: Yes.

THE WITNESS: Then drop down to 35.70. My question is, why -- why pluck 2.3 percent out of a report, where

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immediately below that, maybe, whatever -- 15, 17 times as good.

Why not use it all? Why not use an average of it all if you don't want to bias it either way?

The average of it all is 29 percent survival. Why figure 2 percent?

If the report -- if this report only included fish that survived 2.7 percent, great. But it includes three different experiments here and he chose to pick one.

MR. MACBETH: Mr. Chairman, could I clarify something on Table 1 of Dr. Goodyear's testimony?

MR. TROSTEN: Are you going to clarify Dr. Goodyear's testimony?

MR. MACBETH: I would like to keep -- like to ask Dr. Stevens a question of his interpretation of Table 1 of Dr. Goodyear's testimony.

CHAIRMAN JENSCH: Do you mind if I go ahead a little bit? Write yours down?

MR. MACBETH: Yes.

CHAIRMAN JENSCH: In case you don't get the opportunity, remind us that you have that question.

Thank you.

Dr. Stevens, would you turn to page 5 of Dr. Goodyear's statement? Under the general column staged survival, you go down across from the Edenton, I see .229

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and .357; and it seems to be related to 1970.

Now is there something about that to which you have a suggestion or complaint?

THE WITNESS: It was -- yes, you are correct. It did include that in his Table 1. Where he did not include it is his -- have you got this letter dated April 20?

CHAIRMAN JENSCH: No. I am just looking at the exhibit material.

THE WITNESS: That's correct. I did not criticize the fact that he did not include the -- all the Edenton data in that report. I do not know whether he did or not.

What my criticism of this particular document was, all but three of 15 references are before 1970.

CHAIRMAN JENSCH: Well, if they are consistently running the same figures, what difference does it make?

THE WITNESS: They are not. That's my point.

CHAIRMAN JENSCH: Well, what data do you have other than this Applicant's Exhibit No. 6, which I understand is related to 1970, and the figures are approximately the same as Dr. -- the ones Dr. Goodyear used in his Table 1?

THE WITNESS: The figures I am referring to are-- for one thing in the -- the -- the striped bass committee report, 28 --

CHAIRMAN JENSCH: I don't know anything about that. Is that Exhibit 6?

MR. TROSTEN: No, sir.

CHAIRMAN JENSCH: That's some other document. All I can do is look at what's been introduced in here. If there is something in the evidence that troubles you, will you tell us what it is?

If there is something you have back in your office, it is something I can't work along with you on that.

THE WITNESS: Yes.

CHAIRMAN JENSCH: Any letters that you have written or have received.

THE WITNESS: Well, I am simply saying I can't tell you specifically or relate the reference specifically, but there is a wealth of data subsequent to 1971, as a matter of fact.

For instance, at Moncks Corner, the striped bass hatchery has been operating today -- he includes '62 and '64. There are reports on the operation of that hatchery every year since that time.

CHAIRMAN JENSCH: Are the figures any different?

THE WITNESS: Yes, they are 40 and 45 percent currently.

DR. GEYER: They don't even have the figures in this Table 1.

THE WITNESS: That's my point.

CHAIRMAN JENSCH: I thought you were objecting

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because he was using something in 1960. If the reports -- figures aren't in there -- I am having trouble following your --

MR. KARMAN: I think the problem is there may be a communication gap as to which table we are talking about, the -- Dr. Goodyear's table or this striped bass table.

CHAIRMAN JENSCH: Let's call it the Goodyear table and the striped bass table -- the Department of the Interior table.

In Goodyear's Table 1, I don't see any figures for Moncks Corner for a stage of survival, pre-stocking phase one or two. Do you?

THE WITNESS: Moncks Corner is an egg hatchery. They are not a fingerling hatchery.

CHAIRMAN JENSCH: Well, all right. If we take the figures for Moncks Corner --

THE WITNESS: Let me -- let's take -- I know how we can get to this, I believe.

He's got hatch, under percent of hatch, '62, '64 --

CHAIRMAN JENSCH: Yes, sir.

THE WITNESS: One is 7 percent, one is 31 percent.

CHAIRMAN JENSCH: Yes.

THE WITNESS: If he took the percent of hatch for every year, '65, '66, '67, '68, right up through '72, those figures would run around a 40 percent average every year; so I would have eight years times 40 percent and one year times 31 percent, and one year times 7.3 percent; and then the mean, instead of being 31 percent and 7 percent, or about 20 percent hatch per year, would be essentially double that or 40 percent hatch per year; and if you take that figure and plug it in down here to his ultimate figure of .8 percent survival as a figure, a reasonable figure for hatchery survival, it would certainly increase that a great deal; and then if you did that in every instance, if you used all of the Edenton reports, or all the reports available from Edenton, into the '70s,where they were having better success, or some of the other hatcheries that had been established, some of the other rearing stations that had been established, there is no way he could get .8 percent because we couldn't operate on .8 percent.

Nobody in his right mind would spend the money that is being spent by the state and federal government in the Southeast on the striped bass program; and it is a multi-million dollar program, if the efficiency was .8.

CHAIRMAN JENSCH: That may be your best judgment in a situation, but I don't know that we can relate that entirely to the kind of expenditures that are sometimes carried on. We have to disregard the philosophical comment you have and do you have figures that can be supplied?

Now he has stated from published data -- I

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assume it is the data he found that had been published -- I think you have referred to the fact there are several reports available here, there and so forth. Whether they are generally available or generally published, I don't know.

You have mentioned several reports and several figures which you think are pertinent, I think that we could see the basis of your criticism.

MR. TROSTEN: We can supply that, Mr. Chairman, in response to your question.

CHAIRMAN JENSCH: Thank you. Thank you.

THE WITNESS: I would also like to clear up -- if he means published in a journal, scientific journal -- several of these are not -- contribution service number two, sport fish division, Georgia Game and Fish, number six, is simply scientific report that is mimeographed by the state of Georgia.

Similar type things. In other words, this would not be considered a publication in the sense of being published in a scientific journal.

CHAIRMAN JENSCH: You are talking about Applicant's Exhibit No. 6, is that correct?

THE WITNESS: Right.

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CHAIRMAN JENSCH: Maybe that is the difference between you and Goodyear. You talk about things being available and he talks about things being published and available.

Would you recognize that is a possibility of difference?

THE WITNESS: That is the point I am trying to make.

No. 6 -- turn to his page, page 6.

No. 6 is not -- let's define what we mean by "publication."

CHAIRMAN JENSCH: We will get to that when we get to him. I am trying to find out first what your criticism is and if you recognize there is a distinction between what is published and between what is available elsewhere?

THE WITNESS: He has publications. The one -- two, three, four are publications.

Five, six seven, eight are releases, scientific releases, and eight, nine, ten, eleven are publications. He is using both.

CHAIRMAN JENSCH: You are saying that he should have had available to him the kind of data you say you know is available? Is that your criticism?

THE WITNESS: And use it, yes.

CHAIRMAN JENSCH: But if he doesn't have available to him what you had available to you, you would recognize there could be a difference in the approach to the matter; is that correct?

THE WITNESS: I would say he is not very well prepared.

CHAIRMAN JENSCH: Well, thank you for your comment.

But answer my question, if you will: Do you recognize there could be a difference in analysis if there are more data available to you than are generally available by general publication?

THE WITNESS: That is my point. The data is public that I testified to is public, available to the public. I testified to that respect.

CHARIMAN JENSCH: You mentioned that.

I am saying that assuming maybe you are in error perhaps, I mean as an assumption, would you recognize that that could be the basis of a difference in the analytical work that is undertaken? More data available to you than would be available to him? It would lead to a difference in results, would it not?

THE WITNESS: It would if the data were available to me and not to him.

CHAIRMAN JENSCH: Yes. All right.

Now the next question is would you make available all the data you think should have been utilized by Dr. Goodyear for his analysis?

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: Very well.

Now I was having a little trouble again with your statement that things weren't very optimistic in Applicant's Exhibit 6 and he should have been a little more hopeful and enthusiastic about the program as I understood your testimony.

Let me just ask you if some of these -- I had not seen this until this morning, Applicant's Exhibit No. 6. Perhaps I am taking some things that aren't truly representative. But I wonder if these were some of the things that you thought were optimistic in Applicant's Exhibit No. 6.

Let me try page 5, if you will, please.

Under the section called "Discussion," there is the brood stock. The first line "All attempts to maintain the brood stock after spawning failed."

I didn't detect any optimism in the program there. Can you tell us, is that one you thought he should have utilized for an optimistic analysis?

THE WITNESS: The optimism was on the spawning, the rearing of the fish which is the major function of the Edenton station.

CHAIRMAN JENSCH: Is this one statement one that

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you thought he should have utilized to be optimistic about the program?

THE WITNESS: No.

CHAIRMAN JENSCH: Okay.

Turn to page 8.

It says "Although some success in hatching eggs from the Nanticoke River of striped bass was achieved, further efforts must be made to determine the limiting factors responsible for the relatively low percent hatch."

Is that one of the optimistic statements you thought he should have utilized in his analysis?

THE WITNESS: It is one of the examples he used to prove that hatchery -- that the success that year was relatively poor, and I tried to point out it was relatively poor because it was an experimental -- it was the first time that the Nanticoke River strain had been employed.

CHAIRMAN JENSCH: Thank you for the explanation.

My question was: Should he, in your judgment, have utilized that statement as the basis of some optimism?

THE WITNESS: He in a sense did.

CHAIRMAN JENSCH: He did?

All right. Turn to page 17.

Under the section called "Discussion," the second and third sentences of the first paragraph "The percent survival was similar to past years except for the Edenton fry. Below survival of this group is probably due to the high incidence of deformed fry."

THE WITNESS: Where are you again?

CHAIRMAN JENSCH: Page 17 of Applicant's Exhibit No. 6, under the section entitled "Discussion."

THE WITNESS: Okay.

CHAIRMAN JENSCH: Second and third sentences of the first paragraph.

Is that high incidence of deformed fry a source of optimism that he should have utilized in his analysis in your judgment?

THE WITNESS: Yes. That is another -- he did utilize that group of fish.

CHAIRMAN JENSCH: I mean should that have been used -- you said there was a lot of optimism in Applicant's Exhibit 6. That is not optimism?

THE WITNESS: No.

CHAIRMAN JENSCH: He should have regarded that as optimism for your program; is that correct?

THE WITNESS: That is correct.

CHAIRMAN JENSCH: Turn to page 23, please.

Under the section entitled "Marking," second sentence of that first -- that only paragraph, "Approximately 20 percent of all tagged striped bass perish before reaching their stocking destination."

Is that an illustration of optimism he should have utilized in his analysis, in your judgment?

THE WITNESS: He didn't address himself to shipment.

CHAIRMAN JENSCH: I understand that. I am asking for your judgment.

THE WITNESS: No, that is correct.

CHAIRMAN JENSCH: He should not have used that as the basis of optimism; is that correct?

THE WITNESS: That is correct.

CHAIRMAN JENSCH: Very well.

Turn to page 24, second full paragraph at the top of the page.

"This higher temperature appeared to have a direct bearing on the condition of the fish in that they were extremely tender and could take only a minimum of handling."

Is that a statement --

THE WITNESS: No, that is not a statement of optimism.

CHAIRMAN JENSCH: Turn to page 30. The last full paragraph on the page, the first sentence thereof: "Reasons for failing to sustain striped bass fry on various diets tried can only be surmised."

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Is that a statement of optimism he should have considered for his analysis in your judgment?

THE WITNESS: No.

CHAIRMAN JENSCH: Very well.

Turn to page 36, please.

Under the section entitled "Discussion," the first sentence: "No definite conclusions have been reached as to why the fingerlings held in tanks did not respond to columnaris treatments."

Is that a statement of optimism he should have utilized in his analysis?

End #11 THE WITNESS: No.

CHAIRMAN JENSCH: I think in some of your testimony you indicated that in the last few years, '70, '71, or '72, I have forgotten, you indicated that -- I believe that Moncks Corner, and maybe Edenton, maybe some others, there has been quite a marked increase in successful activities at each of those locations, is that correct?

THE WITNESS: That's correct.

CHAIRMAN JENSCH: In your judgment, are those in any wise related to cyclical activities in the sense that they are occasional? It might happen for a couple of years and then drop off? Or are you on a sustained yield of high optimism, good results, and successful operations? No failures likely?

THE WITNESS: Oh, no. I won't say that. I would say that because of our increased competence we can smooth out the ups and downs.

As an example, Moncks Corner last year had a more difficult time finding brood stock than it had the year before. That's definitely cyclical. Of the brood stock that he found, he would -- percent hatch was as good as it had been before.

So, the cyclical part depends on the nature; but the technique for inducing ovulation and the survival of the eggs seemed to be pretty standardized.

CHAIRMAN JENSCH: But in spite of the standardization

standardization, you recognize that Moncks Corner had a little flip-flop last year, was it, that they couldn't get brood stock?

THE WITNESS: Not as easily.

CHAIRMAN JENSCH: So, there can be variations?

THE WITNESS: Oh, yes, definitely.

CHAIRMAN JENSCH: There is no guarantee you are going to attain the results you had in the last two or three years, is that correct?

THE WITNESS: There is no guarantee that I am going to or not going to. There is no guarantee either way.

CHAIRMAN JENSCH: Either way.

THE WITNESS: Yes.

CHAIRMAN JENSCH: You couldn't rely really on the figures at all, then, is that correct?

THE WITNESS: No.

CHAIRMAN JENSCH: How can you rely on it if there is no guarantee to it?

THE WITNESS: There is no guarantee in this type of thing. You can only improve yourself and the record is clear that we have improved our success of hatch and rearing success through the years, over the previous years.

I may not understand your question. Would you state it again?

CHAIRMAN JENSCH: Well, I think you have answered it.

What I really had in mind was you feel from now on it is going to be -- everything is going -- using the term, rosy; there are going to be no ups and downs, no jiggles in the line; it will be a straight line onward and upward for greater success. Is that your judgment of the operations?

THE WITNESS: I don't -- would not be able to say whether we are going onward and upward. I don't think we are going backwards. That's what I understood you to say, that we have no assurance that we can do anything next year that we have done last year.

CHAIRMAN JENSCH: No, no. Back to my question of it being a cyclical operation. You are going to have variations in performance just as Moncks Corner couldn't get as much brood stock as they wanted. They aren't going to be able to maintain a sustained, high level of activity at all times; you recognize that, do you not?

THE WITNESS: Yes. I also recognize we will never go back to the early days when we failed because we didn't know what to do, not because of some national cyclical variation.

CHAIRMAN JENSCH: To change the subject, the problem I had with the question that the Hudson River Fishermen's Association asked you, I don't know that I understood the process. He asked you how many female fish you would have to get.

I have some notes you would have to get 225 fish to get 200 eligible females, correct?

THE WITNESS: Correct.

CHAIRMAN JENSCH: How do you do that? Do you use the hook and line or net? How do you -- you count 225 and drop your net and say, Well, I have 225; I can go home now? Is that the way that works? What is the process?

THE WITNESS: It varies between hatcheries. You can start with Moncks Corner.

CHAIRMAN JENSCH: How would you do it on the Hudson River?

I recognize the limitation that you expressed this morning, but how would you propose that they do it in the Hudson River?

THE WITNESS: The least damaging method to catch brood stock which would be the most desirable if it were effective, is electroshocking, where you take a generator with electrodes out in a boat and hang a generator -- the electrodes in the water and create a current in the water around these fish and it narcotizes them and many of them come to the surface. You capture the fish before it regains consciousness.

This is a very desirable way to capture because you don't injure the fish as a rule.

The other method is gill nets where the fish is entangled in the netting material; and it is more or less damaged depending upon the time that the fish remains in the net. If you get the fish out soon after he entered the net, the damage is lessened; on the other hand, if you wait too long, it kills the fish.

CHAIRMAN JENSCH: And is a killed fish usable?

THE WITNESS: No. Not at all. No, sir.

In the Chesapeake Bay they have certain times of the year that they have a seine haul operation where they will catch tons of the big spawning fish as they are running up the river.

That may be a possibility. I don't know about the Hudson. It is a possibility. It occurs in the Chesapeake Bay.

I have done it with hook and line, but that's not too desirable either.

(The Board conferred.)

DR. GEYER: While we are talking about the two Table Ones, one in Applicant's Exhibit 6 and the other in Dr. Goodyear's 23rd April submission, I would like to get clear just what survival means and whether there is a standard that's used among all fishery people as to definitions and ways of measuring survival.

Obviously survival is a matter of time and of stage in the life cycle of the fish; so taking the table end of Exhibit 6 first, I presume that the percent survival is

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in this case from the age of about five days to the age of about 45 days?

THE WITNESS: That's correct.

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DR. GEYER: Looking at the information presented in Table 1 of Dr. Goodyear's submissions, this is from pre-stocking to phase one?

THE WITNESS: That's correct -- or even beyond. I think he has a phase two, does he not?

DR. GEYER: Well, I don't know what the time is on that. Maybe Dr. Goodyear can tell me that?

DR. GOODYEAR: The pre-stocking is variable.

DR. GEYER: What's the time -- the time from hatch to pre-stocking, according to Table 1 in Exhibit 6 -- it is four or five days.

DR. GOODYEAR: In Table 1 of my testimony, that pre-stocking period can go up to 15 days. That's one of the reasons for the high variability.

DR. GEYER: So it is, what, five days to 15 days?

DR. GOODYEAR: Probably more like three days to 15 days.

DR. GEYER: And what is it from phase one to phase two, then?

DR. GOODYEAR: Would be from about three days -- at the beginning -- well, the duration of the period would be something on the order of 30 to 60 days.

DR. GEYER: 30 to 60 days? In addition to the 45 days then that lapsed between pre-stocking and phase one?

DR. GOODYEAR: No. Let me start over again.

DR. GEYER: Okay. Can you put the time intervals across this table?

DR. GOODYEAR: These will have to be approximate. I will check to make sure. The hatch is usually in one and a half to two days.

Pre-stocking would be --

DR. GEYER: One and a half to two?

DR. GOODYEAR: Right.

The pre-stocking period would be about three to, say, 15 days in duration.

Phase one would be 30 to 60 days in duration.

Phase two -- I'd have to check, but I think these figures were 200 days. I'd have to check that.

DR. GEYER: Okay.

A hundred, plus or minus?

Now going back to the head of the table, under brood stock, what does the word "survival" mean?

DR. GOODYEAR: That is the cumulative survival of the group of fish which was collected for spawning. The number of fish that actually died before they were able to --

DR. GEYER: What stage were they selected? Did you pick out -- is this eggs to egg spawn or eggs viable?

DR. GOODYEAR: This is female fish.

DR. GEYER: These are the fish from which the eggs were taken?

DR. GOODYEAR: Right.

DR. GEYER: Well, what does -- the survival then means what? You had so many females and so -- such and such a percentage of them survived?

DR. GOODYEAR: Right. There is some mortality of the females associated with collecting of the females.

A certain other portion of them don't respond to the hormones.

DR. GEYER: So what you are doing, this is your calculation back to get the 225 fish or something like this, needed to produce a certain number of eggs?

DR. GOODYEAR: Right.

DR. GEYER: Okay. Coming back then, is there a standard definition for what is meant by survival?

THE WITNESS: Survival in the hatchery system is simply that which is alive at the end -- at the time you measure it.

DR. GEYER: Is there standard time of measurement, standard weight of measure? I want to know whether these figures are comparable or not?

THE WITNESS: Yes, sir. The -- well, let's start with the female.

That's pretty obvious, nine out of 10.

Then you go to the spawn of the female. You get a million eggs, say, per 17 pound female at Moncks Corner.

DR. GEYER: How do you count a million eggs?

THE WITNESS: You take a sample. I know by sitting down and counting them, I knew that there was perhaps 150,000 eggs per ounce. Having satisfied myself that that was within what an ounce of eggs weighed, I just weighed what I stripped out of the fish and multiply it by 150.

Then when the fish hatched into the little fry in the aquaria, I would simply take a small vial and put it in there and take a sample of the water and count how many fry were in that sample of water and ratio proportion to what was in the total.

Then --

DR. GEYER: Is that done at a standard time, a day, two days?

THE WITNESS: It is usually done at the time that the fry leave the hatchery to be shipped by air to North Carolina.

DR. GEYER: This covers several of these stages here?

THE WITNESS: It covers the pre-stocking only.

All right. Then usually the fry are not estimated again before they are stocked. Even if there is a couple of days delay, unless there is an obvious mortality, they accept the original figure.

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You stock them in a pond. You drain down the pond and you might get 200 pounds of fish at 200 fish per pound by the same sampling method that I used, where you just count out a pound or two to find out how many fish per pound.

DR. GEYER: They're essentially all the same size?

THE WITNESS: All the same size.

So -- even if they aren't, you can still get what's representative per pound, how much does -- just take a sample out of a big pile of fish and count them and do that several times until you know you are getting a pretty good sample, consistent sample, and then the amount of fish per pound multiplied by the poundage you have gives you your estimate of your survival. So while in this case you, of course, don't count every egg, every fry, or every fish, you do it by acceptable sampling methods.

It is an estimate.

DR. GEYER: And then these numbers all seem to be related to the pre-stocking to phase one? Is that a standard time?

THE WITNESS: Actually phase one is a term originated at Edenton National Fish Hatchery. Whether anybody else uses it or how many other hatcheries use that particular term, I don't know.

DR. GEYER: Apparently Moncks Corner does, because --

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THE WITNESS: See, there again we have to keep in mind Moncks Corner is an egg hatchery, but not a rearing station.

DR. GEYER: They don't have any rearing ponds at all?

THE WITNESS: Not at Moncks Corner. They have upstate in Piedmont, they have rearing ponds. They are called some other hatchery, not Moncks Corner.

Edenton is primarily a rearing hatchery, but has in recent years become an egg hatchery. That's both. You can have an egg hatchery, a rearing facility, or both, at the same place, but they don't have to be. It is convenient to have it that way, but it is not a requirement.

DR. GEYER: Well, when you give us additional information you think should go in this table, it would be nice to have some of these things made clear because apparently there isn't going to be any data -- there aren't going to be any data from Moncks Corner on the pre-stocking phase one aspect.

THE WITNESS: That's true. That's correct.

DR. GEYER: Okay. Thank you.

CHAIRMAN JENSCH: Hudson River Fishermen's Association?

MR. MACBETH: No further questions, Mr. Chairman.

CHAIRMAN JENSCH: Regulatory Staff?

Any redirect, Applicant?

MR. TROSTEN: No, Mr. Chairman.

CHAIRMAN JENSCH: All right.

MR. TROSTEN: May we have a brief recess, Mr. Chairman?

CHAIRMAN JENSCH: Yes. At this time let's recess to reconvene in this room at 3:15.

(Recess.)

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CHAIRMAN JENSCH: Please come to order.

Mr. Cohen is here representing the Applicant. We will ask Mr. Cohen to go forward.

Are you ready to proceed, Mr. Cohen?

MR. COHEN: Could we wait a few moments, Mr. Chairman?

CHAIRMAN JENSCH: Yes.

MR. COHEN: Thank you.

(Pause.)

CHAIRMAN JENSCH: Is the Applicant ready to proceed?

MR. TROSTEN: Excuse me, Mr. Chairman.

CHAIRMAN JENSCH: Do you have further interrogation?

MR. TROSTEN: Yes, I do, Mr. Chairman.

CHAIRMAN JENSCH: Please proceed, please.

MR. TROSTEN: Dr. Goodyear --

CHAIRMAN JENSCH: Is this Goodyear or Stevens? Can we excuse Stevens?

MR. TROSTEN: We have no more redirect.

CHAIRMAN JENSCH: Very well.

Thank you, Dr. Stevens. You are excused.

If you will send us the data and calculations, we will appreciate it.

Thank you.

(Witness excused.)

CHAIRMAN JENSCH: Before we proceed with Dr. Goodyear, there was a call that came to my office yesterday from Mr. Corcoran who stated that Mr. Corcoran would like to speak today. I am informed Mr. Corcoran is here and before he proceeds, I might state that it would be very helpful to the Board, Mr. Corcoran, if you could tell us who speaks for the State of New York in reference to the Consolidated Edison Company of New York, Inc., proceeding in Indian Point No. 2.

We understand there is a Department of Conservation that had one view and that the attorney general of the State of New York had another view about, I think, cooling towers.

Now do you have something perhaps from the governor that will resolve these efforts? What is the situation?

MR. CORCORAN: Mr. Chairman, I have nothing from the governor, but I am not aware that there is a difference of opinion with regard to the attorney general's office and the Department of Environmental Conservation.

There may be some differences of opinion with regard to individuals within both organizations but I believe the state policy is one and I believe it was clearly expressed in the memorandum of the state of March 6, 1973, to this Board.

CHAIRMAN JENSCH: In summary it is what?

MR. CORCORAN: In summary, it is that the state wants Con Edison to install cooling towers at its Indian Point No 2 plant.

Now the state has instituted a lawsuit against Con edison and we held negotiations on this very point last November in which we formally requested Con Edison to install such cooling towers.

We were not able to resolve this dispute, and we are preparing that case for trial now.

As the attorneys for the State of New York, I think we do represent the interests of the people of the State of New York and that there is no difference of opinion with regard to cooling towers.

Now I think it is very important that this Board require as a condition of the license that Con Edison install cooling towers, because although the state is prepared to go ahead with its trial, this trial will be a long and costly process duplicating much of the evidence that has been submitted to this Board and will undoubtedly drag out for a number of years.

CHAIRMAN JENSCH: Down here we use the term it may be expedited over a period of time.

(Laughter.)

CHAIRMAN JENSCH: You understand there is a difference in jurisdiction under the Water Pollution Control Act amendments as to which we would like to ask the Applicant later to respond to your statement, but as we understand the situation with the Water Pollution Control Act amendments, and the interim policy statement which the Atomic Energy Commission has signed with the Environmental Protection Agency, the matter of thermal releases will depend to a large degree upon what the state action is undertaken on thermal releases.

As you know the Water Pollution Control Act amendments prohibit the Atomic Energy Commission from attaching a condition based upon the interim conditions.

Now is the basis of the lawsuit by the State of New York based upon a thermal situation?

MR. CORCORAN: No, Mr. Chairman. Well, it is based partly on the thermal situation, but it is based also on the adverse effects of impingement and entrainment of passive organisms.

We believe thatthe entrainment is perhaps the most serious of the environmental problems posed by the once-through cooling system, and I think the evidence of the AEC Staff strongly supports this position with regard to these striped bass populations in the estuary.

I might say in following up what I said before, Mr. Chairman, that while there is no lack of desire on

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the part of the state and the state's attorneys to act and act vigorously in our court suit, if this Board does not require cooling towers, the state will certainly go ahead and press its suit, but even if ultimately successful, it will probably put off the day when cooling towers are constructed by, you know -- for many years.

That is why we feel that this AEC hearing is so important, and we hope that cooling towers will be required.

CHAIRMAN JENSCH: Upon what basis do you believe that this Board should recommend, direct the construction of cooling towers, based upon the jurisdiction that is before the -- that is exercised by the Atomic Energy Commission and its subordinate agencies?

MR. CORCORAN: Well, you mean under the National Environmental Policy Act which requires the Atomic Energy Commission to consider all of the environmental factors which the plant poses to the environment.

CHAIRMAN JENSCH: What factors do you believe justify the direction by the Atomic Energy Commission for the construction of cooling towers?

MR. CORCORAN: We believe if cooling towers are not installed and the once-through cooling system is permitted to exist, that the fisheries of the Hudson River will suffer irreversible damage.

CHAIRMAN JENSCH: In what way? How?

MR. CORCORAN: Well, for example, through fish impingement, large numbers of the white perch population will inevitably be destroyed.

CHAIRMAN JENSCH: Do you have studies in that regard?

MR. CORCORAN: We are undertaking studies now, Mr. Chairman.

Some studies are going to be undertaken this spring and summer with regard to the problem of entrainmen t of passive organisms. These studies are not now complete.

CHAIRMAN JENSCH: Well, that I understand is the position of the Applicant here, that these studies are not yet complete and no decision should be made on cooling towers until the studies are completed.

What is your view about the necessity of completing your studies before making your recommendation?

MR. CORCORAN: Well, we feel, Mr. Chairman, that there is sufficient evidence to --

CHAIRMAN JENSCH: In what respect?

MR. CORCORAN: -- to justify our position that irreversible harm will be done, and that if Con Edison is permitted a five-year study period and permitted to operate a once-through cooling system on the Hudson River for five years that irreversible damage will be done, and if cooling towers are then installed five years later, there may not be many fish left to protect.

CHAIRMAN JENSCH: Well, I understand your statement and the conclusions you have given. I think what we are looking for is what facts in this record do you believe justify the direction to construct cooling towers at Indian Point No. 2?

Now we have had -- you have heard here this afternoon since you have been here, consideration about striped bass, although principally I think we have been talking about hatches. I don't know whether you follow the transcript or not.

There has been some discussion about the facilities that Con Edison has constructed. You have got air bubblers outside of screening arrangements; they have got resolving screens.

They are counting and measuring and weighing and examining and dissecting, computerizing, I guess.

MR. CORCORAN: We know of no system, Mr. Chairman, which will solve the problems of impingement, entrainment, and thermal pollution as well as the cooling towers will.

Now the air bubbler system may help reduce the incidence of fish impingement, but I don't believe it will have much affect on entrainment or on thermal pollution problems.

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CHAIRMAN JENSCH: Of course, now, if you will just dismiss for the moment the thermal problem, bearing in mind the more limited jurisdiction that the Atomic Energy Commission has by virtue of the Water Pollution Control Act amendments, what facts can you point to within this record that you believe justify the direction to construction cooling towers at this time?

MR. CORCORAN: Well, I think the most significant fact that I have seen in the testimony and in the record is the project shown of a 30 to 50 percent reduction in the annual reproduction of striped bass in the Hudson River estuary.

CHAIRMAN JENSCH: Can you point to where that appears in the record? If you can't do it now, sir, would you send it to us?

MR. CORCORAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Have you considered making any evidentiary presentation in this case at all?

MR. CORCORAN: Not at this time, Mr. Chairman, no.

CHAIRMAN JENSCH: Well, time is getting pretty short.

MR. CORCORAN: I know, Mr. Chairman.

I might say --

CHAIRMAN JENSCH: It is only 3:30 now, if you would like to take a short recess and deliberate a bit. I don't

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know how long this will really run.

MR. CORCORAN: I might say that I think that the evidence presented by the AEC Staff and by the Hudson Fiver Fishermen's Association is quite substantial and that we concur in that evidence; and I think whatever we would have to offer would be repetitive at this point.

CHAIRMAN JENSCH: Will you submit a brief analysis of the record as you see it for your position in this proceeding?

MR. CORCORAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Do you get copies of the transcript, for instance?

MR. CORCORAN: We don't have complete copies, no. We have some parts of it, but I think we can obtain the information necessary.

CHAIRMAN JENSCH: I think it would be very helpful to the Board if the State of New York would reflect its use with a brief or with proposed findings and conclusions with specific references to the record which you believe justify the exercise of jurisdiction for the construction of cooling towers.

Will you do that?

MR. CORCORAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Thank you.

Anything further you would like to add?

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MR. CORCORAN: I would also like to comment on the question as to when the cooling towers should be completed. I understand that Dr. Westman testified on April 6 or submitted a memorandum at that time in which he stated that cooling towers should be completed as soon as practicable, but no later than January 1, 1978.

Again, Mr. Chairman, we are concerned that if the once-through cooling system is permitted to be employed for the next four-and-a-half years, that it may have irreversible adverse effects on the fisheries of the river and that Con Edison should be required to complete its construction as soon as possible.

CHAIRMAN JENSCH: By that you mean -- what time do you believe would be as soon as possible, or could be accomplished? Do you have a time period?

MR. CORCORAN: I have a time period now, Mr. Chairman; but I would say if it is going to take until January 1, 1978, that the Board consider requiring the Applicant to close its plant during the month of June when it poses the most serious threat to the fish eggs and larvae in the estuary or perhaps take some other steps which will help minimize the effect of the plant on the fisheries during certain critical periods of the year.

CHAIRMAN JENSCH: Well, we encourage all parties, of course, to present evidence of their views. This is a pretty

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sharply contested phase of it. If the State of New York can get some presentation of evidence before the likely close of this evidentiary session, we would welcome the presentation, because I think the State of New York has taken a very positive view about this situation; and if you have evidence from an engineering point of view, any other phase, I think it would be very helpful to the Board.

If you feel the record is sufficient, as made by the parties, of course, you can rest upon that presentation.

MR. CORCORAN: I will endeavor to have that presentation made to the Board, Mr. Chairman.

CHAIRMAN JENSCH: Thank you.

MR. CORCORAN: I might say our resources are not unlimited. We have only a handful of attorneys in the Attorney General's office who work in this -- in the environmental field; and for whatever it is worth, we intend to take a more active role in the Indian Point 3 proceedings and assign an attorney to work on that full-time.

CHAIRMAN JENSCH: Thank you.

MR. CORCORAN: Thank you, Mr. Chairman.

CHAIRMAN JENSCH: Dr. Geyer has some questions.

DR. GEYER: I only have one question for clarification. When you use the term irreversible adverse effects, do you mean that there's danger that fish will be wiped out and could never recover? Or do you mean there is something going to be

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lost and that once it is lost, that it can't be gotten back? In other words, certain number of fish are not produced in a certain year because of the plant and those fish are gone; and obviously they are not recoverable?

MR. CORCORAN: I mean that either a species could be wiped out or that it could be substantially reduced to the point where it will never reach its present level.

DR. GEYER: Do we have any evidence that that sort of thing happens?

I know there are few endangered species which for a variety of reasons may become extinct; but in general it has all seemed to me if you remove pollution from a stream, the shad come back, for example, coming up the Delaware, this sort of thing?

MR. CORCORAN: I don't know if that is so with regard to all species. I would think if a species population is reduced, for example, by 90 percent, and you are left with only 10 percent of what you originally had, it seems to me that that is -- that species has been irreversibly damaged; and certain future stresses may further endanger that species.

DR. GEYER: That is certainly true, but --

MR. CORCORAN: I am not really qualified to talk about it from such a scientific point of view.

DR. GEYER: Well, I think the record doesn't show

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very clearly the basis for which it -- the irreversibly adverse effects, is claimed.

MR. CORCORAN: It seems to me if the annual reproduction of a species is reduced by 30 to 50 percent and this continues over a period of years, that there is going to be an irreversible effect.

DR. GEYER: Well, unless something is done to stop the cause of the reduction.

MR. CORCORAN: Yes.

DR. GEYER: Presumably something was going on prior to 1932; and then striped bass for some reason came back, you see, from almost being extinct to a very significant population.

MR. CORCORAN: I am not sure we can guarantee that if a species is reduced to such a level, that it will inevitably come back up to its present most productive level.

DR. GEYER: Okay. Thank you.

MR. CORCORAN: Thank you.

MR. BRIGGS: In the discussions that we have had about cooling towers and schedules for cooling towers, there are two aspects that affect the schedule substantially.

One has to do with studies that Con Ed believes should be made to assess the environmental effect; and another has to do with the time required for state and federal groups to review the designs and to provide the necessary permits to construct cooling towers.

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Are there mechanisms whereby these reviews by state groups and the permits by state groups can be expedited to make it possible to construct these towers more rapidly?

MR. CORCORAN: Yes, sir, I think that it can be expedited. We have had discussions with the State Department of Environmental Conservation and based upon those discussions I believe the answer is yes to that question.

MR. BRIGGS: It might be helpful if you could address that point in the brief that you propose to provide.

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MR. CORCORAN: Fine.

MR. BRIGGS: Also I believe in the statement by the Attorney General, Con Ed was taken to task for suggesting that one might manage the fishery and one might introduce new species that would be more desirable than the species that are presently there. Is it the State of New York's position that one cannot improve the environment by management of the present species and by introducing new species into the Hudson River?

MR. CORCORAN: I think we would question the use of the word improve in that context. The laws of the state reflect the state's policy that what must be done first is to preserve the indigenous species; and if there is some room for improvement after that, well, improvement should be made; but that it is a contradiction in terms to play games with a species survival and call this improvement and seek to

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substitute other species under the guise of improving the ecosystem. We don't believe that that is improvement.

MR. BRIGGS: Then you would say if it were shown that the -- let's say, reducing the population of white perch by 80 percent resulted in a large increase in the population of striped bass, that this would not be a good thing to do? That this would be forbidden under the laws of the state?

MR. CORCORAN: Well, I think yes, it would be; it would be prohibitive to reduce a species to that level, yes.

CHAIRMAN JENSCH: Any further questions?

If not, Mr. Corcoran, thank you for your appearance here today and your presentation.

Will the Applicant give us a general response?

MR. TROSTEN: Yes. Mr. Chairman, in the first place, I would like to point out that contrary to the impression one might obtain by Mr. Corcoran's remarks, there very definitely are two positions expressed by representatives of the State of New York in this proceeding. The New York State Atomic Energy Council is a party to this proceeding and has filed comments on the draft environmental statement through the Department of Environmental Conservation. Contrary to the impression one might get from Mr. Corcoran's statement, the New York State Atomic Energy Council has not taken the position that cooling towers should be required for Indian Point.

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CHAIRMAN JENSCH: Are they opposed to cooling towers?

MR. TROSTEN: No, sir. They have taken no position on this matter.

CHAIRMAN JENSCH: Where is the conflict?

MR. TROSTEN: The conflict that I perceive is that Mr. Corcoran stated that the state's position was that there should be cooling towers required for Indian Point 2 and that is not the case, Mr. Chairman. The position of the Office of the Attorney General is that cooling towers should be required for Indian Point 2; but as far as the record is concerned, there is no position stated in this proceeding by the agencies of the State of New York that there should be cooling towers required for this facility. They have taken no position on this matter; and if Mr. Corcoran can point -- can address this matter and point to a particular document in this record, that indicates that the agencies of the State of New York have taken the position that cooling towers should be required for Indian Point 2, I wish he would do so.

CHAIRMAN JENSCH: Are you suggesting that the Attorney General of the State of New York cannot reflect the position of the State of New York?

MR. TROSTEN: I am suggesting that it does not reflect the position of the State of New York, Mr. Chairman, because there is nothing that indicates that the agencies

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of the State of New York are favoring cooling towers for Indian Point 2; and the position of these agencies has been set forth in the comments on the Draft Environmental Statement.

CHAIRMAN JENSCH: And they don't oppose cooling towers?

MR. TROSTEN: They neither favor nor oppose cooling towers.

CHAIRMAN JENSCH: Lacking any statement either way, the only representative of the State of New York is the Attorney General who says yes, construct cooling towers.

MR. TROSTEN: There is a representative of the State of New York that favors cooling towers. That is the Attorney General. He does not speak for the other agencies -- for the agencies of the States of New York, who are represented by the New York State Atomic Energy Council.

CHAIRMAN JENSCH: Maybe they don't count as much as the Attorney General.

MR. CORCORAN: Mr. Chairman, the Attorney General is presenting this suit in State Supreme Court, an injunction sought to require Con Edison to install cooling towers at its Indian Point 2 plant. The suit is not entitled the Attorney General of the State of New York against Con Edison. It is entitled the State of New York versus Con Edison.

The Attorney General represents the interests of the state. The position of the state has been made clear

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in this suit; and Mr. Trosten knows this well since it is his law firm which is defending the suit, representing the defendant in the suit.

MR. TROSTEN: Mr. Chairman, I don't think it is necessary -- I don't think it would be profitable to get into an extended discussion of the various responsibilities under the statutes of the State of New York. The Attorney General has his New York State constitutional and statutory responsibilities and the agencies of the State of New York have their responsibilities.

CHAIRMAN JENSCH: If they are not opposing cooling towers, the only positive declaration is by the Attorney General, I infer?

MR. TROSTEN: As I say, Mr. Chairman, they are neither favoring nor opposing them.

CHAIRMAN JENSCH: We set them aside. They are not standing up to be counted in the matter.

MR. TROSTEN: We are left with the question of whether the Attorney General does indeed represent the position of the people of the State of New York or not. I think that is really the question.

CHAIRMAN JENSCH: How do you feel about the lawsuit about -- what is it, five dollars a fish, you are up to a million and a half? The Attorney General represents --

MR. CORCORAN: That is a different lawsuit, Mr.

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Chairman.

CHAIRMAN JENSCH: Still the Attorney General representing the State of New York there, isn't it?

MR. TROSTEN: The Attorney General is representing the state as the chief legal officer of the state. It is our view that in these various proceedings which the Attorney General has brought against Con Edison in the courts of the state that the ultimate resolution will be favorable to Con Edison. The most recent decision of the Appellate Division has been to reverse the decision of the lower court.

CHAIRMAN JENSCH: While we enjoy these probability concepts, let's just confine ourselves to the one question, who is representing the State of New York. That is all we are interested in at the moment.

The State of New York -- you are glad to have the Attorney General representing the state there because you feel you are going to overcome the situation; but -- on the cooling towers you say he can't represent the people?

MR. CORCORAN: May I make a comment about the penalty suit, Mr. Chairman? It was upon the request of Commissioner Diamond, the Commissioner of Environmental Conservation that that suit was brought for $1.6 million. It is totally false to imply that it was the Attorney General who thought up this suit and who acted without cooperation from other state agencies.

13mil

By the way, when he says the decision was reversed, the court did not hold that the statute did not apply. The court merely said you had to prove certain things before the defendant could be found liable. But the court did not say that the statute did not apply to their activities.

CHAIRMAN JENSCH: Excuse me for interrupting. Please proceed.

MR. TROSTEN: Mr. Chairman, with respect to the evidentiary presentation that's been made and the comments by Mr. Corcoran on this, I think that it is really very important to bear in mind that the participation by the Attorney General's office has been really that of commenting on evidence presented by other people.

Now, for example, Mr. Corcoran referred to testimony earlier testimony by the Regulatory Staff that projected a 30 to 50 percent reduction. If he had inspected the more recent testimony of the Regulatory Staff, for example, he would see that the Regulatory Staff has now reduced that estimate to 14 to 43 percent; and this gives you an example of the need to pay close attention to the evidentiary presentation as it appears and as it evolves in this unfolding proceeding.

CHAIRMAN JENSCH: Something like the monkey in space theory?

MR. TROSTEN: Yes, that was the earlier version and

Dr. Goodyear's more refined calculations have reduced it on that order, from 40 to 14 percent.

As far as the time for approval by state agencies are concerned, we would be delighted to learn the Attorney General's views as to the amount of time that could be required for approvals by various state agencies and we would certainly hope that any and all approvals that have to be obtained for this facility will be expedited through the good offices of the Attorney General.

On the other hand, the experience that the company has had with regard to securing approvals from those agencies, not the Office of the Attorney General, that are legally required to give those approvals, does not afford us a great deal of optimism as far as the scheduling necessary to obtain the various approvals from these New York state agencies. As I say, if the Attorney General can expedite these matters, the company would be most grateful.

I have no further comments on this, Mr. Chairman.

CHAIRMAN JENSCH: Very well.

Let us proceed with cross-examination.

Regulatory counsel care to make a statement?

MR. KARMAN: No, not at this time, Mr. Chairman.

CHAIRMAN JENSCH: Hudson River Fishermen's Association?

MR. MACBETH: No.

15mil

MR. KARMAN: Mr. Trosten, might I ask at this time on what you are going to cross-examine Dr. Goodyear?

MR. TROSTEN: Solely with regard to his testimony dated April 23, 1973.

MR. KARMAN: It might be best if I offered it in evidence.

MR. TROSTEN: Mr. Karman, I was under the impression that you had offered this in evidence yesterday as the response to Mr. Briggs' question?

MR. KARMAN: No, I held it back.

MR. TROSTEN: I see.

CHAIRMAN JENSCH: Proceed.

MR. KARMAN: Might I at this time make our entire offer of evidence?

CHAIRMAN JENSCH: Yes, proceed, please.

MR. KARMAN: On behalf of the Regulatory Staff, I would like to offer in evidence, one, a document entitled, "Staff Analysis of Artificial Propagation to Replace Hudson River Fishes Killed by Power Plant Operation," by C. Phillip Goodyear, 23 April 1973.

Two, a redirect rebuttal testimony of Dr. C. P. Goodyear clarifying April 10, 1973, testimony on Staff comments on Applicant's research program dated April 24, 1973.

I might add, Mr. Chairman, that copies of all these documents have been served upon the Board and the parties.

16mil

MR. TROSTEN: May I inquire, Mr. Karman, whether the second document you just mentioned is an attachment?

MR. KARMAN: No. It should not be attached. It should be separate. It was hooked up inadvertently.

MR. TROSTEN: I see.

MR. KARMAN: No. No. It is attached to the Staff response, Dr. Geyer. It is in the back of this.

16

DR. GEYER: Okay.

MR. KARMAN: No. 3 will be set one of Staff responses to Applicant's interrogatories on supplemental Staff testimony on the feasibility of a fish hatchery and replacement by stocking of fish in the Hudson River.

Set two, Staff responses to interrogatories for Dr. C. P. Goodyear re: Staff comments on Applicant's research program.

Then there are, Mr. Chairman, four additional documents which will support the testimony of Dr. Goodyear with respect to the analysis.

CHAIRMAN JENSCH: Will you identify them, please?

MR. KARMAN: Well, one of them is entitled "Distribution of Coastal Recapture of Cheapeake Tagged Striped Bass" -- let me withhold these four documents until we are ready to use them. It might be easier to do it at that time.

Then I have in addition to that, Mr. Chairman, a document entitled "Request for Additional Information from the Regulatory Staff to the Applicant" and the responses of Dr. John P. Lawler on the additional information requested by the Staff on the temperature distribution section of the March 1930-1973 Applicant testimony on cumulative effects of Bowline, Roseton, and Indian Point generating stations on the Hudson River dated April 20, 1973.

With the exception of the four documents which I will offer subsequently, Mr. Chairman, I now --

MR. TROSTEN: May I interrupt a moment?

Are you going to offer into evidence the Staff's answer to the interrogatory of Mr. Macbeth on xenon?

If you are not, I will do so.

MR. MACBETH: I didn't. I am going to make some objections if he does offer it in.

MR. KARMAN: I had no intention of offering it.

CHAIRMAN JENSCH: Let's get back to the offer that has been made.

MR. KARMAN: Mr. Chairman?

CHAIRMAN JENSCH: Have you completed?

MR. KARMAN: Yes, I have.

I request that the aforementioned documents from the Regulatory Staff be admitted as evidence in this proceeding and that it be physically incorporated in the transcript of today's proceedings as if read.

CHAIRMAN JENSCH: Very well.

The four categories of documents identified by Staff counsel starting with the Staff analysis of artificial propagation to replace Hudson River fishes killed by power plant operation, and the fourth category, the request for additional information, and the responses of John P. Lawler, having thus been identified, is there any objection by the Applicant to having them physically incorporated within the transcript?

MR. TROSTEN: No objection.

CHAIRMAN JENSCH: Hudson River Fishermen's Association?

MR. MACBETH: No objection.

CHAIRMAN JENSCH: Very well, the request of Staff counsel is granted and the four categories identified by Staff counsel --

MR. KARMAN: Mr. Chairman, it has come to my attention there is one typographical error --

CHAIRMAN JENSCH: Let me get these in the record.

The request of Staff counsel is granted and the four categories of documents described by Regulatory Staff counsel may be physically incorporated within the transcript as if orally given and shall constitute evidence on behalf of the Regulatory Staff.

(The documents follow:)

STAFF ANALYSIS OF ARTIFICIAL PROPAGATION TO REPLACE HUDSON RIVER FISHES KILLED BY POWER PLANT OPERATION

C. Phillip Goodyear

23 April 1973

I. Introduction

In earlier testimony[1], the Staff presented a brief discussion of the feasibility of maintaining fish stocks in the Hudson River through hatchery replacement of fish killed by power plant operations. It was concluded that artificial propagation is not a reasonable means of maintaining fish stocks because mass culture techniques are not available for most species of estuarine fishes which will be killed by plant operation. In the particular case of striped bass, the Staff concluded that the required size of the hatchery operation would make successful employment of this alternative a virtual impossibility with the present state of the art.

The following analysis is presented in response to a request by Mr. Briggs that the Staff present a more detailed explanation of its conclusions related to the magnitude of the hatchery program which would be required to replace bass removed by the operation of Indian Point Units 1 and 2.

II. Objectives of Artificial Propagation of Striped Bass

The Staff assumes that the sole objective of the proposed artificial propagation is to compensate for reduced survival resulting from Indian Point operations, i.e., the number of bass recruited from the Hudson nursery grounds is maintained at pre-operational levels. Thus, if Indian Point removed sufficient larvae and juveniles to reduce the annual production by say 10 million bass, an offsetting increase of 10 million bass would be required from hatchery production.

If annual production is planned to be maintained at a pre-operational level, then the hatchery operation will be required to offset the cumulative effects of all power plants on the Hudson, rather than just Indian Point. The reason for this conclusion is that a reduction in annual survival that is caused by the operation of other plants will have a negative influence on the spawning stock after one generation. Thus, the hatchery would be required to exploit an annually increasing proportion of the available brood stock to maintain a constant production. Obtaining wild brood stock under these circumstances would become progressively more difficult and finally impossible.

III. Limitations on Hatchery Production

The most important problem which must be resolved in connection with a large-scale program of replacement stocking of bass is how to obtain sufficient reproductive material. The Staff believes that this problem imposes the major limitation on hatchery production, particularly since present plans call for collecting wild brood stock from the Hudson River. This conclusion results from two considerations, i.e., 1) the technical feasibility of collecting brood stock, and 2) the relative efficiencies of natural and hatchery production.

Another potentially important factor is that the hatchery must replace both the power plant losses and the reduced natural production due to the removal of reproductive stock by the hatchery. For example, assuming that hatchery and natural production efficiencies are equal and that the hatchery replaces only a 50% entrainment loss, then the resulting production of fish is only 75% of baseline conditions:

where n = natural production efficiency, i.e., the ratio-yearlings/eggs

eggs = total egg production

h = hatchery production efficiency, i.e., the ratio yearlings/egg exploited

Baseline recruits = (n) (eggs)

Recruits resulting from assumed conditions

Total recruits = river production and hatchery production

= river production + (0.5) (n) (eggs)

= (0.5) (n) (eggs - 0.5 eggs) + 0.5 (n) eggs

= 0.5 (n) (0.5 eggs) + 0.5 (n) (eggs)

= 0.75 (n) (eggs)

= 75% of Baseline

It should be clear at this point that the relative efficiency of hatchery production (h/n) is the most important factor which controls the feasibility of the hatchery proposal. The determining parameters h and n are evaluated below.

IV. Hatchery Return per Egg Exploited

A. Breakdown of losses within the hatchery.

Losses of potential stocklings occur at every point in the hatchery operation. For this analysis, we have categorized the losses to six separate divisions corresponding to various phases of hatchery techniques. Two of these categories are associated with obtaining sufficient eggs for the hatchery operation through exploitation of the brood stock. This includes mortality of brood stock prior to ovulation, and the failure to successfully obtain ova for fertilization and subsequent rearing. The major causes of these losses include factors such as premature ovulation and overripeness.

Once the eggs are spawned, the losses can be equated with mortality of various life stage components. In the present analysis, four life stage components were selected which correspond to particular facets of the culture technique. These include hatching success, pre-stocking survival, Phase I survival, and Phase II survival. Pre-stocking survival represents the survival of larvae between the time they are hatched and the time they are stocked into rearing ponds. Phase I survival includes the first stage in the culture technique and results in the production of fingerlings ranging in length from about 1.5 to 3 in. (presumably this is the size to be stocked in the Hudson River). Phase II survival is applied to fish restocked after being harvested from Phase I and thereby represents an estimate of the survival of 2 in. fingerlings which follows stocking. This estimate is considered to approximate theupper limit of stockling survival.

B. Estimate of the efficiency of hatchery production.

In order to obtain an estimate of the efficiency of hatchery production (h), the Staff analyzed available literature which was at hand. This information is presented in Table 1 below. Data from the several sources were analyzed in two ways. First, the data presented were tallied according to the category of loss. The mean loss of brood stock from each of the categories was subsequently obtained and the cumulative survival was estimated from the mean values. From this technique, the value of the efficiency of hatchery production (h) was estimated to be .0081. The second procedure was to obtain cumulative survival for each of the rows tabulated. Four estimates, ranging in value from 0.0 to .046, were obtained in this manner; however, by assuming a Phase II survival for the Richmond Hill data of 0.5, three more estimates of "h" were obtained. The mean of these 7 factors was .0097, which agrees fairly closely with the value of .0081 obtained from the means of the columns. However, the values which were used in the .0081 estimate are thought to be more accurate. The value for the efficiency of hatchery production (h) is taken to be .0081.

Table 1. Estimation of efficiency of hatchery production (h) from published data. Numbers enclosed in parentheses are assumed values and included in the column means. Data denoted by an * are assumed values that are not included in the column means.

Hatchery	Year	Brood Stock Lbs.	Brood Stock Survival	Brood Stock Fraction Spawn	Stage Survival Hatch	Stage Survival Pre-Stocking	Stage Survival Phase 1	Stage Survival Phase 2	Cumulative Survival of (h)	Reference
Weldon	65				.700					1
					.557					
	66	1572	.60	.86	.270					2
Monck's Corner	62	2655	.75		.073					3
	64	6500	.88	.94	.310					4
Fayetteville	65		.09	1.00	.067	Data from 107 females from commercial fishermen				1
			1.00	.18	.100					
Richmond Hill	68	354	.58	.75	.515	(1.0)	.063	.5*	.007	5, 6
	69	421	.50	.86	.169	.01	.046	.5*	.00002	
	70	328	.88	.50	.009	(1.0)	.141	.5*	.00028	
Edenton	69	233	1.0	.92	.018	0	-	-	.0	7
		102	1.0	.83	.491	.90	.264	.47	.046	
		65	1.0	.79	.434	.92	.027	.45	.0038	8
	70	104	1.0	.80	.131	.75	.229	.61	.011	
							.357	.45		
John Hogan	66	Larvae from Weldon				.94	.170			9
Auburn	71	Larvae from Monck's Corner and Richmond Hill				.44	.155			10
						(1.0)	.107			
Durant, Oklahoma	67	Larvae from Monck's Corner					.094			11
	68						.054			
	69						.079			
Mean			.77	.77	.275	.696	.146	.49	.0097	
Cumulative survival (h)			.77	.59	.16	.11	.017	.0081		

1. Tatum, B. L., J. D. Bayless, E. G. McCoy, and W. B. Smith. 1965. Preliminary experiments in the artificial propagation of striped bass, Roccus saxatilis. Proc. 19th Ann. Conf. S. E. Assoc. Game & Fish Comm., p. 374-389.

2. Smith, W. B., W. R. Bonner, and B L. Tatum. 1966. Premature egg procurement from striped bass. Proc. 20th Ann. Conf. S. E. Assoc. Game & Fish Comm., p. 324-330.

3. Stevens, R. E., and J. C. Fuller, Jr. 1962. A preliminary report on the use of hormones to ovulate striped bass, Roccus saxatilis (Walbaum). Proc. 16th Ann. Conf. S. E. Assoc. Game & Fish Comm., p. 222-235.

4. Stevens, R. E. 1966. Hormone-induced spawning of striped bass for reservoir stocking. Prog. Fish-Culturist, Jan. 1966, p. 19-28.

5. McBay, L. G. 1972. Progress report on fry production and rearing of fingerling striped bass, Morone saxatilis (Walbaum), at Richmond Hill Hatchery, Georgia. Contrib. Ser. No. 4, Sport Fish. Div., Ga. Game & Fish Comm., 27 p.

6. McBay, L. G. 1970. A preliminary report on fry production and rearing of fingerling striped bass, Morone saxatilis (Walbaum), at Richmond Hill Hatchery in Georgia. Contrib. Ser. No. 2, Sport Fish. Div., Ga. Game & Fish Comm., 33 p.

7. Ray, R. H., and L. J. Wirtanen. 1970. Striped bass, Morone saxatilis (Walbaum): 1969 report on the development of essential requirements for production. U. S. Dept. of Interior, Bur. Sport Fish. & Wildl., 46 p.

8. Wirtanen, L. J., and R. H. Ray. 1971. Striped bass, Morone saxatilis (Walbaum): 1970 report on the development of essential requirements for production. U. S. Dept. of Interior, Bur. Sport Fish. & Wildl., 37 p.

9. McGill, E. M., Jr. 1966. Pond water for rearing striped bass fry, Roccus saxatilis (Walbaum), in aquaria. Proc. 20th Ann. Conf. S. E. Assoc. Game & Fish Comm., p. 331-340.

10. Reeves, W. C., and J. F. Germann. 1972. Effects of increased water hardness, source of fry and age at stocking on survival of striped bass fry in earthen ponds. Proc. 26th Ann. Conf. S. E. Assoc. Game & Fish Comm., p. 542-548.

11. Harper, J. L., and R. Jarman. 1971. Investigation of striped bass, Morone saxatilis (Walbaum), culture in Oklahoma. 25th Ann. Conf. S.E. Assoc. Game & Fish Comm., p. 501-512.

C. Reliability of estimate.

It is the Staff's belief that the data presented in Table 1 provide a reasonable approximation of the experience of the various hatcheries, and thus would provide a reasonable estimate of the potential exploitation of spawning stock by a hatchery designed for production at the same approximate magnitude as operating hatcheries. In extrapolating these data to predict the ultimate usefulness of hatchery production, it is necessary to consider the fact that the estimates of brood stock survival, fractional spawn, and the survival of larvae through Phase II are each likely to be overestimates. Thus, the Staff believes that the value of .0081 overestimates the true survival which would be encountered in an operation as proposed by the Applicant.

V. Efficiency of Natural Reproduction in the Hudson River

A. General comments

In comparing natural production to hatchery production, several assumptions have been made. For instance, the percentage of eggs which are produced by the population that are spawned is assumed to be the same in both hatchery and natural reproduction. It is also assumed that fishing mortality has no effect on the relative survival estimate, i.e., the losses due to capture of spawning stock by the spring shad fishery is not a component of the computed loss incurred during natural reproduction. This point is only important to the extent that hatchery operations can utilize commercially caught striped bass in their egg procurement. This assumption is discussed in a later section of this testimony.

B. Estimate of survival of Hudson River spawned eggs.

The Staff presented estimates of the number of eggs spawned in the Hudson River in Table A-V-3 of the FES. The estimates for 1966 and 1968 were about 2 billion eggs in each year.* In 1967, the estimate was 360,000 eggs spawned; however, the 1967 egg concentration estimates are based on data derived from sampling with gear which was significantly less efficient than that of the previous and following years:

> . . .Routine comparisons of selected net dimensions and mesh openings (Fig. 5) indicated that the standard hoop with a 0.020 X 0.031 inch mesh was a more efficient sampler of eggs per volume strained than either the finer mesh in the hoop or the standard mesh in a larger net . . .
>
> In 1966 the mesh opening of the nets used at Cornwall for egg sampling measured 0.020 X 0.031 inches. The scarcity of newly hatched larvae in samples collected in early June suggested that they might have passed through the mesh, hence nets with a mesh opening 0.012 X 0.020 inches were used for the balance of the season. This finer mesh net was used throughout the estuary in 1967 until early July. It was then replaced by nets of progressively larger mesh size in an attempt to collect late larval or early juvenile stages not collected in 1966. The 0.020 X 0.031 inch mesh was used for all routine egg and larvae sampling in 1968 because clogging of the finer mesh used in 1967 resulted in severe reduction of volume strained. . .
>
> (Hudson River Fisheries Investigations 1965-1968, p. 12)

The 1967 data were used to estimate the efficiency of natural reproduction in the Hudson. Reduction of the 1967 spatial and temporal egg collection data results in a total egg production estimate of approximately 360 million, a value known to be biased on the low side. Such negative bias in the egg production estimates would tend to inflate corresponding estimates of the efficiency of natural production. However, larvae may actively escape the plankton nets causing a corresponding negative bias in the estimate of larval abundance. If the two sources of bias are of equal magnitudes, then their effect will be cancelled in estimating natural reproductive efficiency. However, if the larvae are being sampled effectively, then the negative bias

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*It must be pointed out again that these numbers are only interpretable in regard to their relative magnitude. They should not be taken as accurate estimates of the absolute value of the parameters which they are used to evaluate.

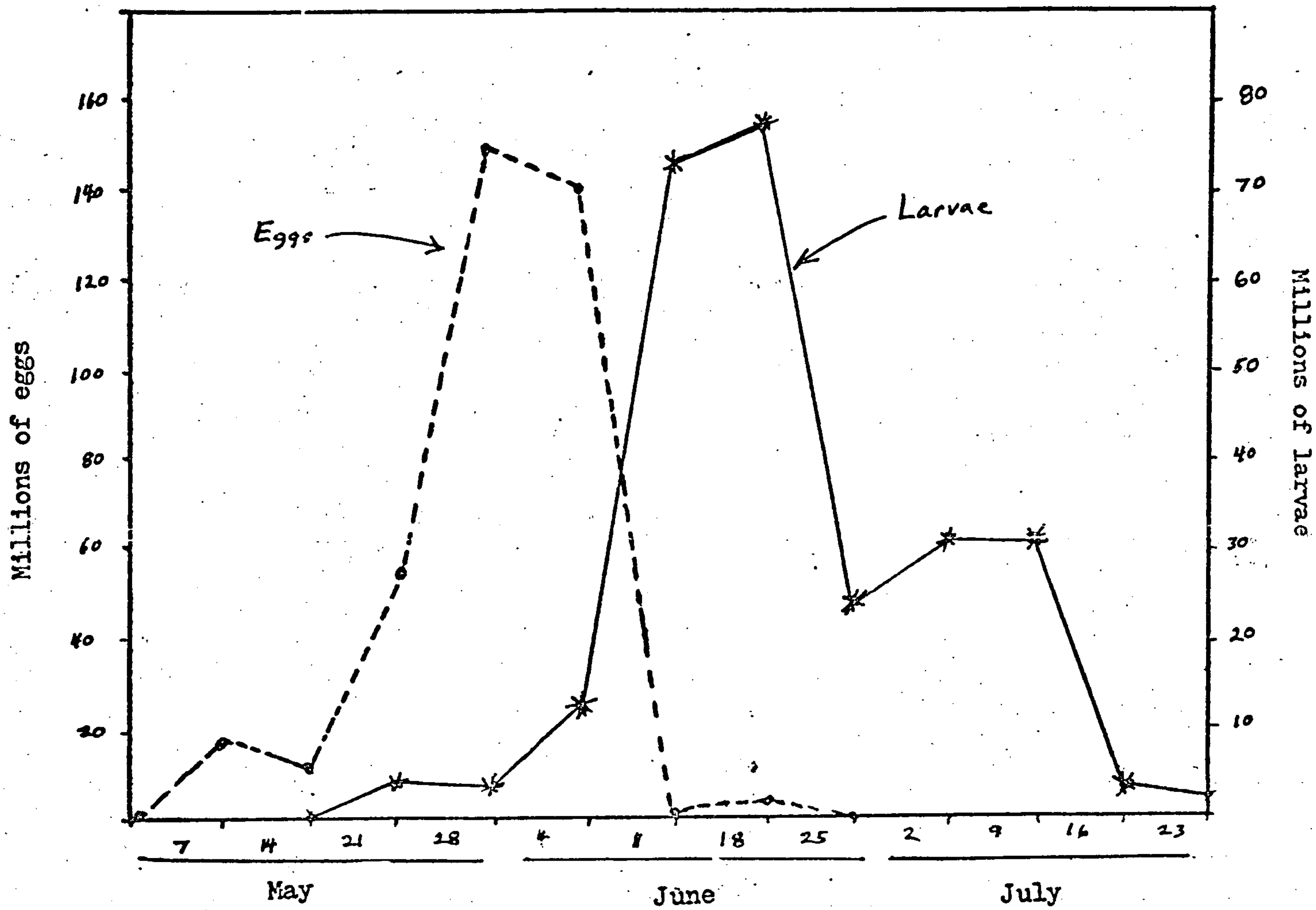
in the estimate of egg production results in serious errors in the estimate of the efficiency of natural reproduction, and it would be more appropriate to use a value similar to the other two years which were sampled, i.e., a total production of about 2 billion eggs.

The relationship between egg abundance and larval abundance through the 1967 sampling periods is presented in Figure 1. Notice that the larval abundance decays rapidly two weeks after the end of spawning. This would be presumably the result of the mortality of yolk-sac larvae in the population, which is partly exaggerated by their low capability of surviving at ambient temperatures above 70° F, as occurs near the end of the spawning season. However, during the first several weeks after spawning, it is apparent that the population of larvae stabilizes at about 30 million fish. This stabilization is partly an artifact of a change in sampling gear; however, the increasing sampling efficiency is offset by a decreased susceptibility of larvae to sampling which would normally have caused a marked reduction in the apparent population.

Assuming for the moment that this plateau value is representative of the total survival of striped bass equivalent to Phase I survival in hatchery operations and that the 1967 egg and larval abundance data are comparably biased, then the total survival from egg to post-larvae in 1967 was 7.8% of the eggs spawned. If on the other hand, a strong negative bias is present only in the egg abundance data, then total egg production might be better represented by the 2 billion estimate. Using this figure, 1.4% of the eggs survive to become post-larval fishes. Thus, under these assumptions, the efficiency of natural production (n) is estimated to range between .014 and .078.

As indicated by the range in computed values, these estimates are tenuous at best and cannot be taken as absolute boundaries. However, there are several additional factors which tend to support a high proportional return

Figure 1. Standing crop estimates for striped bass eggs and larvae in the Hudson River during the spring of 1967.



per egg spawned. For instance, in connection with the 28 million estimate derived from 1967 data, it is important to understand that a large proportion of the population which had grown beyond sampleable size was not included in that 28 million estimate, although the additional mortality which could be expected to reduce the number of fish involved in the 28 million figure would tend to compensate for this error to some degree. In addition, data summarized in the FES (e.g., Fig. A-V-16) indicate that the abundance of juvenile bass does not decrease dramatically during the summer.

VI. Estimate of Relative Efficiency of Hatchery Production

The relative efficiency of hatchery production is estimated by assuming hatchery efficiency of .0081,as estimated previously, and a natural production efficiency ranging from .014 to .078. This results in estimates of relative efficiency of hatchery production of 0.58 to 0.10. Neither estimate is consistent with hatchery replacement of stock losses to power plant operation.

Although the estimate of natural survival is tenuous at best, it is not at all apparent that the hatchery can produce higher numbers of young fish surviving than does natural reproduction. As a result, these numbers have a strong negative interpretation as related to the potential for using hatcheries to replace fish killed by power plant operations.

VII. Staff Estimate of Stock Size Required to Replace 15 Million Juvenile Bass Which Are Killed by Power Plant Operations.

In this discussion, the number of eggs per pound of female is assumed to average 50,000. Thus a 10-pound female would possess approximately 500,000 eggs. In order to produce 15 million young-of-the-year striped bass at an efficiency rate of .0081, the hatchery would require some 1.8 billion eggs to be exploited, which at 50,000 per pound of female fish, converts to roughly 37,000 pounds of mature females which would ultimately be exploited. This is equivalent to 3700 10-pound females. When male fish are also considered, it is obvious that the hatchery effort is likely to require in excess of 7000 to 8000 fish, at a combined weight of more than 50,000 pounds.

VIII. Further Important Considerations

Beyond the considerations which have already been presented, there are several factors which would tend to complicate the proposed hatchery effort. For instance, the physical location of the proposed hatchery, which is in Florida, would impose severe limits on its utilization of Hudson brood stock. This conclusion results from the fact that past efforts to induce ovulation in striped bass collected at temperatures below 50° have not resulted in the production of significant numbers of viable eggs, whereas later collections of striped bass from temperatures within the spawning range allow collections of fairly substantial numbers of eggs. Thus, collections of Hudson spawning stock to be used in hatchery operations must be done during the spawning season in the Hudson River.

Two problems arise in association with the temperature requirements for striped bass reproduction. Most spawning in the Hudson occurs after May 1 when Florida waters are typically 75° to 85° F, according to data published by Barkuloo who also made some extremely pertinent observations related to temperature, i.e.:

> Striped bass eggs at the Florida experimental hatchery survived water temperatures from 56°F. to 72.5°F. All eggs died when the temperature dropped below 54°F., and less than 1% survived above 72°F.

> Water temperature is an important limiting factor in striped bass reproduction. Observations at the Florida striped bass hatchery during 1964 and 1965 have revealed the following facts. Female striped bass with mature ovaries were subjected to water temperatures from 70° to 75°F. The ova in these fish changed in color from pale green to orange and became opaque. Ripe males subjected to the same temperatures had live sperm, but motility was greatly reduced. It is probable that ripe female striped bass can tolerate temperatures of 70°-75°F. for only a short period of time before spawning, or egg reabsorption occurs.
>
> (Barkuloo, 1967, p. 13)

Similarly the stocking of Florida-raised striped bass into the Hudson may be complicated by cold shock phenomena which could severely hamper survival upon stocking.

VIII. Conclusions

Based on the preceding analysis, the Staff concludes that the hatchery proposal cannot be accepted as a realistic method for maintaining striped bass production in the estuary. In fact, it is likely that the implementation of a large-scale hatchery effort which relies on Hudson River stock would itself cause reduced survival of the young-of-the-year striped bass because of a lower overall net efficiency. Although some gain may result from broodfish obtained from commercial fishermen, the only realistic alternative for overcoming this production efficiency deficit would be for the hatchery to maintain its own reproductive stock. The brood stock requirements for this type of operation would necessitate the maintenance of a standing crop of potential breeding stock perhaps exceeding 200,000 pounds.

The Staff concludes that the hatchery proposal is not feasible as designed by the Applicant using present technology.

Hudson River Policy Committee, "Hudson River Fisheries Investigations, 1965-1968, Evaluations of a Proposed Pumped Storage Project at Cornwall, New York in Relation to Fish in the Hudson River," presented to Consolidated Edison Company of New York, Inc., 1968.

Barkuloo, J. M. 1967. Florida striped bass. Fishery Bull. No. 4, Fla. Game & Fresh Water Fish. Comm., 24 p.

Redirect - Rebuttal Testimony of

Dr. C. P. Goodyear

Modifications of April 10, 1973, Testimony on

Staff Comments on Applicant's Research Program

April 24, 1973

References:

(1) Testimony of Dr. James T. McFadden (5 February 1973), "Effects of Indian Point Units #1 and #2 on Hudson River Fish Populations".

(2) "Testimony of Dr. James T. McFadden, Dean, School of Natural Resources, University of Michigan, on Effects on Hudson River Fish Populations of the Simultaneous Operation of Indian Point Units #1 and #2, Plus the Bowline and Roseton Power Plants," dated March 30, 1973.

Research Program

In his most recent testimony, Dr. McFadden again stated the Applicant's position that the research program would be able to detect a serious impact from plant operations within time to initiate any of several alternative procedures which could be used to alleviate any problem which might arise. It is the Staff's belief that the research program underway will be quite capable of producing beautifully quantitative information related to the description of the changes which may or may not be occurring in populations of fishes in the River. However, it is very likely that the Applicant's program of research will not provide information which will be sufficient to analyze the cause-and-effect relationships governing populations of striped bass and other fishes in the River.

The reliability of any research program is a function of several factors, including not only the personnel but also the methodology which is used in doing the research. Of utmost importance is the type of approach used in deriving the information utilized in the research effort. In general, the experimental approach can be distinguished as either one which is mostly descriptive or one which is more analytical in nature. In the case of a descriptive approach, it is common for investigators to obtain data with multiple alternative indications. However, unless effort was made beforehand to evaluate all of the alternative relationships, then the utility of the information which is described during the period of the investigation is generally quite limited. On the other hand, if the more analytical approach is taken to the study of the particular problem under consideration, then it is common to evaluate the various alternative cause-and-effect relationships that contribute ultimately to the selection of one or more various conclusions.

This type of approach requires that the information to be investigated by research efforts be thoroughly understood before the investigation begins so that the investigator is not continually concerning himself with problems or concepts which need not have been a part of the study from the very beginning.

Design of research

The importance of such considerations can be illustrated in the following situations which are particularly pertinent for this proceeding. Dr. McFadden in previous testimony discussed an electrophoretic technique for determining relative proportions of the various components of the stock in the Middle Atlantic region, based on a paper which had just been published in Transactions of the American Fisheries Society. This process has much merit and much information can be gained through electrophoretic techniques; however, it is very important to understand all of the processes which may be involved in deriving conclusions from these studies before embarking on a program of research using this type of an approach. In this particular case, the electrophoretic technique was applied to blood serum proteins. These are traditionally quite variable because they regulate many different processes within the animal. In order to extrapolate the information derived from local studies in the Chesapeake to areas in the Atlantic, or for that matter within the Chesapeake Bay itself, one must first demonstrate that the proteins which were found to vary from place to place would not lose their identity if the fish were to move from one place to another within the Bay or within the Atlantic Ocean. It seems obvious that this type of research effort cannot be done on the spur of the moment and that it would take several years to complete and in fact may not provide the most desirable results.

A particular point of concern to the Staff is that the Applicant is continually discussing the reality of compensatory changes in survival rates within Hudson River fish populations (particularly the striped bass) but has not yet proposed any mechanism nor a way of experimentally testing for its existence. Furthermore, analytical obstacles such as the annual variation in distribution of larval striped bass within the Hudson, the lack of precise historical data on either the mechanism or the rate of mortality of early life stages, and the apparent decline in commercial landings in the Mid-Atlantic region since the late 1960's will make any attempt at such an analysis more difficult. The Staff has seen no evidence in any of the populations of striped bass where data have been gathered to indicate that a mechanism is operant such as that which is used in the Lawler model.

In conclusion, the Staff has not perceived that the Applicant has developed a framework of experimental design based on a logical working hypothesis which will evaluate mechanisms through which the compensatory processes would work according to convention adopted in the Lawler model. From the information which the Staff has evaluated, it would appear that the Applicant's research effort as presently designed will likely produce fairly precise estimates of certain parameters such as mortality rates and leave other very important factors such as compensatory survival completely unresolved.

Data analysis

One absolutely essential factor in this or any other scientific inquiry is that the conclusions which are drawn from data gathered during the study must be consistent with the data which was gathered in past studies and a conceptual framework of logic. This requires that data analysis be more than a superficial process. An example of the dangers in obtaining unjustified conclusions from superficial examination of data is available within this proceeding. Dr. McFadden in his comments on the Staff's analysis of the fishery data for striped bass concluded that the correlation between fishing effort in the Hudson River and the fishing effort in the Atlantic 5 years later was a reason that the landings in these were correlated in the same temporal relationship (p. 11 of rebuttal testimony and transcript p. 9470-9471). It is the Staff's opinion that Dr. McFadden's reasoning in this case is erroneous simply because of a superficial approach to a particular set of data.

The fundamental assumption existing in Dr. McFadden's discussion is that catch is a function of effort. It is a positive function, such that if effort increases, catch increases, and if the effort declines, the catch declines. This is the reason why Dr. McFadden is able to draw the generalization that two fisheries exhibiting parallel changes in effort could be expected to exhibit parallel changes in landings. However, because of the positive relationship between catch and effort, the only way for a decrease in effort to influence catch directly is to cause a decrease in catch. Over the period which was being discussed and to which Dr. McFadden was directing his conclusions, the effort was declining in a direction opposite from the change in catch; thus, the logic he was applying was invalid.

It is extremely important that the evaluation of information derived from this study (and hopefully the information which was used in designing the study) be done in a manner consistent with logic and with all information which is available.

SET II

Staff Responses to Interrogatories for Dr. C.P. Goodyear re Staff Comments on Applicant's Research Program

1. Have you reviewed with the biologists on site the study program? If so, when did you meet with these biologists? How much time did you spend in discussion with the biologists on site?

Response: I have not reviewed the study program with the biologists on the site. My understanding of the Applicant's research program is derived from descriptions of that program which the Applicant has provided and from testimony of Dr. James T. McFadden and Harry J. Woodbury in this proceeding. Among the documents which I have examined are:

1) Lower Hudson River Ecological Study - Proposed Program for 1972-6.

2) Hudson River Ecological Study in the Area of Indian Point. A Proposal Prepared for Consolidated Edison Company of New York, Inc., by Texas Instrument Company, November 30, 1971.

3) Testimony of Dr. James T. McFadden and Harry J. Woodbury, dated February 5, 1973, on Indian Point Studies to Determine the Environmental Effects of Once Through vs. Closed Cycle Cooling at Indian Point Unit No. 2.

2. Is it your understanding that the Applicant's research program is "descriptive" or "analytical" as those are used on pages 1 and 2 of the Comments?

Response: It is important to realize from the very beginning that the descriptive vs. analytical distinction is a subjective comparison. My usage of these two terms is consistent with the following definitions:

(a) descriptive study - a study designed to obtain a numerical value which can be used to characterize a particular phenomenon, i.e., the question asked is what

(b) analytical study - a study designed to quantify the parameters and processes which control the numerical value of the phenomenon which is the focus of the descriptive study, i.e., the question asked is why.

The Applicant's research program contains both descriptive and analytical components as described above. However, it is apparent that the descriptive requirements of the program far outweigh the analytical component, at least from my point of view.

3. Are you aware of the invitation of the Hudson River Policy Committee to have an AEC representative attend their meetings at which they review the progress of the ecological study program?

Response: I was not aware of such an invitation. Furthermore, to the best of my knowledge, no formal invitation has been sent to the AEC to attend any particular meeting.

4. Are you suggesting that, because the electrophoresis study procedure

<u>is of recent origin, it is not worth carrying out these studies?</u>

<u>Response</u>: No. In the first place, the electrophoresis technique is not that recent in origin. My first exposure to this technique was in 1963 when I was involved in an effort to determine whether this technique could be applied to separate various species and subspecies of small mammals in the Mississippi. Furthermore, as I stated in the testimony, the process has much merit and much information can be obtained through the electrophoresis technique. It was not the intent of the discussion to negate the utility of the information produced using this methodology. In contrast, my intention in discussing Dr. McFadden's proposal was to provide an example of the importance of a thorough evaluation of the processes which are the subject of a research effort before deriving conclusions related to the probability of fulfilling the stated goals of the study, or the length of time which will be required for its completion.

In this connection, Dr. McFadden described the study as follows:

> "The study will require only a single year to complete. It will be carried out by collecting striped bass from the Hudson River, and from Chesapeake Bay tributaries, determining, through electrophoretic analysis their protein patterns. Samples will be taken from plasma, serum, heart, muscle, and whole eye. The samples taken from the Hudson River and Chesapeake Bay tributaries--will provide us with standards which identify fish as belonging to those races, and we will then collect a sample of striped bass from the Mid-Atlantic fishery, analyze them for their protein patterns, and be able to break that sample down attributing various percentages of it to the different rivers from which these fish have originated." (Tr. 9520)

Dr. McFadden presented theoretical foundation for his projections in his testimony as follows:

> "Fish, in fact all species of living organisms, have characteristic protein banding patterns in their tissues which are not nearly as susceptible to alteration by external environmental influences as are some other characteristics that can be studied in order to separate living races of living organisms." (Tr. 9519)

Unfortunately, this basic hypothesis is not valid, particularly for serum proteins. For example, the recent study which Dr. McFadden cited in his testimony distinguished 31 protein bands from their electrophoretic technique. These proteins were statistically analyzed as to their distribution in relation to age, sex, date of collection, and source. The distribution of 10 of the proteins was found to be related to age, 9 to sex, and 10 to day of collection, thus indicating the inherent susceptibility of the protein composition of the blood to both external environmental influences and the age and sex composition of the sample. These problems make the application of the electrophoresis procedure for distinguishing striped bass by origin a much more formidable problem than is reflected in Dr. McFadden's discussion. In effect, before definitive conclusions can be arrived at it will be necessary to understand the factors involved in the expression of those proteins which are found to vary from one place to another.

Another important problem which limits the utility of the technique as proposed by Dr. McFadden in his testimony is that the data which commonly are produced from this technique are not readily suitable for the type of application which would be required for study under discussion. This situation results from the fact that the presence of a particular serum protein in one population and the absence in another is an uncommon situation. Most studies have found that the variation which can be distinguished between populations is attributable to variations in the frequency of occurrence of the serum proteins in sampled fish. This was the case with striped bass studies done in the Chesapeake, and it is likely to be the case with other populations along the Coast.

The importance of this consideration is related to the site of the project which would be required to adequately demonstrate the origins of fish in the Middle Atlantic region. For instance, if the Hudson population were to possess a protein which was unique to those fish originating in the Hudson and which was retained after the fish left the Hudson River to enter the Atlantic coastal waters, it would be possible to obtain fairly reliable estimates on a fairly small number of samples because each Hudson fish which was captured could be identified from its unique protein component. In this case, the proportion of the stock composed of the Hudson River fish would simply be the ratio of the number of Hudson fish identified to the total number of fish analyzed.

Unfortunately, it is highly improbable that a unique or nearly unique protein will be found which can be used to distinguish striped bass from the Hudson River. It is more probable that the features which can distinguish the Hudson population based on electrophoretic analysis would be related to variations in the proportions of bass from each spawning area which possess certain of the proteins. In this type of situation, the use of the data to compute the proportion of the stock derived from the Hudson will be much more tenuous and will require a much larger sample size and more extensive sampling of all of the potential sources of the stock. This situation is the result of the fact that the computation procedure which must be utilized to estimate the Hudson proportion of the Middle Atlantic stock will be based on variations in the ratios of the frequency of various proteins that are present in the Middle Atlantic stock.

The effect of this factor is illustrated by comparing the frequency distributions of the five proteins which were found to distinguish among populations of striped bass in the upper Chesapeake Bay, to the frequency distributions of those proteins computed for assumed mixtures of fish from the various populations. The data used in this discussion are presented in Table 1.

Table 1

	Frequency of occurrence of serum proteins used to distinguish populations in the Upper Chesapeake Bay *					Values computed for assumed mixtures			
Band	Choptank	Elk	Nanticoke	Patuxent	Potomac	a	b	c	d
3	.90	.99	1.00	.91	.88	.93	.91	.96	.95
4	.10	0.00	.22	.10	.03	.10	.03	.05	.16
6	.25	.20	.05	.09	.17	.15	.16	.18	.15
12	.68	.83	1.00	1.00	.81	.87	.84	.85	.84
15	.85	.71	.67	.73	.80	.75	.78	.73	.76

a) 20% from each of the subpopulations

b) 60% from Potomac; 10% from each of others

c) 60% from Elk; 10% from each of others

d) 50% from Choptank; 50% from Nanticoke

* Morgan, R. P.II, T.S.Y. Koo and Krantz, G.E., "Electrophortic Determination of Populations of the Striped Bass, *Morone saxatilis* in the Upper Chesapeake Bay," *Trans Amer. Fish. Soc.* 102(1): 21-32, (1973)

In order to investigate the potential of using this data to evaluate origins of striped bass, four hypothetical combinations were assumed and the frequency distributions of the proteins for each of the assumed combinations was calculated and presented in columns a through d of Table 1. By assuming that column d represented the true composition of a mixture of fish, the capability of statistical separation of this combination from the other three combinations based on the frequency distribution of serum proteins was evaluated for a sample size of 100 fish and the Chi-square statistic (Table 2). Using this procedure, it was not possible to distinguish between the assumed mixture, although the characteristic composition of each of these mixtures was quite different.

This brief discussion is meant to point out the importance of considering the use to be made of data in evaluating a procedure for a study, and in projecting either the time scale which will be involved or the conclusions which will be derived from the study.

It is my opinion that the utility of electrophoresis was overestimated by Dr. McFadden. I do not believe that an adequate study can be done in a single year and am skeptical that any definitive conclusions will result from the study itself, at least as related to the stated objectives. However, I do not believe that it is reasonable to conclude that the electrophoresis study as proposed is not worth carrying out because of the possibility that a unique quality of the protein composition may be found in the population.

Table 2

Band	Frequency	Expected*	Observed a	Observed b	Observed c
3	0.95	95	93	91	96
4	0.16	16	10	9	5
6	0.15	15	15	16	18
12	0.84	84	87	84	85
15	0.76	76	75	78	73
		X^2 =	2.4	3.6	8.2

* Based on a sample of 100

5. Are you aware of the tagging study to be conducted by the Federal Government and N. Y. State to quantify the contribution the Hudson River makes to the Atlantic bass fishery?

Response: Yes

6. Is it your understanding that the results of this tagging study will be available to Con Edison and the AEC?

Response: Yes

7. On page 3 you refer to "populations where data have been gathered" with respect to compensatory mechanisms To what populations are you referring?

Response: Is is apparent from the question that some misunderstanding may exist concerning the intent of the studies related to the data which I have examined with respect to compensatory mechanisms. Very little, if any, of this data were gathered in a study specifically directed towards determining the compensatory mechanism per se; however, the information which has been compiled does allow an examination of various hypotheses which are related to assumptions concerning the nature of compensation.

The utility of this approach is related to the highly involved nature of the interaction of organisms with their environment. In effect, each assumption concerning compensation carries with it certain corollaries, many of which can be readily examined. For example, the mechanism of compensation which is incorporated in the Lawler model requires mortality rates to increase as larval population density increases. This notion of compensation does not presuppose any physical mechanism which is responsible for the characteristic density-dependent behavior of the larval mortality, etc. In contrast to Dr. Lawler's presentation of compensation, Dr. McFadden has presented the notion of compensation through changes in growth rates in the population, rather than changes in mortality rates. Furthermore, Dr. McFadden has presented growth rate data which indicate to him, at least, that the population of striped bass in the Hudson River is stunted in relation to other populations along the east Coast and projects this information to indicate that the operation of Indian Point Units Nos. 1 and 2 can be evaluated by using changes in growth rate of striped bass. The basic concept incorporated in Dr. McFadden's analysis of compensation is that good is a limiting factor in the population of striped bass, at least during their first year of life.

Although the two discussions presented by Dr. Lawler and Dr. McFadden are not entirely consistent, there is no basic violation of the assumptions inherent in either of the two notions by the adoption of the other. The two concepts are interrelated because growth rates influence mortality rates in such a manner that high growth rates tend to decrease mortality rates. This relationship is a result of the fact that the component of the mortality rate which is attributable to predation decreases as the size of the individual fish increases; therefore, a change in growth rate upward would tend to increase the size of the fish and decrease the rate of mortality in the population. Thus, the combination of these two notions of compensation are mutually reinforcing, rather than mutually exclusive. However, a corollary of this relationship is that the functions which would describe mortality rate in a population as it is related to density of the individual must be an inverse relationship. If rate condition is violated, specifically the survival rate and population density are positively correlated, then the first approach to compensation is incompatible with the data.

The result of these mental gymnastics is a conclusion which relates the computational methodology adopted in the Lawler model to a physical explanation of the mechanism involved. Furthermore, the importance of the relationship between mortality rate and growth rate in early life stages which is a requirement of the coexistence of the physical manifestations of the two conceptual notions provides a comparatively simple, yet powerful, test of the assumed relationships. Data from studies conducted in the Sacramento-San Joaquin provide an example evaluation of the validity of these assumptions. In that population, investigators have determined that the survival rates in the early life stages are in fact directly related to the density of the population as indicated in the following quotation from Turner and Chadwick*:

* Turner, J. L. and Chadwick, H. K., "Distribution and Abundance of Young-of-the-year Striped Bass, Morone saxatilis, in relation to River Flow in the Sacramento-SanJoaquin Estuary," Trans Am. Fish. Soc. 101 (3): 442-452(1972).

> " The survival rate of each year class was compared with the density of bass at 1 inch, log of the river outflow, and numbers of striped bass caught at Tracy. The only significant single correlation coefficient was a positive one between rate of survival and density of bass at 1 inch ($r=0.61$: $r_{0.95}=0.55$). High density is unlikely to favor increased survival, so this correlation probably just reflects better environmental conditions."

This, in itself, casts doubt on the inverse relationship which is hypothesized in the Lawler notion of compensation between survival rate and density. However, in itself it does not demonstrate the invalidity of the basic notion since the density is also a function of survival rate, and as a consequence an increased food resource could positively affect survival rate by increasing the rate of growth. The variation in food resources from year to year would alter survival rates in such a way that survival rates would be positively correlated with density in comparison among years. This relationship does not violate the notion of compensation in the Lawler model, but would be more appropriately interpreted as the causal relationship governing year class fluctuations.

Unfortunately, further study of the Sacramento-San Joaquin also demonstrated that the growth rate in the population is inversly proportional to the survival rate, rather than directly proportional. This observation violates the basic assumptions which are integral components of the Applicant's approach to compensation, and it must be concluded that the Lawler model of compensation is not applicable to the striped bass on the West Coast. Furthermore, the Sacramento-San Joaquin data provide insight related to the probable relationships which govern the importance of predation in the mortality of early stages of striped bass. In particular, the influence of predation on mortality rate is partly a function of the density of the predators in the population in the estuary.

Under the assumption that mortality rates are predominately a result of predation and with a specified and limited number of predators involved, the survival rate of striped bass could be expected to be positively related to the density of striped bass at any time that the predators become satiated. Thus, once the saturation point of the predators is reached, the actual magnitude of the loss of juvenile fishes on a daily basis would be set. Thus the number of fishes surviving the predation would be the original stock at the beginning of the day minus the loss to the predators which is a constant. If the number of fish at the beginning of the day is increased by a certain amount, that amount is already added to the number of bass surviving after the predation mortality is extracted. Under these conditions, the proportion of the stock which survives from one day to the next is positively correlated with the density of the stock. This relationship is outside of the conceptual and mathematical notion of compensation supplied by the Applicant, and would tend to exaggerate the effect of any increased mortality in the larval population.

Many other similar factors related to compensation were evaluated in conjunction with the various populations which have been studied; however, it is reiterated that the studies for the most part were not specifically directed towards evaluating the mechanisms related to compensation in early life stages of striped bass. In addition to the data on the Hudson River and the Sacramento-San Joaquin system, I have relied on data from various other populations for various conclusions. These populations for the material examined exist in the following water bodies:

Santee-Cooper Reservoir, South Carolina

Delaware Bay

Chesapeake Bay

San Francisco Bay

Canton Reservoir, Oklahoma

Keystone Reservoir, Oklahoma

John H. Kerr Reservoir, Virginia

Hudson River

Albemarle Sound, North Carolina

8. On page 3 there is a statement that the research effort "will likely produce fairly precise estimates of certain parameters and leave other very important factors completely unresolved." What are the parameters for which the program will achieve precise estimates?

Response: The parameters which will be measured with greatest precision will likely be the physical data, such as temperature, dissolved oxygen, salinity, pH, turbidity.

9. How precise will these estimates be?

Response: The precision of these estimates will primarily be governed by the equipment used to measure these parameters and the care which is taken in these measurements. However, it is my belief that these values will be considerably more precise than would be required for determining the relationships to biological observations.

10. What are the uses and limitations of the data for which we will get "fairly precise estimates"?

Response: The utility and limitations of the data for which fairly precise estimates are obtained cannot be fully determined until after the data are gathered and the study completed. However, one limitation concerning data application is related to the assumptions underlying the interpretation of precison itself, particularly for estimates of biological parameters, such as absolute population size. This limitation is the result of the fact that high precision does not necessarily indicate a high degree of accuracy in a particular estimate of a parameter. This results from the fact that precision, is related primarily to the repeatability of an estimate, whereas accuracy is a measure of the deviation of the estimate from the true value of the parameter. In the case of physical data, such as temperature, dissolved oxygen, etc., it is very simple to check the calibration of the instrumentation or the process used to evaluate the parameters, so as to insure both accuracy and precision. Biological sampling procedures often cannot be calibrated to insure both accuracy and precision, thus an increase in precision may or may not increase the accuracy of a particular estimate of a parameter. Accuracy in these situations is insured through use of alternative methodologies in estimating the parameters and in detailed evaluations of sampling procedures. This limitation may or may not be of importance, depending upon the characterization of the data gathered and the use which is made of the data.

11. <u>What factors will be unresolved?</u>

<u>Response:</u> A list of factors which will be unresolved at the end of the study is without end. Some of the important factors which will be unresolved based on the present study program will be the influence of the fishery on survival rates and the relationship between fishing intensity and fish mortality. Another will be the question of compensation as a factor operating within larvae populations of striped bass. Generally speaking the factors which will be unresolved will be adequate quantification of the processes which control the magnitude of important biological parameters such as mortality rate, within the population and growth rates within the population, the distribution of spawning, etc.

12. <u>With reference to the paragraph on page 5, is it your understanding that the AEC Staff will not have an opportunity to draw its own conclusions from data gathered during the research program?</u>

<u>Response:</u> No.

BEFORE THE UNITED STATES

ATOMIC ENERGY COMMISSION

In the matter of	)	
	)	
Consolidated Edison Company of	)	Docket No. 50-247
New York, Inc.	)	
(Indian Point Station, Unit 2)	)	

A response by
John P. Lawler, Ph.D
Quirk, Lawler & Matusky Engineers
on
Additional Information Requested By the Staff on the Temperature Distribution Section in our March 30, 1973 Testimony on Cumulative Effects of Bowline, Roseton and Indian Point Generating Stations on the Hudson River

April 20, 1973

TABLE OF CONTENTS

TABLE OF CONTENTS (continued)

INTRODUCTION

The purpose of this document is to provide answers to the Staff's questions of April 11, 1973 on temperature distribution resulting from multi-plant operation on the Hudson River. Section I of this document and the enclosed attachments respond to the first set of requests. Answers to the additional Staff's questions on pages 3, 4 & 5 of the April 11, 1973 document are given in Section II.

Based upon the material presented in this document and in previously submitted reports, it is our opinion that:

1. We agree with the need for more investigation and field data as recommended by the Staff. In fact, the Applicant's proposed study and satisfaction of the requirement of the New York State Department of Environmental Concervation in its letter of November 4, 1971 to the Applicant envisions a comprehensive field program and mutual dependence of model and data development.

2. We believe that the available Danskammer field data used to compare the Staff's predictions with actual River behavior in our March 30, 1973 testimony describes rather completely the behavior of the Danskammer plume. Some 35 field survey runs covering a range of meteorological, plant and hydrodynamic conditions and spanning of the entire surface and subsurface temperature spectrum were made in 1969.

   Two extensive field programs at Lovett consisting of a total of 106 field runs were also conducted in 1969 & 1970. We feel that these data provide sufficient information to delineate the Lovett & Danskammer thermal effects and to obtain a reasonable level of confidence in model predictions.

3. We do not agree with the statement that our March 30, 1973 findings were contradictory to previous testimonies. This document clearly indicates that the previously simulated conditions were not the same as those modeled in the March 30, 1973 document. The previously simulated conditions addressed themselves to incipient salt flow conditions at Indian Point while the March 30, 1973 testimony discussed conditions similar to those used by the Staff in its February 8, 1973 testimony to show that the Staff's results are unrealistic.

4. It is important to indicate that neither the Applicant's multi-segment model results presented in the March 30, 1973 testimony, nor the Staff's February 8, 1973 temperature distribution findings take into account the role the well established net non-tidal flow of density-induced circulation concept in the Hudson River.

During normal summer, fall and winter months and on the basis of long-term (1918 to 1970) observations in the Hudson River, the Indian Point site is within the salt-intruded reach. This is the case during the warmest natural thermal period (July, August, September) as well as during the striped bass entrainment period (June and July). During these months, the Hudson River flow available for dilution of thermal discharges (ranging between 21,500 cfs and 35,000 cfs) at Indian Point is significantly higher than the flow and dispersion values used in the Applicant's March 30, 1973 testimony and in the Staff's February 8, 1973 testimony on the effects of multi-plant operation. Therefore, the Applicant's multi-plant thermal findings (Ref. 5) can only be used as representing conservative estimates of the thermal effects during the entrainment period and the warmest natural thermal period.

To determine compliance with the New York State thermal discharge criteria during more critical (non-summer) periods, the incipient salt flow (ISF) conditions must be used. Our April 5, 1972 testimony addressed itself to these conditions and concluded that under ISF conditions the thermal criteria will be met at Indian Point and that the overall temperature rise at Indian Point will be 1.75°F.

5. The thermal simulation approach used by the Applicant was generally accepted by the New York State Department of Environmental Conservation as a basis for a reasonable assurance that the thermal criteria will be met, provided that the predicted thermal effects will subsequently be tested by field observations after plant start-up. On this basis, the Department of Environmental Conservation issued operating and/or construction permits for the Roseton, Bowline, Lovett Unit 5, Astoria Unit 6, Indian Point Units 1 and 2, and Oswego Unit 5 power plants. The position of the New York State Department of Environmental Conservation to the Applicant's assurance that the Indian Point Units 1 and 2 will meet the thermal criteria was clearly stated by the State in Volume II, pages 58 and 59 and pages 71 and 72, of the AEC Final Environmental Statement.

Section I of the Staff's April 11, 1973 Request

Request No. 1:

Provide a complete listing of the computer programs used to generate the curves shown in Fig. III-8.

Response:

A listing of the computer program, HRETEM, for the multi-segment model which was used to generate the curves in Fig. III-8 is presented in Plate 1 (attached).

Request # 2. Provide the original input and output data used for all the actual cases run as shown in Figures, III-3, III-7, III-8, III-9. Specifically the longitudinal dispersion coefficient (E) and the thermal stratification factor (TSF) used should be given as a function of distance, since they were not specified anywhere in Lawler's testimony.

Response

The input and output data files corresponding to individual and multi-plant effects presented in the present study are reproduced in plates 1-A through 6 as shown below:

Ref. 5 Figure No.	Profile	Plate or Remarks
Figure III-3	Prediction by the Staff's Model	Plate 1-A
Figure III-3	Prediction by QL&M	Rated Capacity Predictions - Ref. 1
Figure III-7	Bowline, Indian Point & Roseton	Plates 2, 3 & 4
Figure III-8	Combined Effect	Plate 5 gives combined Bowline, Indian Point & Roseton effects. The observed effects of Lovett and Danskammer were added manually to produce the combined effect profile.
Figure III-9	Predictions by the Staff	From February 8, 1973. testimony
Figure III-9	Demonstration by QL&M	Plate 6

Figure III-8 in Reference 5 was constructed using a set of temperature decay parameters (K_t) combining the influence of thermal stratification (TSF) and heat transfer to the atmosphere ($\overline{K}$). This approach is valid since both of these parameters appear in one term in the governing differential equation. As indicated in Reference 5, a heat transfer coefficient of 130 BTU/Sq. Ft. °F day was used to represent model heat transfer conditions. This value was termed model $\overline{K}$ in Figure III-8 in Reference 5 to distinguish it from <u>prototype</u> conditions.

Reference 5 also stated that although we used in previous reports $\overline{K}$ values on the order of 130 BTU/ft^2 °F day, we recognized from the beginning that in real estuaries, the heat transfer coefficient may be significantly greater than this value. This difference between model and prototype $\overline{K}$ values was indirectly taken into account in Reference 5 by selecting appropriate K_t values to reflect this as well as the influence of thermal stratification. This approach did not require delineation of $\overline{K}$ and TSF values.

In order to respond to this request, i.e. to assign individual values to $\overline{K}$ and to TSF rather than to combine the two effects in one parameter K_t, we have made new runs based on our most recent findings on these two parameters. These findings were still in progress while Reference 5 was being prepared. The influence of the TSF and $\overline{K}$ values used in the present document, shown on the attached computer input and output files, and reproduced in Table 1 approximated that of the K_t parameter used to generate the results shown in Reference 5.

Study results corresponding to these values of TSF and $\overline{K}$ are depicted on Figures 1, 2 and 3.

Figure 1 shows the river cross-sectional average rises calculated by combining the observed effects of Lovett and Danskammer plants [References 1, 14 & 15] with the simulated effects of Indian Point Units 1 & 2, Bowline

TABLE I

HUDSON RIVER INPUT USED IN THIS STUDY

RIVER FLOW = 3500 CFS, MODEL $\overline{K}$ = 130 BTU/SQ. FT. °F DAY, PROTOTYPE $\overline{K}$ 1.5 MODEL $\overline{K}$

REACH NO.	DISPERSION COEFF. (SQ.MI/DAY)	VELOCITY (MI/DAY)	TSF	DIST. FROM BATTERY (MILES)	HEAT LOADS (BTU/DAY)
1	.10000+001	.8131+000	.10+002	.800+002	.000
2	.30000+001	.4551+000	.80+001	.700+002	.000
3	.38000+001	.3816+000	.60+001	.680+002	.000
4	.41000+001	.3949+000	.45+001	.665+002	.000
5	.45000+001	.3682+000	.35+001	.660+002	.000
6	.46000+001	.3618+000	.30+001	.657+002	.000
7	.46000+001	.3516+000	.20+001	.654+002	.120+012
8	.50000+001	.3813+000	.20+001	.644+002	.000
9	.53000+001	.3516+000	.25+001	.630+002	.000
10	.60000+001	.3516+000	.35+001	.614+002	.000
11	.72999+001	.3695+000	.45+001	.575+002	.000
12	.92000+001	.3613+000	.50+001	.540+002	.000
13	.10700+002	.3695+000	.50+001	.510+002	.000
14	.11700+002	.4405+000	.45+001	.475+002	.000
15	.11900+002	.4019+000	.30+001	.450+002	.000
16	.11000+002	.3249+000	.25+001	.440+002	.000
17	.11700+002	.3471+000	.20+001	.435+002	.000
18	.11600+002	.3181+000	.20+001	.430+002	.200+012
19	.11500+002	.3329+000	.20+001	.425+002	.000
20	.11400+002	.3471+000	.20+001	.420+002	.000
21	.11300+002	.3471+000	.20+001	.410+002	.000
22	.10900+002	.3014+000	.20+001	.385+002	.000
23	.10400+002	.2366+000	.20+001	.375+002	.124+012
24	.10200+002	.2082+000	.20+001	.355+002	.000
25	.10100+002	.2468+000	.25+001	.340+002	.000
26	.10000+002	.2335+000	.40+001	.300+002	.000
27	.11400+002	.2363+000	.50+001	.200+002	.000
28	.13900+002	.3368+000	.10+002		

COMBINED EFFECTS OF DANSKAMMER, ROSETON, INDIAN POINT, LOVETT AND BOWLINE POWER PLANTS ON HUDSON RIVER TEMPERATURES
QL&M'S PREDICTION

CONDITIONS: Q_f = 3,500 CFS
MODEL K = 130 BTU/FT^2 °F DAY

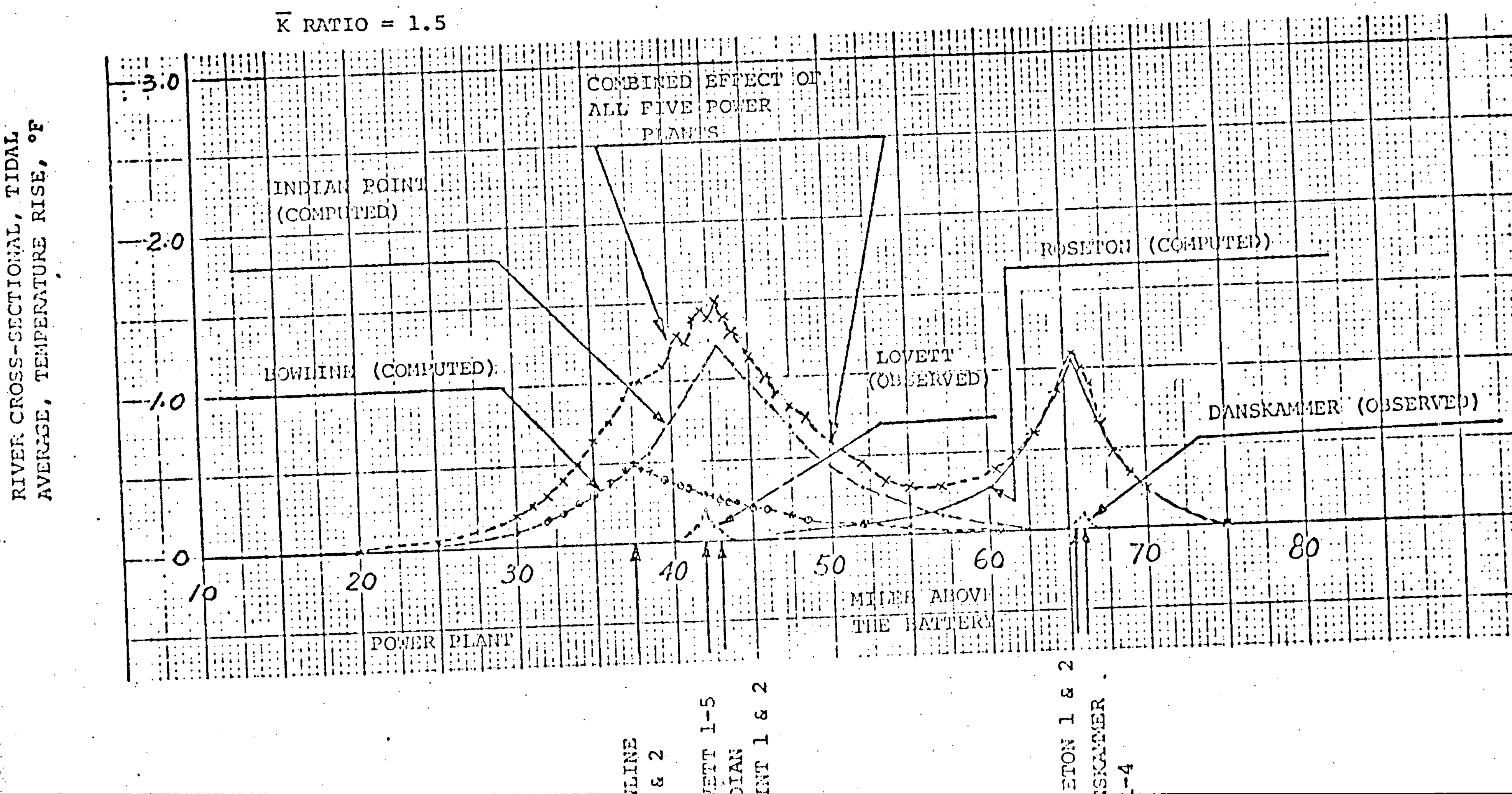
VARIABLE DISPERSION COEFFICIENT - SEE TABLE 1

VARIABLE TSF - SEE TABLE 1

$\overline{K}$ RATIO = 1.5

HEAT LOADS:

	BBTU/DAY
DANSKAMMER	54
ROSETON	120
INDIAN POINT	200
LOVETT	57
BOWLINE	124



Units 1 & 2 and Roseton Units 1 & 2 under rated capacity operation. This Figure is a revision of Figure III-8 in Reference 5.

Figure 2 compares the combined profile shown in Figure 1 with its Reference 5 counterpart. The difference between the two profiles is small and ranges from zero to about 0.1°F.

A brief description of the prototype $\overline{K}$ value used in this study is given in the following paragraph. Our response to question 1 of Section II of the Staff's April 11, 1973 document will discuss the methodology we employed to select the TSF values used in this study.

As indicated in our April 5, 1972 testimony [See page 15 of Reference 6] and in our March 30, 1973 testimony [see pages III-6 & 7 of Reference 5], the previously used $\overline{K}$ values underestimate actual river behavior to a considerable extent.

Reference 17 discussed these values and concluded that:

- They are based upon lake rather than estuarine measurements
- They do not take into account the effect of the higher order terms in computation of the back radiation. Analysis indicate that neglect of this item alone result in 6 to 10% lower $\overline{K}$ values.
- Reference 16 suggests that river evaporative rates are substantially higher (150% higher) than standard lake measurements.

More recent work conducted in 1972 by Hindley & Miner [Reference 11], by Ryan & Stolzenbach in 1972 and by Ryan & Harleman in January of this year reinforced our 1970 findings presented in Reference 17. A comprehensive review of these and other references (3, 7, 10, 11, 12, 13, 16 & 17) indicated that

COMBINED EFFECTS OF DANSKAMMER, ROSETON, INDIAN POINT, LOVETT AND BOWLINE POWER PLANTS ON HUDSON RIVER TEMPERATURES
QL&M'S PREDICTION
- AREA AVERAGE ($\overline{\Delta T}$) TEMPERATURE PROFILES -

CONDITIONS: Q_f = 3,500 CFS
MODEL K = 130 BTU/FT^2 °F DAY

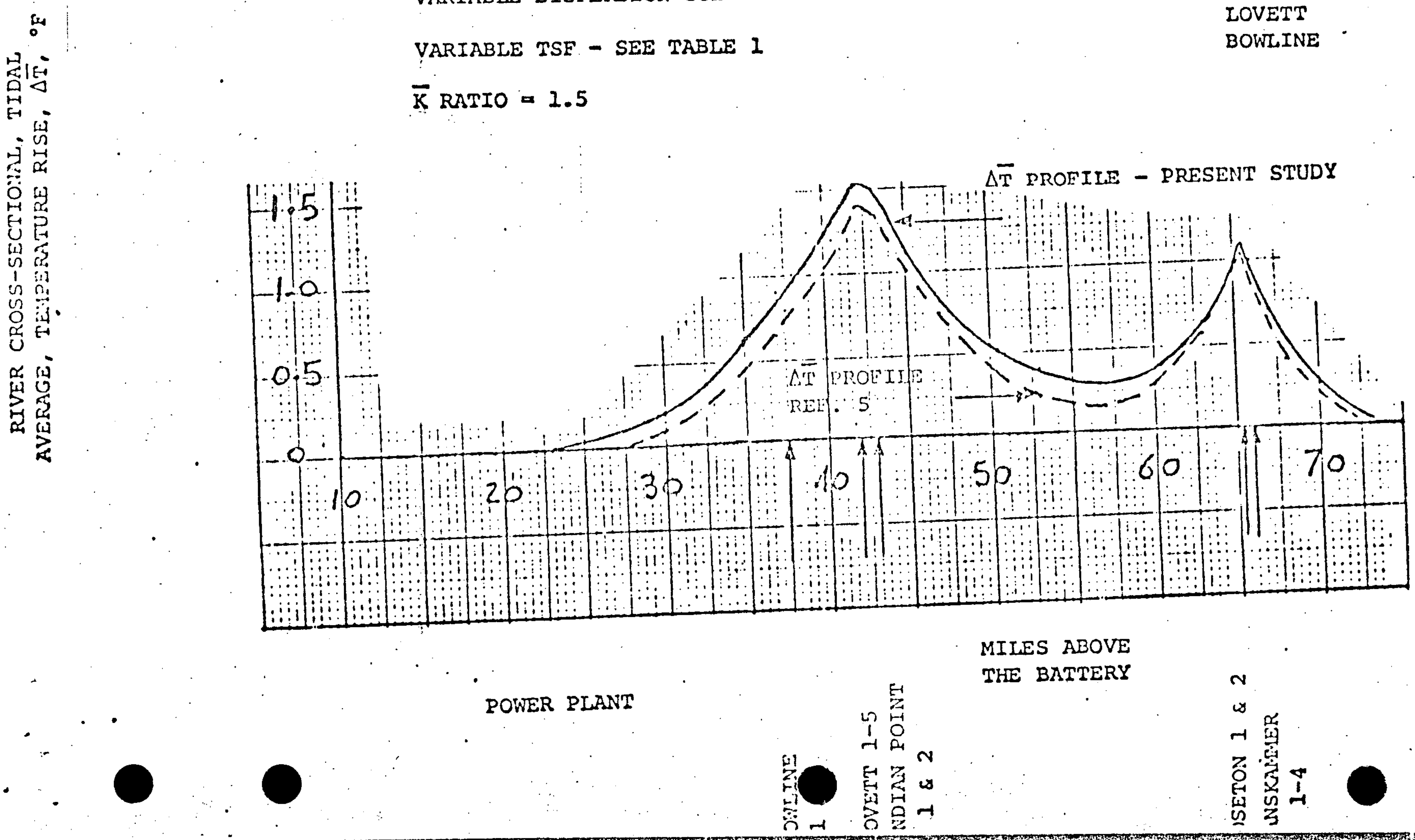
VARIABLE DISPERSION COEFFICIENT

VARIABLE TSF - SEE TABLE 1

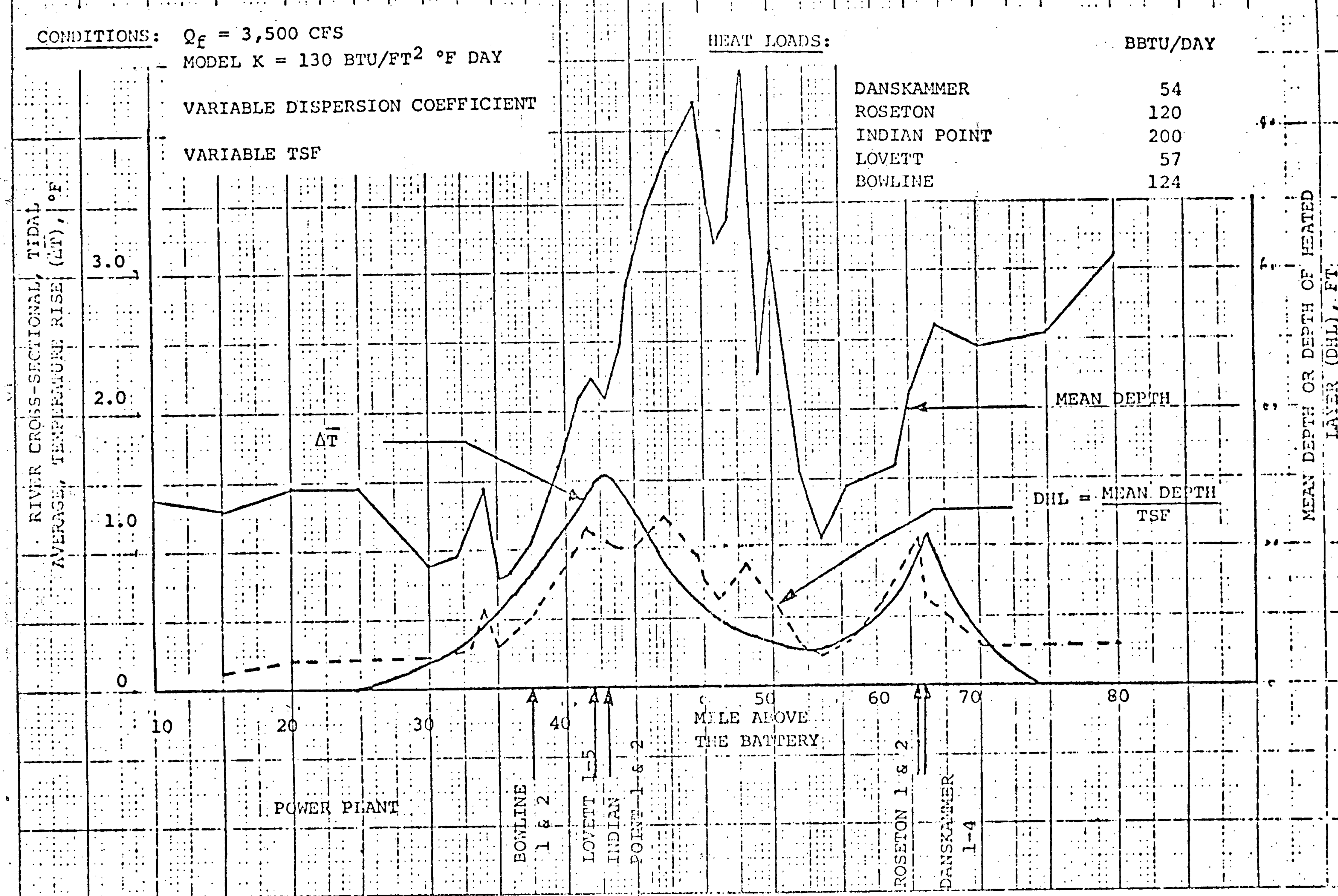
$\overline{K}$ RATIO = 1.5

HEAT LOADS:

	BBTU/DAY
DANSKAMMER	54
ROSETON	120
INDIAN POINT	200
LOVETT	57
BOWLINE	124



COMBINED EFFECTS OF DANSKAMMER, ROSETON, INDIAN POINT, LOVETT AND BOWLINE POWER PLANTS ON HUDSON RIVER TEMPERATURES
QL&M'S PREDICTION
(ΔT - MEAN DEPTH - DHL)



the 1973 MIT method [Reference 13] for predicting the heat transfer coefficient gives results consistent with a wide range of field data. Even this method, at least in the example cited in Reference 13, underestimate $\overline{K}$ by about 20%.

A summary of $\overline{K}$ results derived from all available formulae using the Hudson River August 1964 monthly average meteorological data is given in Table 2. Notice that the results presented in Reference 13 yields Hudson River $\overline{K}$ values ranging from 162% to 194% of the value of 135 BTU/ft^2 °F day in our 1968 report or a ratio of 1.62 to 1.94.

Based upon these findings we have applied in Reference 5 and in the present study a ratio of 1.5 to the previously used $\overline{K}$ values. The previously used $\overline{K}$ values and this ratio have been designated Model $\overline{K}$ and POVERM in the computer program, respectively. Employment of this approach is tantamount to using a prototype $\overline{K}$ value of 195 BTU/ft^2 °F day in the model (or 1.5 X model $\overline{K}$ of 130).

COMPARISON BETWEEN HEAT TRANSFER COEFFICIENT VALUES
DERIVED USING AVAILABLE FORMULAE AND AUGUST 1964 HUDSON RIVER METEOROLOGICAL DATA

Hudson River Data

T_a (air) = 71°F

T_S (water surface) = 78°F

T_D (dew point) = 63°F

T_E (equiv. temp) = 76.8°F

e_a (vapor pres.) = 14.5 mmHg e_s = 26 mmHg

u (wind speed) = 7.6 MPH

Relative Humidity = 62%

$$\left.\begin{aligned} T_S + \Delta\overline{T}_S &= 80°F \\ \frac{T_{so} + T_{si}}{2} &= 80 \end{aligned}\right\} \text{Surface Tem (heated conditions}$$

No.	Source	$\overline{K}$ Value (BTU/sq.ft. day °F)	Remarks
1	Edinger et al (1965)	135	Same as in QL&M Jan. 68 Report (Appendix B)
2	Brady et al (1969)	120	Design Chart (not modified)
3	Brady et al (1969)	129	Approximate Method
4	Brady's design chart (1969)	138	Modified to include effect of thermal discharges
5	Hindley & Miner (1972)	150	Based on their finding K_A = 1.25 Ref. 2.
6	Ryan & Stolzenbach (1972)	205	Using 10°F design chart
7	Ryan & Harleman (1973)	218	Includes forced & free convection & effect of thermal discharges
8	Ryan & Harleman (1973)	262	Case No. 7 modified to reflect the difference between the Susquehanna River measurements and computed values

Request No. 3:

Figures III-7, III-8, and III-9 should be completed to show the extent of the curves below Mile Point 30. Provide input and output data and computer runs for such information.

Response:

The requested information is provided in Plates No. 2 through 5 and Figures 1 and 2 (attached).

Request No. 4:

Provide a copy of the report, "Application of the M.I.T. Transient Salinity Intrusion Model to the Hudson River Estuary," Technical Report No. 153, Ralph M. Parsons, Laboratory for Water Resources and Hydrodynamics, Department of Civil Engineering, M.I.T., prepared under the support of Quirk, Lawler and Matusky Engineers, Tappan, New York, September 1972.

Response:

Attachment No. 2 is a copy of the report requested.

Request No. 5:

Provide all empirical correction factors which have been used in all computer runs to derive the curves in the above mentioned figures.

Response:

No empirical correction factors have been used in this study.

Request No. 6

For any of the observed cases for the power plants in actual operation which Dr. Lawler has relied on for calibrating his mathematical models or used for comparison with Staff predictions, provide all the actual meteorological conditions (wind velocity, humidity, dry and wet bulb temperatures, cloudiness, rain, equilibrium temperatures), actual river conditions (ambient temperatures, salinities, fresh water flows, tidal ranges or water surface elevations, equilibrium temperatures), actual ocean conditions (temperatures, water levels, salinities) and actual power plant conditions (intake temperatures, discharge temperatures, condenser flows, discharge velocities, actual power plant operating loads). Those conditions should be specified for the period of monitoring and data collection as well as those conditions for the period of at least two months prior to that time.

Response

The meteorological data during the Lovett and Danskammer survey periods, actual River conditions and actual power plant conditions are given in References 1, 14 & 15. A copy of Reference 1 which included the Danskammer field observation data used in our March 30, 1973 testimony to compare actual River behavior with predictions made using the Staff's model had been made available to the Staff a long time ago. All three references are included as attachments to the Staff's copy of this document.

Requested information not contained in References 1, 14 & 15 are either described below, readily available from Federal Agencies, or not available at the present time.

1. Equilibrium temperatures were not measured or computed for the monitoring period. However, long-term monthly average meteorological conditions were used in this study to compute equilibrium temperatures (see Appendix B, Ref. 3).

2. Fresh water flows are given in Table 3 (attached).

3. Tidal ranges predicted by USCGS are given in plates #A and #B. These present conditions at the ocean entrance as well as along the Hudson River.

4. Ocean temperature and salinity levels can be obtained from the USC&GS.

For the period of at least two months prior to the monitoring period. the data for all the conditions in question are presently not available except fresh water flow and tidal ranges as shown in Table 3 and Plates A & B, respectively.

TABLE 3

HUDSON RIVER FRESHWATER FLOW AT GREEN ISLAND AND LOWER HUDSON RIVER MONTHLY AVERAGE FLOW, CFS

	1969		1970	
Month	Green Island	Lower Hudson River	Green Island	Lower Hudson River
Oct.	5,173	6,400	4,856	6,000
Nov.	14,400	22,200	14,270	22,000
Dec.	15,600	24,300	11,800	17,900
Jan.	11,680	13,700	8,206	12,000
Feb.	12,760	21,300	15,340	23,800
March	17,470	27,200	15,060	23,200
April	40,730	54,700	39,350	53,000
May	20,910	29,000	14,550	20,000
June	9,995	12,400	6,404	8,000
July	5,430	6,500	5,997	7,100
August	6,102	7,300	3,923	4,600
Sept.	4,133	4,800	6,165	7,400

Request No. 7

For the power plants in operation, provide complete lists of the data collected and measurements taken to monitor the thermal plume. The number of thermocouples used, their locations, and the frequency of measurements taken should be specified.

Response

Generally, boats equipped with six channel and one channel tele-thermometers and thermistors with cables with their appropriate probes (up to 30 or 50 ft in length) were used to continuously measure the water temperatures at and below the surface.

A detailed information concerning temperature survey equipment, procedures, times of run, data collection and measurements taken are provided in Chapter II and associated Appendices of References 1, 14 & 15.

Extensive thermal surveys were conducted in the years 1969 and 1970 at the Danskammer Plant and Lovett Plant at different tidal phases. The following summarizes the number of runs conducted:

Plant	Survey Period	High Water Slack	Low Water Slack	Max Ebb	Max Flood	Total
Danskammer	4/17 - 10/22/69	5	6	12	12	35
Lovett	5/27 - 10/2/69	12	10	15	19	56
Lovett	6/18 - 10/26/70	15	11	9	15	50

It is our position that these survey runs provide a detailed description of the Lovett and Danskammer discharges. We believe that the Danskammer measurements which lasted for a period of several months and covered a range of river flow, tidal, meteorological and plant conditions can be used to compare field observations with mathematical model predictions.

Request #8

Provide the calculations made to evaluate the longitudinal dispersion coefficient, the heat exchange coefficient and the thermal stratification factors which existed during the observation period when the power plants were in operation.

Response

The longitudinal dispersion coefficient (E) and the heat exchange coefficient (E) were not measured during the observation period. However, the E & $\overline{K}$ values used in this study have been obtained using the methods described in Reference 3.

The thermal stratification factor is computed by using the observed surface average temperature rise, $\Delta\overline{T}_s$, and cross-sectional average temperature rise, $\Delta\overline{T}$.

SECTION II OF THE STAFF'S APRIL 11, 1973 REQUEST

Question 1:

The Staff has calculated the heat released into the atmosphere based on Fig. III-8 and found that this heat is about 20% of the total heat dumped into the River by all the 5 power stations. It seems from this that Dr. Lawler has used on the average a thermal stratification factor (TSF) of about 5.

If this is correct, should the same thermal stratification factors be used for evaluating the 4°F excess temperature for the River surface width? How does the thermal stratification factor value of 5 compare with the previous values used in your earlier predictions made in, for example, Lawler's testimony of April 5, 1972 entitled "The Effect of Indian Point Units 1 and 2 Cooling Water Discharge on Hudson River Temperature Distribution?"

Response

The calculated overall thermal stratification factor (TSF) of about 5.0 reflects, in addition to thermal stratification, the influence of the low heat exchange coefficient estimate used in our model. As has been shown before (response to question 2, Section I), our recent analysis indicates that the prototype heat exchange coefficient ($\overline{K}$) is 1.5 times its model counterpart. Therefore, the overall TSF, corresponding to thermal stratification only, should be 3.33 rather than 5.0, i.e., 5/1.5.

The thermal stratification factor is a function of distance. Generally, its value increases as one moves away from the point of discharge. An overall average TSF cannot be used at all points along the River for locating the 4°F isotherm. In our March 30, 1973 testimony (Ref. 5), the TSF values used were segment average values, while the TSF values employed in the April 5, 1972 testimony were only for the plane of discharge. Due to the increase in TSF with distance, the segment average TSF value should be higher than the plane of discharge value. The plane of discharge TSF value used in the April 5, 1972 testimony was 1.5 (Table 7 of Ref. 6) at Indian Point. The Indian Point segment average value used in the present study is 2, i.e., somewhat higher than the plane of discharge value as it should be. It must be recognized, at this point, that selection of appropriate TSF values for a given thermal effluent is not arbitrary and is controlled by the following three criteria:

1. $\Delta\overline{T}_{sx} = \dfrac{TSF_o \Delta\overline{T}_o}{TSF_x}$ (for a discussion of this criterion, see Ref. 2)

   This criterion recognizes that the surface average temperature rise away from a thermal discharge must be equal to or more realistically, less than the plane of discharge value due to heat dissipation.

2. Depth of Heated Layer (DHL) = $\dfrac{\text{Mean Depth}}{TSF}$ (for a discussion of this criterion, see Ref. 1). $DHL_x \leq DHL$.

   This criterion indicates that the effect of a thermal discharge can be considered to be represented by an elevated temperature $\Delta\overline{T}_s$, located at the surface of the River in a layer, DHL thick, and that the thickness of this layer decreases as one moves away from the source due to heat dissipation and buoyancy. (See Figure 3)

3. $\bar{K} \int_{-\infty}^{\infty} \Delta\bar{T}(X)\ TSF\ (X)\ B\ (X)\ dX = \Sigma$ Heat Load

This criterion recognizes that under steady state conditions, the heat input must equal the output.

Selection of a set of TSF values satisfying all of these criteria, particularly in evaluating a multi-plant effect, is difficult and an appropriate set meeting the third criterion and approaching satisfaction of the first two controls is usually employed.

The Applicant agrees with the Staff that comprehensive field post-operational measurements are needed to quantify this and other thermal parameters. However, we believe that employment of TSF values of unity (or strong and complete mixing of the effluent with River waters) throughout the estuary, as was the case in the Staff's model runs, is unrealistic, cannot be supported using existing measurements and does not satisfy the first two criteria, i.e., $\Delta\bar{T}_{sx}$ & DHL controls. One may attain a TSF value of unity at the plane of discharge, by using an appropriately designed diffuser, but maintaining this value away from the discharge and throughout the estuary may not be possible.

Question 2

When using such a TSF, one takes advantage of the assumption that the temperature increases are mainly concentrated on the surface and that the lower layers stay relatively cold. For such a case, it may be assumed that only part of the River depth is actually participating in heat absorption and *dispersion. Was such reduced effective depth considered in Lawler's model? If yes, what value was assumed? If not, why?*

Response

Yes. As indicated above, utilization of a depth of heated layer (DHL) control in selecting an appropriate set of TSF values is tantamount to indicating that the River water below this layer is unaffected by the thermal discharge. The depth of heated layer is determined by the following relationship:

$$\text{Depth of Heated Layer} \doteq \frac{\text{Mean Depth } \Delta T}{\Delta \overline{T}_S}$$

$$\doteq \frac{\text{Mean Depth}}{\text{TSF}}$$

Depth of heated layer values used in this study are given in Figure 3.

Question 3

For Fig. III-8, the excess temperatures were indicated but no mention was made of the actual River ambient temperature. Is this because the excess temperature predictions are almost independent of the ambient temperature? If yes, could you explain again the comments on page III-5?

Response

Yes. As derived in Appendix A in Ref. 3 and shown by Edinger, Geyer and Graves (Ref. 7), the energy transport equation can be written on an ambient temperature basis as follows:

$$E \frac{d^2 \Delta T}{dx^2} - U \frac{d\Delta T}{dx} - \overline{K}_T \Delta T = \frac{\partial \Delta T}{\partial t}$$

where:

ΔT = $T - T_A$

T_A = Ambient temperature

$\bar{K}_T$ = $(T_S - T_{AS})/(T - T_A) = (T_S - T_A)/(T - T_A)$

since the TSF for the ambient condition is 1.0

T_S, T_{AS} = Water surface temperature under heated and ambient conditions respectively

The comments on Page III-5 in our March 30, 1973 testimony are directed at comparing the ambient temperature profile used by Staff with available field data and not at attacking the concept of variable ambient temperature used by the Staff.

Employment of a constant and high summer ambient temperature (79°F) throughout the estuary by the Applicant was done to simplify the analysis and to obtain a conservative estimate of expected thermal effects. Utilization of lower ambient temperature in the lower reaches of the River would have produced lower absolute temperatures in these reaches due to the influence of cooler sea water.

QUESTION #4.

In Figure III-8 it is shown that the average temperature rise at the Indian Point site for simultaneous operation of all five power stations is about 1.3°F. However, in Dr. Lawler's testimony of April 5, 1972, mentioned above, and in Table 4, page 209 of Vo. II of the FES, it is indicated that the temperature rise will be 1.75°F.at the Indian Point site with the Indian Point Units 1 and 2 and Lovett power plants alone in operation. Please explain this contradiction in connection with this comparison. Explain how the values were obtained in Table 6 of Lawler's April 5, 1972 testimony (see also Table 3, page 207, Vol II of FES), based on the equation given on page 206, if "no empirical correction were employed" as claimed.

The Staff has used this equation with no correction factor and with all the numerical values specified by the Applicant and finds for the first case a ΔT of 1.14°F. per 100 billion BTU/day instead of 0.84°F. This means a ΔT of 2.24°F for the heat load of Indian Point Units Nos. 1 and 2 alone. Clarify this point in relation to the 1.3°F given in the present testimony for all five power stations operating simultaneously.

RESPONSE:

The area average temperature rise of 1.75 F in Table 4, page 209 of Vol. II of the FES, is not contradictory to the present result. The difference between the two is caused by the different conditions and models used in the two analyses. These different conditions and models are listed and compared as follows:

* *It should be noted that Figure 1 (Figure III-8 revised) yields a value of 1.5°F rather than 1.3°F.*

Condition & Model		Table 4 Analysis	Present Analysis
1.	Model	Similar to the present model, but 4-segments	Multi-segment Model
2.	Meteorological & Hydro-dynamic conditions	Incipient Salt Flow	Summer
3.	Salinity conditions at Indian Point	100 ppm	6,700 ppm
4.	Model K. BTU/ft^2 F. Day	90	130
5.	Fresh Water Flow, CFS	20,800	3,500
6.	TSF	1.5	2.0
7.	Dispersion Coefficient, Smd	6	11.5
8.	Effect of Lovett Plant	Computed (Over-estimated)	Observed

The Table 4 results represent the so-called "incipient salt flow conditions", while the March 30, 1973 testimony addressed itself to conditions similar to those used by the Staff to show that the Staff's results are unrealistic. It is obvious, therefore, that the above listed eight differences in study conditions prevent direct comparison between the two sets of results.

As indicated in Footnotes 2 and 3 in Table 3 of the FES,(Volume II, page 207) and in Table 6 of the April 5, 1972 testimony, the 0.84°F/100 BBTU/day value was based upon Table 10 of Reference #3 (Reference 4 in this document) which included the plane of discharge conversions factor, f_5 for a TSF value of 1.5. The plane of discharge value presented in Table 10 is 2.84°F per 340 BBTU/day, or 0.84°F per 100 BBTU/day using an f_5 of 0.73. Using an f_5 value of 1.0, this value becomes 1.15°F per 100 BBTU/day or 2.3°F per 200 BBTU/day. The 2.3°F value is consistent with the equation given on page 206 of Volume II of the FES and with the Staff's value of 2.24°F. This value corresponds to a third set of hydrological and meteorological conditions consisting of:

- River flow = 4,000 cfs
- Dispersion coefficient = 12 square miles/day
- Heat transfer coefficient = 90 BTU/sq. ft.,^{o}F, day
- TSF = 1.5
- The f_5 factor was an attempt to incorporate the role of the well established net non-tidal flow used by Dr. Goodyear of the AEC Staff, in his model entrainment, into the analysis.

It must be recognized that the predictions presented in the April 5, 1972 testimony did not use the 0.84°F/100 BBTU/day value. The first paragraph on page 24 of the April 5, 1973 testimony clearly indicates that this value was included in Table 6 for comparison purposes only and that the incipient salt flow conditions, rather than the 0.84°F/100 BBTU/Day which is derived from a convection dispersion model, were used to arrive at the predictions presented in the April 5, 1972 testimony.

While on this topic, it is important to indicate that neither the Applicant's multi-segment model results presented in the March 30, 1973 testimony, nor the Staff's February 8, 1973 temperature distribution findings explicity take into account the role of the well established net non-tidal flow or density-induced circulation concept in the Hudson River.

During normal summer, fall and winter months and on the basis of long-term (1918 to 1970) observations in the Hudson River, the Indian Point site is within the salt-intruded reach. This is the case during the warmest natural thermal period (July, August, September) as well as during the striped bass entrainment period (June and July). During these months, the Hudson River flow available for dilution of thermal discharges (ranging between 21,500 cfs and 35,000 cfs) at Indian Point is significantly higher than the flow and dispersion values used in the Applicant's March 30, 1973 testimony and in the Staff's February 8, 1973 testimony on the effects of multi-plant operation. Therefore, the Applicant's multi-plant thermal findings (Ref. 5) can only be used as representing conservative estimates of the thermal effects during the entrainment period and the warmest natural thermal period.

To determine compliance with the New York State thermal discharge criteria during more critical (non-summer) periods, the incipient salt flow (ISF) conditions must be used. Our April 5, 1972 testimony addressed itself to these conditions and concluded that under ISF conditions the thermal criteria will be met at Indian Point and that the overall temperature rise at Indian Point will be 1.75°F.

Question 5

Dr. Lawler shows in his testimony that there will be no measurable Lovett-plant-induced temperature rises in the vicinity of Indian Point. The effect of Indian Point Unit No. 1 is evaluated by Dr. Lawler to be about 0.2°F area average temperature rise. In Section XII, pages 9-10 of the FES, there are tabulated temperatures which were observed by New York University Staff at the Indian Point site for two excessive years. This observed data show an area average temperature of 80.4°F for August 1968 and 80.66°F for August 1969. In light of the Applicant's repeated position that the maximum ambient temperature of the River is 79°F, can one conclude that the area-average temperature rise observed by New York University is about 1.5°F as compared to 0.2°F claimed by Dr. Lawler, or should one conclude that the River ambient temperature was 80.3°F?

Response

The area-average temperature rise, $\Delta\overline{T}$, cannot be accurately estimated from only 6 (August 14 or 27, 1968) or 8 (August 26, 1969) measurements. More measurements are needed so that spatially weighted average can be conducted instead of simple and direct average. We usually use either continuous recorders or some fifty measurements spanning the entire temperature spectrum and extending from shore to shore and from top to bottom, to arrive at a single area average or surface average temperature rise.

Continuous recorders or some fifty measurements spanning the entire temperature spectrum and extending from shore to shore and from top to bottom, to arrive at a single area average or surface average temperature rise.

The operation of Indian Point Unit No. 1 definitely affected the N.Y.U. measurements at the east station, the one designated (near effluent). The operation of Lovett Plant might have affected a small west side portion of the survey cross-section, but the effect of the Lovett Plant on cross-sectional average rise should be expected to be less than its effect on the west side portion.

As indicated in Reference 9, in addition to recirculation and thermal effects resulting from operation of Indian Point and Lovett, the N.Y.U. observations referred to by the Staff, were made using conventional temperature instruments rather than precision thermometers and using centigrade rather than Fahrenheit units.

Review of available observations, other than NYU's, made during these days indicated that due to the above-lighted reasons and the fact that the major objective of the N.Y.U. program was the biological activity rather than temperature distribution, per se, indicate that the NYU measurements can neither be used to precisely evaluate the effect of Indian Point Unit 1 or to extract supportable ambient temperature levels. A brief comparison between the NYU observations and those made by others is given below:

Item	Date	Reported NYU Value, °F	Counterpart* reported by others, °F	Source
1.	Aug. 14, 68 East, bottom	80.1	79.0	Standard Brands Inc intake @ Charles Poi 27 ft. deep
2.	Aug. 14, 68 East, bottom	80.1	79.0	Indian Point intake (discharge temp. = 8
3.	Aug. 27, 68 East, bottom	79.9	79.0	Same as Item 1
4.	Aug. 27, 68 East, bottom	79.9	79.0	Same as Item 2
5.	Aug. 27, 68 West, top	80.4	78.8	USGS Station at Jone Point max. surface t a min. value of 75.1 was recorded on that day
6.	Aug. 26, 69 West, top	80.2	77.2	OL&M Lovett measure
7.	Aug. 26, 69 West, bottom	80.1	77.1	Same as Item 6
8.	Aug. 26, 69 East, bottom	80.1	76.0	Same as Item 1
9.	Aug. 26, 69 East (near East Shore, top)	81.1	78.0	Same as Item 2
	Average	80.21	78.12	

* Counterpart temperatures for the remaining NYU values are not available.

It is clear from the above-tabulation that the disagreement between the NYU observations and those made by others make employment of the NYU values for purposes other than those related to the NYU study objectives questionable.

QUESTION #6.

Based on the same observed data one can see that the thermal stratification factor (TSF) is very close to 1.0 (1.012 for August, 1968 and 1.018 for August, 1969). How do these values agree with the values Dr. Lawler has been using in his testimony of March 30, 1973 for the five power stations on the Hudson River?

RESPONSE:

By definition, the thermal stratification factor (TSF) is the ratio of the overall surface average temperature rise, $\Delta\overline{T}_s$, and cross-sectional average temperature rise, $\Delta\overline{T}$. Both $\Delta\overline{T}_s$ and $\Delta\overline{T}$ are also tidal averages. The $\Delta\overline{T}_s$ and $\Delta\overline{T}$ should be properly weighted. In addition and as indicated in our response to the previous question, the N.Y.U. observations cannot be used for this purpose.

LIST OF REFERENCES

1. Quirk, Lawler & Matusky Engineers, "Effect of Roseton Plant Cooling Water Discharge on Hudson River Temperature Distribution and Ecology" Report submitted to Central Hudson Gas & Electric Corporation, December 1969. Some data presented in the above report were updated according to more recent studies.

2. Quirk, Lawler & Matusky Engineers, "Environmental Effects of Bowline Generating Station on Hudson River" Report submitted to Orange & Rockland Utilities and Consolidated Edison of New York, Inc., March 1971.

3. Quirk, Lawler & Matusky Engineers, "Effect of Indian Point Cooling Water Discharge on Hudson River Temperature Distribution", Report to Consolidated Edison Company of New York, Inc., January 1968 (Appendix I of the Environmental Report).

4. Quirk, Lawler & Matusky Engineers, "Effect of Indian Point Cooling Water Discharge on Hudson River Temperature Distribution", Report to Consolidated Edison Company of New York, Inc., February 1969 (Appendix J of the Environmental Report).

5. Additional Testimony of John P. Lawler, Ph.D., Quirk, Lawler & Matusky Engineers, on the Cumulative Effects of Bowline, Roseton and Indian Point Generating Stations on the Hudson River, March 30, 1973.

6. Testimony of John P. Lawler, Ph.D., Quirk, Lawler & Matusky Engineers, on the Thermal Effects of Indian Point Cooling Water on the Hudson River, April 5, 1972.

7. Edinger, J.E., Geyer, J.C. and Graves, W.L., "The Variation of Water Temperatures Due to Steam Electric Cooling Operations." Presented at the Research Symposium of the 1964 Water Pollution Control Federation New York Conference (October 8-13, 1967).

8. AEC Regulatory Staff, "Consideration of Other Power Plants, Enclosure No. 1: Preliminary Study of the Expected Temperature Distribution in the Hudson River as a Result of Operation of Danskammer, Roseton, Indian Point Units 1 & 2, Lovett, and Bowline Power Stations," February 8, 1973.

9. Quirk, Lawler & Matusky Engineers, "Supplemental Study of Effect of Submerged Discharge of Indian Point Cooling Water on Hudson River Temperature Distribution," Report to Con Edison, May 1972.

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11. Hindley, P.D., R.M. Miner, "Evaluating Water Surface Heat Exchange Coefficients", Journal of the Hydraulics Division, Proceedings of the ASCE, August 1973.

12. Ryan, P.J. and Stolzenbach, K.D., Chapter 1: "Environmental Heat Transfer", Engineering Aspects of Heat Disposal from Power Generation, (D.R.F. Harleman, Ed.), Ralph M. Parsons Laboratory for Cambridge, Massachusetts, June 1972.

13. Ryan, P.J. and D.R.F. Harleman, "An Analytic and Experimental Study of Transient Cooling Pond Behavior", Ralph M. Parsons Laboratory for Water Resources and Hydrodynamics, Massachusetts Institute of Technology, Report No. 161, January 1972.

14. Quirk, Lawler & Matusky Engineers and Oceanographic Analysts, Inc., "Effect of Lovett Plant Cooling Water Discharge on Hudson River Temperature Distribution and Ecology", Report submitted to Orange & Rockland Utilities, Inc., November 1969.

15. Quirk, Lawler & Matusky Engineers, "Environmental Effects on Hudson River, Lovett Plant Unit #5 Submerged Discharge", Report submitted to Orange & Rockland Utilities, Inc., March 1971.

16. Tichenor, B.A.: "Water Temperature Prediction. Technical Seminar on Thermal Pollution", Southeast Water Laboratory, Athens, Georgia, January 9-10, 1969.

17. Quirk, Lawler & Matusky Engineers, "Hudson River Water Quality and Waste Assimilative Capacity Study", Chapter X, Report Submitted to State of New York, Department of Environmental Conservation, December 1970.

MR. KARMAN: Mr. Chairman, in the document entitled "Set 2," which were the Staff responses to interrogatories for Dr. Goodyear on Staff commetns on the Applicant's research program, on page 11, on the 14th line from the top, the word "good" should be changed to "food." An "f" instead of a "g."

CHAIRMAN JENSCH: Will you undertake to correct the copies which have been given to the reporter for inclusion in the transcript?

MR. KARMAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Very well.

With that as a basis, is Dr. Goodyear available for cross-examination?

MR. KARMAN: He is.

MR. TROSTEN: Mr. Chiarman, may we please handle this matter of the other interrogatory posed to the Regulatory Staff by Mr. Macbeth so we can get it out of the way?

CHAIRMAN JENSCH: Proceed.

MR. TROSTEN: Mr. Chairman, a copy -- by letter dated April 20, 1973, the Regulatory Staff through Mr. Karman responded to an interrogatory posed to Mr. Macbeth, posed by Mr. Macbeth, concerning xenon override.

Here is a copy of the letter, Mr. Chairman, in case you don't have one readily available.

CHAIRMAN JENSCH: If I recall it was something about ups and downs?

MR. TROSTEN: That is right. For some reason Mr. Macbeth seems to be unwilling to offer the answer to his question into evidence and I don't quite understand the problem.

In any event I offer it in evidence on behalf of the Applicant.

MR. MACBETH: Mr. Chairman?

CHAIRMAN JENSCH: Mr. Macbeth?

MR. MACBETH: I object on the grounds that it is an answer from Mr. Karman not from any identified expert on the Staff. I think first we need some identification of who in fact is answering this question and whether he or she is technically competent to answer it.

Secondly, if it is admitted, I would then ask the Staff to actually produce the witness because as the Staff frankly says in the first line of the response to the question, the Staff has not made an estimate of the incapacitated period specified in the question. In other words, to put it bluntly, they have not answered the question. In that case I would like to have a witness here who would answer the question.

There is a kind of general description of various xenon problems, but it is at a level of generality that

that simply demands for specificity. That is the basis on which I was not going to offer it.

I don't know who answered it, and it hasn't been answered; I would like to see it answered if we are going to go forward with any further evidence on the question.

CHAIRMAN JENSCH: It stands as hearsay as against your position; is that correct?

MR. MACBETH: Yes. I mean, I respect Mr. Karman, but I really would like something a little stronger than his sitting as the answer.

MR. TROSTEN: Well, Mr. Karman, can you advise the Board and the parties as to the author of this?

MR. KARMAN: This was a combined effort on -- by several members of the Regulatory Staff, Mr. Chairman. When we furnish interrogatories -- an interrogatory such as this, we leave it usually to the recipient to decide what he wants to do with it, whether or not he wants to make use of it This was the information which was given to me by members of the Regulatory Staff for my submission to Mr. Macbeth based on the question that was posed to us.

CHAIRMAN JENSCH: It is an interesting document. We end up with the necessity of a body, I take it.

MR. TROSTEN: Do you --

MR. KARMAN: I have not offered it.

MR. TROSTEN: I know. Do youhave someone who can support this, Mr. Karman, who can walk over here from Norfolk Avenue and support the document?

MR. MACBETH: Let's be frank. He is going to have to do more than that. He is going to have to answer the question. The Staff didn't answer the question. That is the first thing they say.

Having put the question, if something is going to go in, I would like to have a response go in.

CHAIRMAN JENSCH: I take it the Applicant wants to put on some evidence to answer your question. I take it on behalf of the Applicant, that the witness should be provided to support the position he seeks to establish.

Is there anything further we can take up at this time?

If not, shall we proceed with the cross-examination of Dr. Goodyear?

MR. TROSTEN: Would you rule on my offer, Mr. Chairman? We are back to another case. Somehow when documents that are prepared by the Regulatory Staff are offered, it is another case like the Division of Compliance report on the situation at Indian Point 1. They are not reliable somehow --

CHAIRMAN JENSCH: Because they weren't signed, is that it?

MR. TROSTEN: This one was signed, but by Mr.

Karman. The problem is we have the wrong person signing it.

CHAIRMAN JENSCH: I don't think that is the principal objection. The principal objection is the question wasn't answered. It is an interesting question.

The objection is sustained.

MR. TROSTEN: I have a few questions to pose to Dr. Goodyear with regard to his testimony of April 23rd.

XXXXXXXX

Whereupon,

DR. C. P. GOODYEAR

was called as a witness on behalf of the Regulatory Staff and having been previously duly sworn, was examined and testified further as follows:

FURTHER CROSS-EXAMINATION

BY MR. TROSTEN:

Q This is entitled "Staff Analysis of Artificial Propagation to Replace Hudson River Fishes Killed by Power Plant Operation."

CHAIRMAN JENSCH: Thank you.

BY MR. TROSTEN:

Q Dr. Goodyear, in this document which has now been received in evidence --

MR. KARMAN: Could you use the microphone, Mr. Trosten, please?

MR. TROSTEN: Yes.

BY MR. TROSTEN:

Q You have presented a new estimate differing from the two previous estimates that you presented during the past week or so as to the number of female striped bass that would be required to replace those that are killed by power plant operations. This appears on pages 11 and 12.

It is unclear to me the basis for your conclusions in this respect, and I would like to ask you a few questions about this.

In the first place, when you say --

CHAIRMAN JENSCH: Excuse me just a minute.

I don't have any pagination.

MR. TROSTEN: This particular copy is numbered, Mr. Chairman. Up at the very top right-hand corner it says page 11. It appears under heading 7, Staff estimate of stock size.

Perhaps I could identify it for you like this. It is the 11th page.

End #17

CHAIRMAN JENSCH: Thank you very much.

#18

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BY MR. TROSTEN:

Q Do you have the page, Dr. Goodyear, before you?

A Yes.

Q The heading is entitled "Staff Estimate of Stock Size Required to Replace 15 Million Juvenile Bass Which Are Killed by Power Plant Operations." It is not clear to me what you mean by power plant operations. Would you specify the power plants that are the subject of this analysis, please?

A The power plants that I am -- configuration that I was concerned with would include all of the plants that are operating on the river. The 15 million estimate is not a firm estimate for the number of fish which would have to be replaced.

Q Well, let's take it one at a time.

Do you have a copy of your February 14 testimony in which you analyze the multi-plant effects?

I have a copy here. I just want to turn to that and make sure we know what we are dealing with here.

I am referring to Table 1 -- of your papers entitled "Probably Reduction in Survival of Young-of-the Year Striped Bass in the Hudson River as a Consequence of the Operation of Danskammera, Roseton, Indian Point Units 1 and 2, Lovett and Bowline Steam Electrical Generating Plants," dated February 8, 1973.

MR. KARMAN: We have it in front of us.

MR. TROSTEN: Yes.

BY MR. TROSTEN:

Q Now when you refer to power plants, do you mean -- well, tell me now what are the particular plants that you mean? Would you name them so I can be sure that I know which power plant you are referring to?

MR. KARMAN: When you say to power plants, refer to it in which testimony?

MR. TROSTEN: The testimony of April 23rd, page 11.

THE WITNESS: The estimate that's given on page 11 is based upon an absolute number of fish.

Now that number of fish could correspond to more than one configuration of these plants or it might be in error entirely.

In other words, what I am saying is that the estimate for the total mortality in the population as a result of power plant operations is going to be on the order of 50 to 60 percent based on this earlier testimony with an assumption of approximate annual production of two-inch fish of around 30 million. 15 million would be half of that.

BY MR. TROSTEN:

Q Dr. Goodyear, I am afraid you are not answering my question. When you refer to power plants, do you mean Roseton, Danskammera, Bowline and Lovett, Indian Point

1 and 2, is that what power operation means?

A Yes.

Q Thank you.

Now when you refer to an estimate of 15 million juvenile bass which are killed, would you please state the assumptions upon which you base this calculation of 15 million?

If you like, Dr. Goodyear, you could turn back to your set one interrogatories which are attached to your -- to Mr. Karman's April 20, 1973 letter to Mr. Macbeth, and you will see on the third page of that set that there's a list of assumptions; and if you want to use the same format, you could do that; or if you want to use some different format, you could do that.

A Well, if you were to use this format, the assumptions would be a natural production of two-inch fish. It will -- to 30 million individuals. A replacement requirement of 50 percent of that number.

Q Would you stop there for a minute so I can ask you a question about that?

When you say a replacement requirement of 50 percent of that number, what do you mean?

A I am having some difficulty --

(Pause.)

Q What does that mean? I don't understand what you mean by the phrase "a replacement requirement of 50 percent" of 30 million.

A Well, if the object of the hatchery effort is to maintain constant recruitment or recruitment at a pre-operational level, where the population had been at 30 million, and power plant operations were to cut that number by a factor of two, then the replacement production that would be required to be added to the river, to end up with the original number, would be --

Q Okay. I understand.

Go through the rest of your assumptions, please, if you would.

A It also assumes there is no exploitation of the spawning stock by the hatchery operation so that natural production would remain constant.

Q May I ask --

A Natural egg production would remain constant.

Q Does that mean when you say "no exploitation," that the spawning stock doesn't come from the Hudson River? Is that what you mean by that?

A It could either be from another source or from the Hudson, but from normally exploited stock by the fishery.

Q Okay.

Go ahead.

A The other main assumption is that the ratio of -- could I have a moment?

Q Yes.

(Pause.)

A Could I have the question read back?

CHAIRMAN JENSCH: Do you want to restate it? Which is easier?

MR. TROSTEN: The question is upon which do you base the assumptions?

THE WITNESS: That would be the assumptions.

BY MR. TROSTEN:

Q You have completed the list now?

A Yes.

Q All right. Is it not a major assumption of 50,000 viable eggs per pound of mature female bass?

A Not for the 15 million fish.

Q And it is not a major assumption of 10 pounds per female bass?

A Not for the 15 million.

Q Now what is the basis for your assumption of 30 million two-inch fish by way of natural production?

A Well, the discussion of that assumption is part of the testimony; and, of course, it is subject to quite a bit of error because of the type of information that's used to evaluate it.

But essentially it is just an extrapolation from Carlson-McCann.

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Q From a particular year?
A 1967.
Q 1967?
Dr. Goodyear, does the 30 million correspond to the 30 million that appears in Figure 1,in your chart on Figure 1, the horizontal -- those two connected points, the second and third from the right-hand side? On Figure 1, page 10 of your April 23rd testimony?
A Basically, yes. The value that's obtained from the average of the first three -- or those three population estimates, from the end of June through mid-July, averages to 28 percent, rounded to 30. Excuse me, 28 million.
Q Over what dates does this figure of 30 million apply? Over what dates is the population of 30 million?
A I'd have to check to give you the exact date, but approximately from the middle of the last week in June through the middle of the third week in July.
Q How large were those fish?
A They are about seven-tenths of an inch.
Q Do you know how old they were?
A Most of them were probably on the order of five weeks.
Q Now what is the basis for your assumption that there is a replacement requirement of 50 percent of the 30 million two-inch fish?

MR. KARMAN: I thought that was answered about five minutes ago.

MR. TROSTEN: No, I don't think so.

THE WITNESS: The basis of that is the information that was presented in Table 1 of the --

BY MR. TROSTEN:

Q Table 1 of the April 23rd testimony?

(Pause.)

A It is a multi-plant --

Q Table 1 in the multi-plant. All right.

Could you point me to the particular section that leads you to assume a 50 percent replacement requirement?

A The generalization that the combined effects of all of the plants as tallied in that table.

Q I see. For the Roseton, Danskammera, Indian Point Unit 2, Lovett, and Bowline Plants?

A Yes.

Q I see.

A That percentage reduction for that configuration for all of the estimates is around 50 percent.

(Pause.)

Q Now what is the entrainment period that's employed in your model?

A 63 days.

However, I might point out the vulnerability

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to entrainment decreases from the 42nd day on, so that it is not every fish that is exposed for the 63-day period.

In other words, the average exposure, average duration of exposure, is approximately 52 days.

Q Dr. Goodyear, turning back to page 3 of set one of your interrogatories and response to Mr. Macbeth's questions --

MR. MACBETH: Just to be clear, I think they are your questions.

MR. TROSTEN: Excuse me. Those are our questions.

BY MR. TROSTEN:

Q You have a statement here, the first assumption you state is that the hatchery production is one half of natural production. What do you mean by that statement?

A The number of fish to be produced by the hatchery is one half of the natural production of that size group of fish in the estuary.

Q Is that the same thing that you said before, that there is a 50 percent replacement requirement?

A Yes.

Q I see.

(Pause.)

MR. KARMAN: Are you going to be referring to any document we might be looking at, Mr. Trosten?

MR. TROSTEN: Yes. What I am trying to be clear

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about is this: The response to this set of interrogatories appears to me to indicate that Dr. Goodyear's supplemental Staff testimony on the feasibility of a fish hatchery, replacement stocking of fish in the Hudson River, was addressing itself to the combined impact of all of the power plants on the river, not just Indian Point 1 and 2.

BY MR. TROSTEN:

Q Is that correct?

A I am not sure that we are together on the testimony.

MR. TROSTEN: May we have a five-minute recess, Mr. Chairman?

CHAIRMAN JENSCH: Yes. At this time let's recess to reconvene in this room at 4:30.

(Recess.)

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CHAIRMAN JENSCH: Please come to order.

Is Applicant ready to proceed?

BY MR. TROSTEN:

Q Dr. Goodyear, would you look at page 3 of Set 1 of the Staff's responses to Applicant's interrogatories on the fish hatchery?

A I have got it.

Q Do you see the statement that the hatchery production is one-half of natural production?

Were you assuming with regard to that statement that all the power plants were operating and hence there would be a 50 percent replacement requirement?

A One moment.

(Pause.)

The assumption that was made in reference to the size of the hatchery production was that it would be half of natural production. It was not directly referenced to any power plant configuration.

Q What was the basis for the assumption that there would be a requirement for replacement of half the natural production? What -- was there no basis, just a number you picked out? Was there some basis?

A Again it is within the range -- within the upper limit of the range that we associated with both Indian Point and the combined effects of all of the plants.

Q Well, the upper range for the Indian Point 1 and 2 is 43 percent, the upper range. The lower range is 14 percent.

A The -- I am afraid we have a mixture of two types of factors contributing to the estimate. One is connected with the error believed to be associated with technique; and another which is a natural alteration of the exposure of the fish due to the environmental variables.

The upper estimates happen to coincide with the normal flow conditions; in other words, the 1970 and '69 flow conditions are more similar to the median flow conditions than they are -- than are the other years that are listed.

The effect of that is that the project that was made in the final environmental statement of the 30 to 50 is based on median flow situation. Rather than on a range in flow situation so that the error that is associated with 30 to 50 is on 40. In other words, a 10 percent, plus or minus, can be associated with the estimate of median situation, simply with the assumptions inherent in modeling the location of the current itself and its spawning distribution.

Q Could you tell me what is the basis for the choice of 50 percent replacement requirement? Does it have nothing to do with how many plants are operating on the river?

A It was taken as an assumption, a point of departure for investigating the size of the stock that would be required for artificial propagation.

Q It isn't related to how many plants are operating on the river or what the actual replacement -- what the actual number of fish removed by entrainment and impingement is?

A No, it is not.

Q Thank you.

Now, would you turn to the response to the Hudson River Fishermen's Association's interrogatory on the fish hatchery which is attached to Mr. Karman's letter to Mr. Macbeth dated April 20, 1973?

MR. KARMAN: We have it.

BY MR. TROSTEN:

Q Now, there again you assume a 50 percent mortality -- excuse me. Strike that.

You have a statement in there, "Assuming a 50 percent mortality of these fish upon stocking in the Hudson River."

Is that assumption the same as the assumption we have been talking about the past few minutes about the requirement for replacement of 50 percent of the natural production?

Is that a different way of stating the same?

A No.

Q It is a different point?

A It is a different point. The 50 percent mortality, which is a part of that response, is associated with the survival of the fish that are put back in the river.

Q Okay. All right.

Now, Dr. Goodyear, in your response to Mr. Macbeth's -- you have presented three calculations in this proceeding as to the number of female bass which would be required: 2000 female bass, 2700 female bass, and 3700 female bass.

Would you please explain the difference and the reason for these three estimates? What is the basis for these three different estimates as to the number of females bass that would be required to replace those lost in power plants?

Are you familiar with the three estimates and where they appear?

For your information, the first one appears in your testimony of April 9 entitled Supplemental Staff Testimony on the Feasibility of the Fish Hatchery and Replacement by Stocking of Fish in the Hudson River, page 8.

The second one, the 2700 estimate, appears in your response to Mr. Macbeth attached to Mr. Karman's letter of April 20.

And the third one, 3700, appears on page 12 of your testimony of April 23, 1973.

A All right.

The first estimate was just presented to indicate the size of the spawning stock that is utilized in the Hudson. It assumes the hatchery survival would be the same as natural survival throughout the entire course of the propagation and assumes that the fish which are removed from -- the spawning stock is not being removed from the river.

That is how the 2000 estimate came about, just a very simple straightforward computation of the size or -- based on the assumptions that are presented in Response 9 on page 3 of the Set 1 interrogatories.

Q Well, is the 2000 fish intended to be representative of the number of female spawners that would be required? That is what I had thought you meant on page 8 of your April 9 testimony.

You said "Based on the experience of some of these hatcheries, the Staff has estimated that in order to replace only the striped bass killed by plant operation, a hatchery operation would have to replace about half the number spawned, about two billion eggs at an average of 500,000 eggs per female. This would be equivalent to an eight to ten pound bass, 2000 eggs."

I assume from that language you meant that this is the number of female bass that would be required for this spawning operation.

CHAIRMAN JENSCH: You have that document before you?

THE WITNESS: Yes. I think I see what the problem is.

The intent -- the intent that was inherent in that discussion was not to say how many fish would be required by the hatchery, but how many fish it represented in the field.

In other words, it is just a very simple computation that was presented solely to indicate the size of the stock that is being utilized. At that point there was no assumption of the -- of whether or not the hatchery production would be more or less efficient. It was considered to be the same.

The response to the question that Intervenors raised, the estimate was revised to include some information based on hatchery production, actual production.

BY MR. TROSTEN:

Q So is it your testimony in this proceeding that in order to replace the Hudson River striped bass taken by Indian Point 1 and 2 you would need 2700 ten-pound fish; is that right?

This is contained in the answer to Mr. Macbeth's interrogatory attached to Mr. Karman's letter of April 23, 1973?

CHAIRMAN JENSCH: Of course, from a strictly legal point of view, as I understand it, that is not in evidence.

MR. TROSTEN: Yes, sir. It is in evidence.

MR. KARMAN: It is in yesterday's transcript.

CHAIRMAN JENSCH: I was wondering if this was considered the opposite of the monkey in space program.

MR. KARMAN: It follows page 11,047.

CHAIRMAN JENSCH: Thank you.

(Pause.)

THE WITNESS: The estimate that is presented there in the response to the Intervenors' interrogatory is based on less information than the response that was presented in the April 23rd testimony.

BY MR. TROSTEN:

Q What is the new information you obtained that added to your knowledge in this area?

A It is all of the information presented in Table 1; and the estimate of standing crop of young of the year striped bass, of that age group.

Q Let me be sure I understand this. Did you revise your estimate from 2700 to 3700 based upon additional information which became available to you as to the efficiency of the hatchery operation? Was that the reason?

A The inclusion of more information in the analysis, yes.

Q All right. And was there something else? You said something about the standing crop in the Hudson? Was -- what was that additional information?

A Well, the actual standing crop of eggs of that age group of fish in the Hudson has not been the subject of an absolute estimate on my part, anyway, before now. All the estimates that I have attempted to make have been relative numbers; however, the hatchery efficiency information is based on absolute numbers which does provide a very difficult situation in terms of making the comparison, if you are computing a number to obtain an estimate of the number of fish which could be -- or would have to be stocked in the river.

So the estimate of 30 million fish of that age group, from the standing stock of the estuary, is a more -- is a fairly recent estimate.

Q What number did you use for standing crop of two-inch fish when you made your estimate of 2700 ten-pound females in response to Mr. Macbeth's interrogatory?

A Ten million.

Q Ten million? And you raised it by a factor of three?

A No. Excuse me. It was twenty million.

Q Twenty million?

Now, in your response to Mr. Macbeth's interrogatory you estimated that the hatchery will be required to replace ten million juvenile fish. You see that?

A Yes.

Q And in your testimony of April 23, 1973, you revised that to an estimate of 15 million juvenile fish?

A Yes.

Q What was the basis for the revision in this estimate?

A The change from an assumption of twenty million young of the year to thirty million young of the year in the standing crop.

Q That was the only basis for the difference?

A Yes.

Q It was not related to the fact that your response to Mr. Macbeth dealt with Indian Point 1 and 2 whereas your April 3rd testimony dealt with all the plants?

A As we discussed before the assumption, baseline assumption of the proportion of the stock which would have to be replaced was 50 percent. You can make a linear extrapolation of that to the proportion that would have to be replaced or the proportion of the adult stock that would have to be utilized for any assumed number of fish that would have to be replaced.

Q Dr. Goodyear, have you been responsible for the operation of a hatchery?

A No.

Q Or the stocking of fish in a water body?

A Yes.

Q In what -- what was the occasion where you were responsible for such an operation?

A It was a study to test the capability of rearing an insecticide-resistance-trained mosquito fish. In graduate school, when I started graduate school, I was supported by -- I am not sure which agency now, but a research grant which was directed at determining the effects of crop pesticides in the Mississippi Delta region.

We had identified insecticide-resistant strains of fish in that area, and wished to see if we could replicate the selective process and raise insecticide-resistant strains from local stock; and in doing so, we reared and planted fish in a number of different ponds.

MR. TROSTEN: I have no further cross-examination of Dr. Goodyear, Mr. Chairman.

CHAIRMAN JENSCH: Hudson River Fishermen's Association?

MR. MACBETH: Yes, Mr. Chairman.

BY MR. MACBETH:

Q Dr. Goodyear, on page 15 of the additional testimony of John P. Lawler, on the contribution of Chesapeake Bay to the striped bass fishery in the Middle Atlantic states, Dr. Lawler presents some additional comments on your testimony of April 9, 1973; and the first one concerns the relationship between water withdrawal by Hudson River power plants and Mid-Atlantic landings five years later; and Dr. Lawler says that the decline of Mid-Atlantic landings is more related to national fluctuations rather than planned effects.

DR. GEYER: What page, please?

MR. MACBETH: Page 15.

BY MR. MACBETH:

Q Have you reviewed that part of Dr. Lawler's testimony?

MR. TROSTEN: Mr. Chairman, I would like to ask if Mr. Macbeth is cross-examining Dr. Goodyear or calling him as his own witness?

MR. MACBETH: Well, I'll --

MR. TROSTEN: Are you calling him as your witness?

CHAIRMAN JENSCH: What is the basis for the inquiry?

MR. TROSTEN: Well, this is intended to be cross-examination with regard to testimony that has been offered by Dr. Goodyear. Mr. Macbeth is asking a question about Dr. Lawler's testimony; and it appears to me that he is calling this witness as his own.

I want to establish that for the record before we proceed so I can determine what my position is going to be on the questioning.

CHAIRMAN JENSCH: Well, I think we are having a little trouble with your premise. There has been such a necessity to consider the statements made by several persons in the proceeding concerning their views on various aspects of this matter. Now, Dr. Lawler's testimony is in the record. In many respects it tests the validity of some of the contentions that Dr. Goodyear has presented. It may involve a comparison with some other comments from another witness who has spoken to the same subject or some aspect of it.

I don't think a reference to another person's evidence necessarily makes the witness being examined a witness of the -- a witness being interrogated with reference to another subject.

MR. TROSTEN: May I ask you this question: Have you discussed with Dr. Goodyear, prior to raising this question, the question you are about to ask of him?

MR. MACBETH: In a very general way, I have. I don't know, though, that that's --

MR. KARMAN: The same way you have, Mr. Trosten.

CHAIRMAN JENSCH: I thought that was one of the reasons we were hoping that this seems to be expedited.

MR. KARMAN: May I say, Mr. Chairman, we have been doing this all along. We have certainly indicated to the applicant our areas of cross-examination and the applicant has done it with us. Mr. Macbeth has done it with the applicant and with us.

I don't understand the question at this stage of the hearing.

CHAIRMAN JENSCH: I don't either.

Any other thing you would like to try?

MR. TROSTEN: I would like to have an answer to my questions. I haven't received one to either of my questions.

MR. MACBETH: I didn't generally ask Dr. Goodyear about it. I discussed questions I was going to ask with your witnesses.

CHAIRMAN JENSCH: Proceed.

BY MR. MACBETH:

Q Have you had a chance to review that paragraph of Dr. Lawler's testimony?

A Briefly.

Q And having reviewed it, what is your opinion as to whether or not the decline is more related to national fluctuations or to planned effects?

A Well, what exactly do you mean by national fluctuations?

Q Well, I -- it is somewhat difficult to do this, having not had an opportunity yet to cross-examine Dr. Lawler on it. Dr. Lawler discusses fluctuations in the early 1930s, the mid 1940s, and the early 1950s. I would assume those are the national fluctuations to which he refers.

Is this -- has this decline -- is this a recent decline -- in your opinion, of the same origin as the fluctuations of the early 30s, mid 40s, and the early 50s?

A No.

Q And what is the basis of that opinion?

A Well, the fluctuations in the early 30s can be associated and very likely is associated with the fishing intensity of the shad fish in the Hudson River and the implementation of legal restrictions on the fish which could be taken by the fishermen.

The shad fishery was very intense in the very early 20s. The intensity stayed at a fairly high level for several years; more than one generation of bass would have been influenced -- the spawning potential of more than one generation of bass would have been influenced in that manner.

Twelve years later -- or -- yes, about 12 years later, the 1934 year class was produced at a time when the fishing intensity had been low for approximately two generations.

The fishing in the river, the shad fishery again increased in the late 1940s and early 1950s which would again reduce the survival of spawning stock in the river, which would be reflected by a subsequent reduction in the Mid-Atlantic stock.

In the late 50s, the fishing intensity declined throughout the Atlantic, Mid-Atlantic region, and in the Hudson River itself.

The spawning stock reached a peak in 1959 and '60 with a resultant production in very large year classes subsequent to that.

However, the fishing intensity has remained low in the Hudson, and one would expect that the proportion of stock escaping the fishery would remain high; likewise, the fishing intensity along the Coast, the seine haul fishery, has stayed at a very low rate.

One would expect from that information that the population would not decline in the way that is indicated but would continue to grow and that it would eventually level off. I don't know what's caused the reduction.

I certainly could characterize assumptions concerning the fact that the power plants there operating have had no impact to be not very well-founded.

I would consider it to be unfounded.

Q Turning to the answer to the interrogatory which I have put to the staff on hatcheries, it was pointed out this morning by Dr. Stevens that you refer to only part of the evidence, data from the Edenton Lab.

Was there a particular reason why you did refer only to part of the data?

A Yes, the methodology that was employed was to determine a recruit per pound of female stock that the fishery itself -- or the commercial operation -- hatchery operation was exploiting in producing its potential for stocking its juvenile fish.

To do so required a continuous record of a group of fish from collection through the period they would be stocked. That evidence I used was the most recent which I had available to me. The 35 percent, 35.7 percent number that was not included from that Edenton report represented fish that were imported from the -- I think Moncks Corner. I'd have to check.

I have no data on the stock size, their survival, the spawning fraction, the percent of the eggs which hatched, or the pre-stocking mortality.

CHAIRMAN JENSCH: While there is a pause, is -- has Dr. Stevens left?

MR. TROSTEN: Yes, sir, he has.

CHAIRMAN JENSCH: Very well. Proceed.

BY MR. MACBETH:

Q Do you know of any studies which provide survival rates in national water bodies for striped bass eggs or larvae?

A There are several studies which have been made in which those estimates were derived.

MR. TROSTEN: Mr. Macbeth, are you talking -- would you have the reporter read the question back, please?

(The reporter read the question as requested.)

MR. TROSTEN: Which types of national water bodies are you talking about?

MR. MACBETH: Well, that was an inclusive question. I -- if Dr. Goodyear knows of some, I will break them down by fresh water, estuarine, whatever.

MR. TROSTEN: When you answer, then, Dr. Goodyear, would you identify the types of water bodies you are referring to?

THE WITNESS: Most of this data is for fresh water streams which feed reservoirs. The Kerr Reservoir, in Virginia, being one of them; the Congree and the Waterberry are two other rivers which we have sampled in that manner.

In connection with sampling of striped bass eggs at a power plant, the Vienna Steam Electrical Station, there was an estimate derived concerning the proportion of live eggs in the intake water.

In that case there were -- there -- they were comparing opaque to viable eggs. That also would apply. That would be an estuarine situation.

BY MR. MACBETH:

Q How does the survival reported in those studies compare to that in the hatchery estimations that you have included in table 1?

MR. TROSTEN: Survival from where to where, Mr. Macbeth? I am sorry, I am not following your question. I think it's important that the record be clear on this.

Survival of what and where?

MR. MACBETH: Let me back up. I thought maybe Dr. Goodyear would give us that information in comparative terms.

BY MR. MACBETH:

Q Could you answer that question in terms of comparing it to the various stages of brood stock survival that are reported in your table 1?

MR. TROSTEN: This is survival in a natural body of water relative to survival in the hatchery of brood stock, is that what you are referring to? Is that what you mean?

MR. MACBETH: Yes.

MR. TROSTEN: Okay.

MR. MACBETH: Brood stock and the eggs and larva. I am asking for a comparison with the -- of the natural water bodies to the brood stock in stage survivals that are reported in table 1.

MR. TROSTEN: Table 1 of the Goodyear --

MR. MACBETH: Of the Goodyear testimony, 23 April, "Staff Analysis of Artificial Propogation to Replace Hudson River Fishes Killed by Power Plant Operation."

THE WITNESS: I may -- my comments were directed at the hatching percentage only. The fraction of viable developing eggs which had been collected in these field studies ranged from about -- about 55 percent to 78 percent, the Kerr reservoir information.

The The 55 percent, as I remember it, refers to the data which was available in the samples at the Vienna station.

BY MR. MACBETH:

Q That would correspond to 27 and a half percent of their catch in table 1 of your April 23 testimony?

A Yes.

One other thing. The data under "hatched" include production of large number of eggs by non-viable eggs by females which have been injected with hormones, so that it's biased in terms of the proportion of viable eggs which hatch, potentially viable eggs.

For instance, the effect -- you can see the effect of that factor in the '65 data from Weldon which is the first row. The 70 percent hatching figure is for naturally mature natural ovulated females; and the 55 -- or the point 557 is for hormone injected females.

Q There are a number of items I want to clarify in this testimony of April 23. On page 2, you discuss the effect of placing just those fish taken by the Indian Point plant in the context of the larger number of plants operating on the Hudson. You say thus the hatchery would be required to exploit an annual increased portion of brood stock to maintain a constant production. Obtaining wild brood stock under these circumstances has become more difficult and finally impossible.

Am I correct in understanding that to mean that if only the fish killed by Indian Point were replaced through the hatchery, the fact that the other power plants were steadily reducing the total stock of striped bass in the river would, in the long run, mean that it would be impossible to take from the river enough fish to replace those killed by Indian Point at a level at which they would be killed in the first years of operation?

MR. TROSTEN: Mr. Macbeth, what is the premise for your question? I --

CHAIRMAN JENSCH: Page 2. He is on page 2. He is asking if the meaning of it, if he has interpreted. He is asking the witness if that is what he is conveying.

MR. TROSTEN: I think it would be helpful, Mr. Chairman, if we could be clear about which plants Mr. Macbeth is talking about so that we have a clear statement of what the premise of the question is.

MR. MACBETH: Well, the plants I had in mind were

Lovett, Danskammera, Roseton, and Indian Point One and Two. I refer to Indian Point.

CHAIRMAN JENSCH: Do you have the question in mind?

THE WITNESS: Not any more.

CHAIRMAN JENSCH: Would you read the question please, Mr. Reporter?

(Whereupon, the pending question was read as requested.)

THE WITNESS: Yes. Do you

BY MR. MACBETH:

Q Further on, page two, last full paragraph, it is stated, "Another potentially important factor is that the hatchery must replace both the power plant losses and the reduced natural production due to the level of productive stock by the hatchery." Am I correct in taking that to mean that in estimating the number of fingerlings that must be placed in the Hudson, one must take account not only of the number of fish which would be killed by the plant, but also the fact that brood stock would itself be producing a certain number of fingerlings which haven't been removed to the hatchery, and yet will no longer produce?

A Could I have that again?

CHAIRMAN JENSCH: Would the reporter read it?

(Whereupon, the pending question was read as requested.)

MR. MACBETH: I think you got most of it except the very end.

BY MR. MACBETH:

Q You have to take into account that the brood stock would itself have produced a certain number of fingerlings which it will no longer produce, having been removed from the river into the hatchery, and that to bring the river back to what it -- what it would have been without the power plants operation --

CHAIRMAN JENSCH: Would you restate the whole question or is it too difficult?

MR. MACBETH: I'll try to restate the whole question.

BY MR. MACBETH:

Q Does this statement at the beginning of the last full paragraph indicate that in order to maintain the level of fingerlings in the river which would have been there but for the operation of the power plants, one must not only take into account the number of fish which would be directly killed by the plants but also the fact that the brood stock removed from the river to the hatchery to replace the power plant kills would itself have been productive of a certain number of fingerlings in the river and that those, too, must be replaced?

A Yes.

21

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Q Am I correct in concluding from your testimony that it is your opinion that hatchery operation would not be as efficient as the natural system in producing juvenile fish in the Hudson River?

A That would appear to be the case. The gauge that is derived in the hatchery occurs during the early stages of development in, for instance, phase one as it is tallied in the testimony.

However, one loses from mortalities of the stock which are collected and the loss of potential annual production by hatching the brood stock that results in their not spawning, and the premature ovulation of eggs which results in non-vibrant material, those factors combine to offset the increased survival one would get in the early stages of rearing; and it is principally the cumulative effects of those two factors together that give you the efficiency difference between hatchery and natural production.

Q Is it true that if the hatchery is less efficient in producing fingerlings than the natural river, that removing the fish into the hatchery can never succeed in producing as many fish as would naturally have been in the river?

A Yes.

Q On the other hand, leaving the fish in the river and allowing a certain percentage of them to be killed by the power plant could produce a lower total number of fish,

ar2

of fingerling size, so that the best course of action, if a hatchery were to be employed in the situation, would be to remove all the fish from the river at the time of spawning and return them as fingerlings?

A No, I don't believe that would be an accurate statement. I think the most reasonable way to replace through a hatchery the fish which would be killed would be by utilizing a domestic brood stock which would be raised strictly for egg production.

CHAIRMAN JENSCH: Is this a convenient place to interrupt your examination?

MR. MACBETH: It is, Mr. Chairman.

CHAIRMAN JENSCH: What time is it convenient to resume in the morning? 9:00 o'clock?

MR. TROSTEN: Mr. Chairman, we could resume tomorrow morning at 9:00 o'clock. I have two very brief matters that I would like to get out of the way before we adjourn tonight, if it would be satisfactory. It will take a very short period of time.

CHAIRMAN JENSCH: Well, I want to accommodate you if we can. Sometimes brevity gets a little overly exaggerated. Let's take a shot at it.

What are the two subjects?

MR. TROSTEN: The two subjects are, first, we have prepared a response to Mr. Briggs' question which he

ar3

asked at transcript page 11038 having to do with WCAP-7982, and I would like to now offer this response pertaining to peaking factors in evidence, Mr. Chairman, on behalf of the Applicant under the sponsorship of Messrs. Uram, Boman and Rowe.

CHAIRMAN JENSCH: What's the other item?

MR. TROSTEN: The other item is a very brief piece of testimony, which I will have Mr. Woodbury offer orally, pertaining to Mr. Clark's testimony.

CHAIRMAN JENSCH: Well, is there any reason the latter can't go until morning?

MR. TROSTEN: The reason basically I would like to do it this evening, Mr. Chairman, is because Mr. Clark testified with regard to work done by Mr. Bibko who was working in association with Dr. Lauer on the study of the pressure effects on striped bass.

Mr. Woodbury is prepared to testify with respect to this and Dr. Lauer, who happens to be present at this moment, would also be available in case there are any questions that the Board had or Mr. Macbeth had and that's why I wanted to get it out.

CHAIRMAN JENSCH: That's the brevity problem. The competence of Mr. Woodbury has been displayed in the proceeding. It is such that I am sure he can handle all the questions that might arise in connection.

ar4

It is getting on towards 6:00 o'clock, and we have kind of had it. The lights are on, I know. Let's take your WCAP.

Any objection to having this statement as reflected in a two-page -- three-page typewritten document physically incorporated into the record as if read?

MR. KARMAN: No objection.

MR. MACBETH: No objection.

CHAIRMAN JENSCH: Very well. The request is granted and the three-page document entitled "Applicant's Response to Board Question, Indian Point 2, April 25, 1973" may be physically incorporated within the transcript as if orally given and shall constitute evidence on behalf of the Applicant.

(The document follows.)

Applicant's Response to Board Question
Indian Point 2 - April 25, 1973

QUESTION (Mr. Briggs, Tr. 11,038)

"The question is the following: In WCAP-7982, Figure 5.5 and the discussion that goes along with that figure, seems to indicate that axial offset measurements would not provide adequate assurance of the LOCA limits not being exceeded for a peaking factor of 2.7 and power exceeding 85 percent of fuel power early in life.

We would like to know how the curves that are shown in that report, and the values shown on the curves, relate to the Indian Point plant and the conditions in the core of the Indian Point plant."

ANSWER

The discussion in WCAP-7982 is illustrative of the effects of fuel densification on plant power capability. Figure 5.5 is illustrative of the effects of burn-up on allowable operating power as a function of F_Q and is not intended to be typical of Indian Point Unit No. 2 but rather, is intended to be typical of plants in which the allowable operating power is limited to less than 100% at beginning of life. As shown in the report "Fuel Densification -- Indian Point Nuclear Generating Station Unit No. 2," allowable operating power for Indian Point Unit No. 2 is not limited to less than 100% at the beginning of life. The example presented in Figure 5.5 of WCAP-7982, including the discussion associated with Figure 5.5, is therefore not applicable to Indian Point Unit No. 2 because Indian Point Unit No. 2 has a different core and fuel rod design and different characteristics pertinent to the analysis of the loss-of-coolant accident.

Adequate assurance of the LOCA limit not being exceeded for

Indian Point Unit No. 2 is provided by showing that the maximum value of the peaking factor for modes of operation permitted by the Technical Specifications for the plant do not exceed the maximum allowable peaking factor permitted by LOCA considerations. A discussion in comparison of these two items follows for the example plant of WCAP-7982 and Indian Point Unit No. 2.

(1) Maximum Peaking Factor -

Figure 4.2 of "Fuel Densification -- Indian Point Nuclear Generating Station Unit No. 2" shows that a total peaking factor, F_Q, no greater than 2.70 can be maintained whereas Figure 5.6 of WCAP-7982 shows that for the example plant an F_Q no greater than 2.75 can be maintained.

(2) Allowable Peaking Factor for LOCA -

Figure 5.1 of WCAP-7982 shows the maximum allowable linear power for the example plant with a calculated clad temperature of 2300^{o}F in the event of the LOCA, to be 14.35 KW/ft at beginning of life. At 100% power this corresponds to a peaking factor of 2.28 which is less than the maximum operating value of 2.75, thus requiring a power level less than 100%.

A comparable allowable linear power at beginning of life (BOL) for Indian Point Unit No. 2 is 17.35 KW/ft as shown in Figure 5.1 of the Indian Point Unit No. 2 Densification Report. At 100% power, this corresponds to an allowable peaking factor of 2.90, which is in excess of the maximum operating value of 2.70. Thus no restriction on beginning of life power level is required and the improvement in power capability with reduced peaking factors and time illustrated by Figure 5.5 in WCAP-7982 is not important.

ar5

CHAIRMAN JENSCH: At this time let's recess to reconvene in this room tomorrow morning at 9:00 o'clock.

(Whereupon, at 5:32 p.m., the hearing was adjourned, to reconvene at 9:00 a.m., Thursday, 26 April 1973.)

e22

ADDENDUM TO

FUEL DENSIFICATION - INDIAN POINT NUCLEAR GENERATING

STATION UNIT NO. 2

March 22, 1973

Addendum to "Fuel Densification - Indian Point Nuclear Generating Station Unit No. 2" (January, 1973)

The changes indicated below have been made to the subject report.

1. Change to Region 1 Fuel Assemblies

Section 3.1.1.1 of the Fuel Densification Report for Indian Point Unit 2 states that 9 of the 65 Region 1 assemblies have been designed for two cycles of operation by use of the shorter plate-type bottom nozzle. During fuel refabrication, an additional 5 Region 1 assemblies were modified and now use this same plate-type bottom nozzle. Thus, Region 1 now contains 14 assemblies designed for two cycles of operation.

2. Change in Total Heat Flux Peaking Factor F_Q, vs Axial Offset

Since the issuance of the Indian Point Unit No. 2 Fuel Densification Report, the F_Q vs. axial offset correlation, which was presented in Figure 4.2, has been re-evaluated on a basis consistent with Attachment 3 of Westinghouse letter NS-SL-524 to D. Knuth from R. Salvatori, dated January 2, 1973. The methods employed for the results presented below are the same as in the original IPP 2 analysis, but with more control rod maneuvers and resultant transients considered. The same margins for uncertainty and manufacturing tolerances are included as in the previous analysis. As in the previous analysis, the axial power peaking factor and a horizontal peaking factor, F_{xy}, at the plane of the peak local power no lower than 1.44 were also incorporated. This new correlation is shown in the revised Figure 4.2 and necessitates the following changes in operating procedures to assure conformity to the assumptions of the revised analyses.

a. The control rod insertion limits must be revised as indicated below in the revision for Section 7.0. This is due to the fact that unacceptable values of F_Q result from some transients considered with control rods at their previous full power insertion limit.

b. The change in the shape of the F_Q envelope necessitates a modification to the reactor trip settings. This has also been incorporated in a change for Section 7.

The re-evaluation indicates that a total peaking factor, F_Q, of 2.70 is still maintained by reference only to the ex-core detector axial offset.

3. Changes to the Technical Specification

Changes to the Technical Specifications are contained on the revised pages 7-2, 7-3, 7-4 and 7-6 and the revised Figures 3.10-1 and 3.10-2.

1. For $(q_t - q_b)$ within the range between ΔI_1 and ΔI_2 given in the table below, $f(\Delta I) = 0$ (where q_t and q_b are percent power in the top and bottom halves of the core respectively, and $q_t + q_b$ is total core power in percent of rated power.)

2. For each percent that $(q_t - q_b)$ is less than ΔI_1, the Delta-T trip set point shall be automatically reduced by 4.5% of its value at rated power. For each percent that $(q_t - q_b)$ is greater than ΔI_2, the Delta-T trip set point shall be automatically reduced by 2% of its value at rated power.

ΔI_1 and ΔI_2 are linear functions of the gain K_4. The proper limits on ΔI_1 and ΔI_2 shall be obtained from the following table which gives the allowable values corresponding to the actual value of K_4.

K_4	ΔI_1	ΔI_2
$\leq$ 1.01	$\geq$ -16.0	$\leq$ +16
1.04	$\geq$ -15.33	$\leq$ +14.5
1.07	$\geq$ -14.66	$\leq$ +13
1.10	$\geq$ -14.0	$\leq$ +11.5
1.13	$\geq$ -13.33	$\leq$ +10
1.16	$\geq$ -12.66	$\leq$ +8,5
1.19	$\geq$ -12	$\leq$ +7

Basis for Revision:

The $f(\Delta I)$ function in overpower and overtemperature protection system setpoints have been revised to include effects of fuel densification on core safety limits. The revised setpoints as given above will ensure that the safety limit of centerline fuel melt will not be reached and DNBR of 1.30 will not be violated.

Section 3.10 CONTROL ROD AND POWER DISTRIBUTION LIMITS

Specification:

The referenced portion of the previous specification is noted in parenthesis.

(3.10.1) Control Rod Insertion Limits

(3.10.1.5) The part length rods shall not be more than 70% inserted.

(3.10.2) Power Distribution Limits and Misaligned Control Rod

(3.10.2.1) (Change 50% to 75%)

(3.10.2.2-b) The hot channel factors shall be determined and maximum allowable power shall be reduced one percent for each percent the hot channel factors exceed the design values of:

$F_Q^N \leq 2.62\ [1 + 0.2(1-P)]$ in the indicated flux difference range of +7 to -12 percent

$F_{\Delta H}^N \leq 1.65\ [1 + 0.2(1-P)]$

where P is the fraction of full power at which the core is operating.

For every percent outside of the indicated flux difference range +7 to -12 percent, the allowed F_Q^N may be increased above 2.62 by 2 percent in the positive range and by 4.5 percent in the negative range.

The measured values, with due allowance for measurement error, must be corrected by including a penalty as shown on Figure 3.10-4 (at the approximate core location) to account for fuel densification effects before comparison with the limiting values above.

(3.10.2.6) Except during physics tests, the following power distribution restrictions must be maintained:

a. At rated power, the indicated axial flux difference must be maintained within +7 percent and -12 percent.

b. If, at rated power, the indicated axial flux difference exceeds the permissible range defined above for a period of more than eight hours, the situation shall be corrected or the reactor power shall be reduced 2 percent for each percent the flux difference exceeds the permissible positive range and reduced 4.5% for each percent in the negative range.

c. For every 2 percent below full power, the permissible flux difference range is extended by 1 percent in the positive range and 0.44 percent in the negative range.

Basis for Revision:

Part length rod insertion has been limited to eliminate certain adverse power shapes.

Two criteria have been chosen as a design basis for fuel performance related to fission gas release, pellet temperature and cladding mechanical properties. First the peak value of linear power density must not exceed 21.1 kw/ft. Second, the minimum DNBR in the core must not be less than 1.30 in normal operation or in short term transients.

In addition to the above, the initial steady state conditions for the peak linear power for a loss of coolant accident must not exceed the values assumed in the accident evaluation. This limit is required in order for the maximum clad temperature to remain below that established by the Interim Policy Statement for LOCA. To aid in specifying the limits on power distribution the following hot channel factors are defined.

F_Q, Heat Flux Hot Channel Factor, is defined as the maximum local heat flux on the surface of a fuel rod divided by the average fuel rod heat flux, allowing for manufacturing tolerances on fuel pellets and rods.

3. The control bank insertion limits are not violated.

4. Axial power distribution guide lines, which are given in terms of flux difference control, are observed. Flux difference refers to the difference in signals between the top and bottom halves of two-section excore neutron detectors. The flux difference is a measure of the axial offset which is defined as the difference in power between the top and bottom halves of the core. Calculation of core average axial peaking factors have been correlated with axial offset. The correlation shows that an F_Q^N of 2.62 and allowed DNB shapes, including the effects of fuel densification, are not exceeded if the axial offset is maintained between -15 and +10 percent.

For operation at the fraction, P, of full power the design limits are met, provided,

$F_Q^N \leq 2.62 \, [1 + 0.2 \, (1-P)]$ in the indicated flux difference range range of +7 to -12 percent.

and

$F_{\Delta H}^N \leq 1.65 \, [1 + .2 \, (1-P)]$

For every percent outside of the indicated flux difference range +7 to -12 percent, the allowed F_Q^N may be increased above 2.62 by 2 percent in the positive range and by 4.5 percent in the negative range.

The permitted relaxation of F_Q^N and $F_{\Delta H}^N$ allows radial power shape changes with rod insertion to the insertion limits. The allowed increase in F_Q^N for large flux differences is consistent with power shapes assumed in setting the overpower and overtemperature ΔT setpoints. It has been determined that provided the above conditions 1 through 4 are observed, these hot channel factors limits are met.

For normal operation and anticipated transients the core is protected from exceeding 21.1 KW/ft locally, and from going below a minimum DNBR of 1.30, by automatic protection on power, flux difference, pressure and temperature. Only condition 1 through 3, above, are mandatory since the flux difference is an explicit input to the protection system.

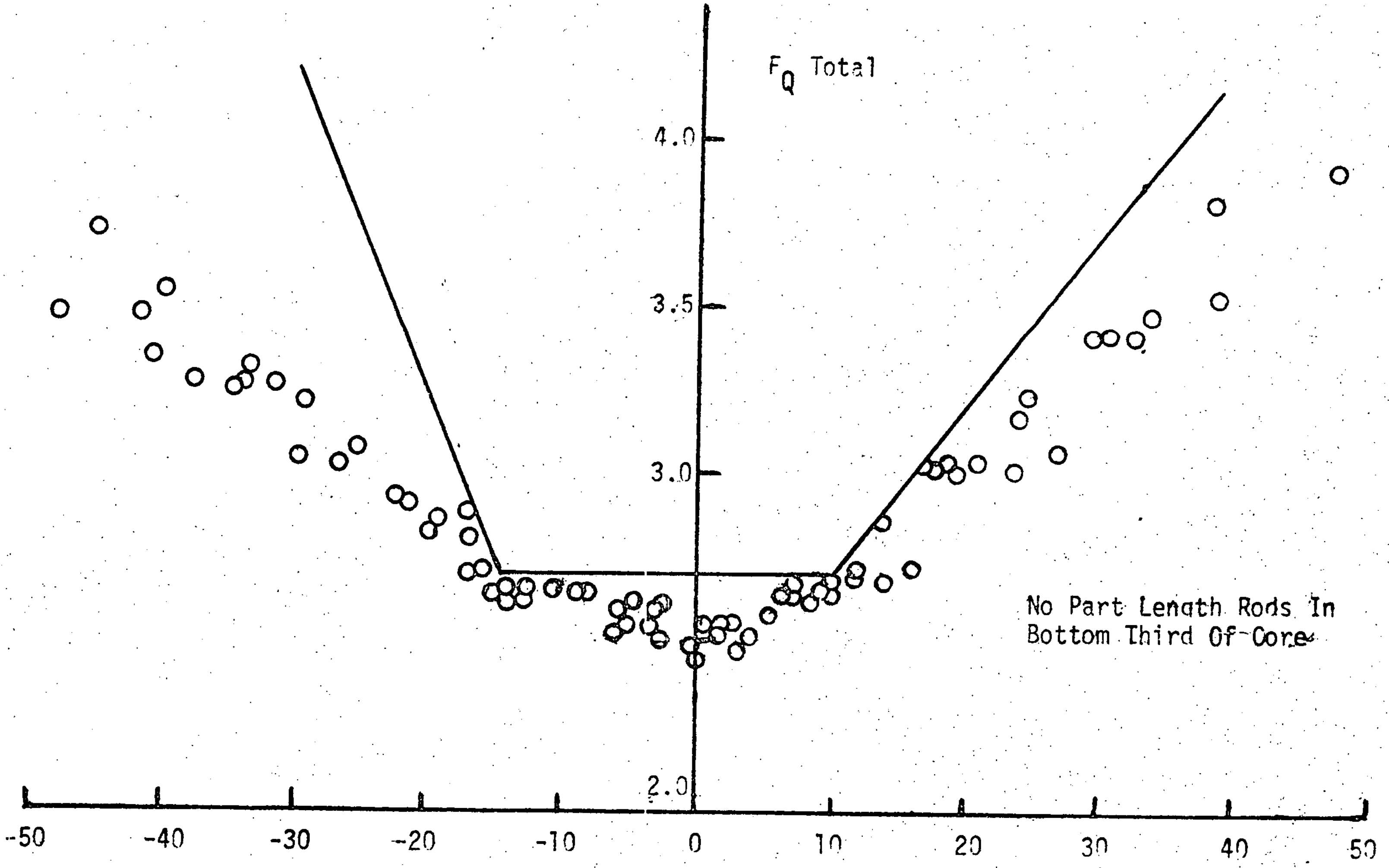

FIGURE 4.2 TOTAL HEAT FLUX PEAKING FACTOR - F_Q VS AXIAL OFFSET

Revised 3-22-73

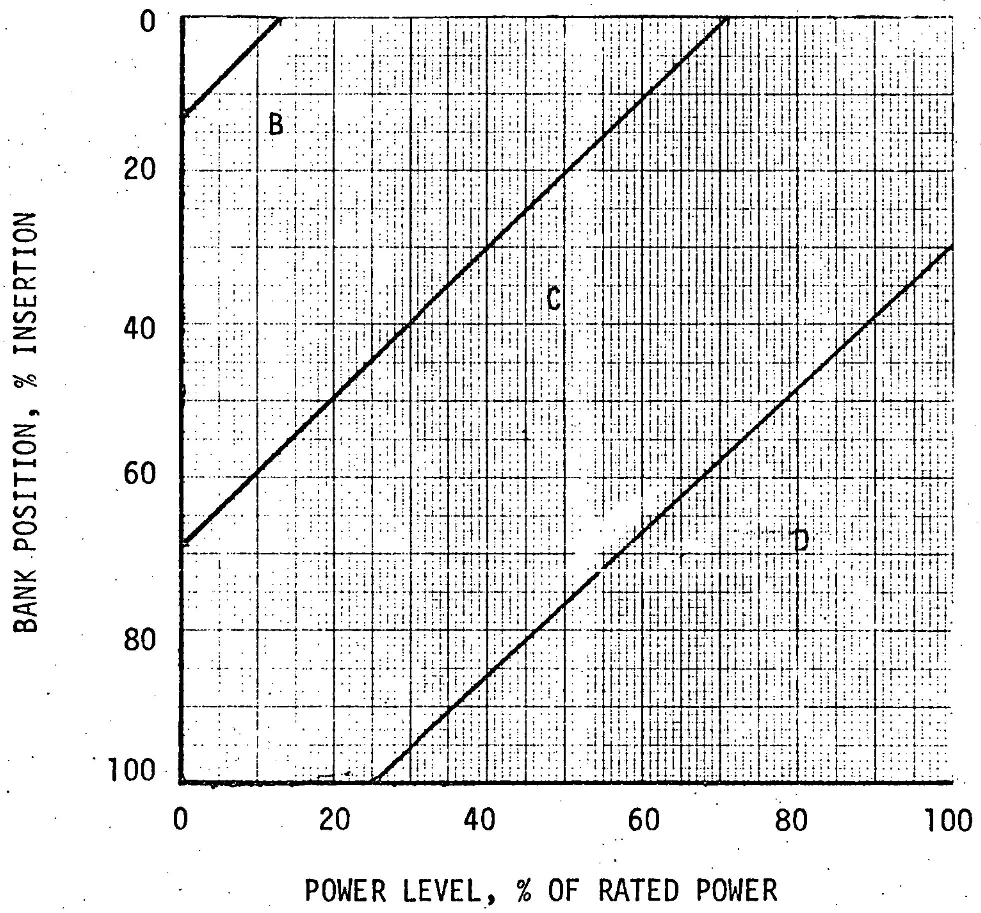

FIGURE 3.10-1 CONTROL BANK INSERTION LIMITS FOR 4 LOOP OPERATION

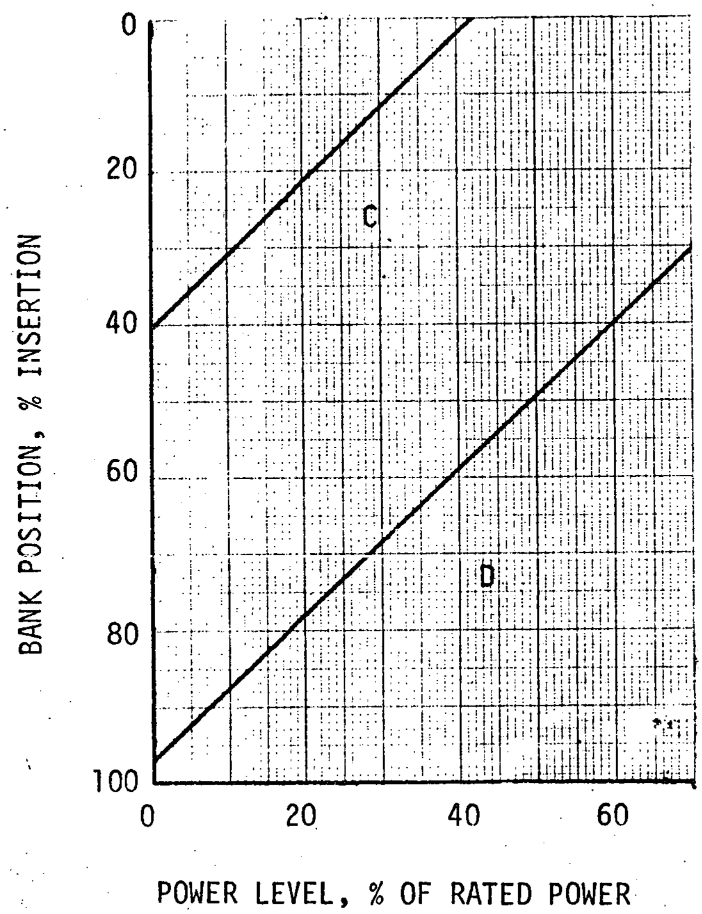

FIGURE 3.10-2 CONTROL BANK INSERTION LIMITS
FOR 3 LOOP OPERATION

FUEL DENSIFICATION - INDIAN POINT NUCLEAR GENERATING STATION UNIT NO. 2

JANUARY, 1973

Material that is proprietary to the Westinghouse Electric Corporation has been deleted from this document.

TABLE OF CONTENTS

TABLE OF CONTENTS (Continued)

LIST OF TABLES

LIST OF FIGURES

LIST OF FIGURES (Continued)

1.0 INTRODUCTION AND SUMMARY

During the latter stages of construction of the Indian Point Unit No. 2 nuclear plant, the phenomenon of in-pile densification of UO_2 fuel was identified as a matter of concern in light water reactors. This report contains an evaluation of fuel densification as it relates to Indian Point Unit No. 2, and establishes that full rated power operation can be achieved, and presents proposed changes to the Technical Specifications consistent with that evaluation.

Densification of UO_2 fuel has been observed to occur under irradiation in several operating reactors. This densification causes pellets to shrink both axially and radially. The pellet shrinkage combined with random hang-up of fuel pellets results in gaps in the fuel column when the pellets below the hung-up pellet settle in the fuel rod. These gaps vary in length and location in the fuel rod. Because of increased moderation and decreased neutron absorptions in the vicinity of the gap, power peaking occurs in the adjacent fuel rods. The initial radial shrinkage of the pellet increases the clad-pellet radial gap and results in higher fuel temperatures. As months of operation are accumulated however, the clad creeps inward and fuel temperature decreases.

The major significance of these factors, generally, are the following:

a) The axial shrinkage of the pellet combined with power peaking due to gaps increases the power peaking factor. In Addition, the initially larger radial pellet-clad gap produces a higher fuel centerline temperature for a constant average linear power density. Thus, the limiting linear power density must be compatible with operation at full rated power or power penalty restrictions must be imposed.

b) The increased local power density and higher average fuel temperature must be taken into account in the analysis of the loss-of-coolant accident to determine whether the fuel cladding temperature associated with full rated power operation can be maintained below 2300°F in the event of a LOCA.

To eliminate such penalties associated with fuel densification in Indian Point 2, it was decided to modify the design of the fuel. Thus, the original unpressurized fuel was returned from the reactor site to the factory to perform the modifications. Basically, these modifications consisted of (1) prepressurization of the fuel rods, (2) replacing the fuel pellets in Regions 2 and 3 with those of higher density (95% theoretical) and U-235 enrichment, and (3) increasing the number of burnable poison rods. Other changes in the core design are described in Section 3.

The determination of reactor power capability with the modified fuel was made following the methods described in WCAP-7984[1], "Fuel Densification Penalty Model," as modified according to the AEC fuel gap size[2] distribution. The conclusions presented in Section 5 are based on the premise that the fuel cladding in the regions of the fuel column gaps will not flatten during operation as justified in Section 2. The evaluation considered the fuel performance limits described in Section 4.

In Section 6, all of the accidents analyzed and reported in the FSAR[3] which could potentially be affected by fuel densification have been reviewed. The results for those requiring reanalysis and the justification of the applicability of previous results for the remainder are presented.

The Technical Specifications requiring change for the operations described have been reviewed and modified Technical Specifications have been proposed in Section 7.

It has been concluded that the operations of Indian Point Unit No. 2 with the modified core described herein can be carried out in conformity with the rules and regulations of the Atomic Energy Commission in a manner that provides reasonable assurance that such operation will not endanger the health and safety of the public.

References for Section 1

1. Eng, G., et al., "Fuel Densification Penalty Model," WCAP-7984, October, 1972.

2. "Technical Report on Densification of Light Water Reactor Fuels," Regulatory Staff U. S. A. E. C., November 14, 1972.

3. "Final Facility Description and Safety Analysis Report - Indian Point Nuclear Generating Station Unit No. 2," Docket No. 50-247.

2.0 CLAD FLATTENING ANALYSIS

Using current Westinghouse analytical techniques[1] and operating conditions appropriate for the "worst-case" lead burnup rods in each region of Indian Point 2, the following minimum time to clad flattening is predicted.

	Minimum time to clad flattening
Region 1	Greater than 21,000 EFPH* (nominal burnup for two cycles)
Region 2	Greater than 29,000 EFPH* (nominal burnup for three cycles)
Region 3	Greater than three cycles

*EFPH = Effective Full Power Hours, integrated flux equivalent to operating at 100% power at stated time.

No clad flattening is predicted in Indian Point 2 since the best estimates for cycles 2 and 3 burnups are less than the conservative, minimum calculated time for initial clad flattening.

Recent data from Point Beach No. 1 indicate that the analysis is conservative. Whereas the analysis predicted clad flattening in Regions 2 and 3 after [ ] EFPH, clad flattening was not observed after 13,000 EFPH, the end of the first cycle.

References for Section 2

1. Eng, G., et al., "Fuel Densification Penalty Model," WCAP-7984, October, 1972.

3.0 REACTOR

3.1 REACTOR DESIGN

A description of the Indian Point Unit No. 2 reactor core can be found in the FSAR.(1) The reactor core is a three-region cycled core with a rated power of 2758 MW thermal. It contains 193 fuel assemblies each containing 204 fuel rods. The fuel rods have a nominal active length of 12 ft. and contain enriched UO_2. The fuel rod cladding is Zircaloy-4, with a nominal O.D. of 0.422 in. and 0.0243 in. wall thickness. The UO_2 pellet is approximately 0.365 in. O.D. by 0.6 in. high with dished ends.

Due to fuel rod Zircaloy growth and fuel densification considerations, the fuel rods, fuel assembly and core loading pattern have been modified for the replacement core, as described in the following sections.

3.1.1 MECHANICAL DESIGN AND EVALUATION

This section supplements and modifies the corresponding Section 3.2.3 in the Indian Point Unit No. 2 FSAR.(1) The original Regions 2 and 3 unpressurized fuel rods have been replaced by new fuel rods which are prepressurized with helium to [ ] and which contain higher initial density (95% T.D.) fuel pellets. This change was made to minimize the potential effects due to fuel densification. The average fuel enrichments of Regions 2 and 3 have been increased by 0.1% to 2.8 and 3.3 w/o respectively. The increased fuel density and enrichment in the replacement core requires an increase in the number of burnable poison rods from 1160 to 1412 in order to avoid a positive moderator coefficient at BOL. The location of the fuel assemblies and the burnable poison rods in the core are described in the Nuclear Design Section. The Region 1 fuel has been modified by pressurizing with helium to [ ] and modifying the fuel rods and assemblies in order to accommodate a greater Zircaloy clad growth rate. Changes in the core mechanical design parameters are given in Table 3.1. Additional modifications to the fuel assemblies and an evaluation of the design changes are given in the following core component sections.

3.1.1.1 Fuel Assembly Dimensional Changes

In order to accommodate a higher Zircaloy irradiation growth than was originally anticipated, the fuel assembly dimensions described in the FSAR Figure 3.2.3-9 have been modified to three types of assemblies with dimensional differences. Region 1 has 56 fuel assemblies with the fuel rod lengths reduced to 149.42 inches by using shorter bottom end plugs. The remaining 9 Region 1 fuel assemblies have unchanged fuel rod length, but shorter plate-type bottom nozzles are used. The plate-type nozzles are described in Section 3.1.1.2. Their reference length is 2.738 inches instead of the reference 3.188 inches for the bar-type bottom nozzles. This provides 0.45 inches of additional gap clearance between the fuel rods and nozzles. The new Region 2 and 3 fuel assemblies have fuel rod lengths of 149.37 inches and use the plate-type bottom nozzles. As in Figure 3.2.3-9, Region 1 retains the 144 inch fuel column length, but Regions 2 and 3 have a fuel length of 141.7 inches. The interface of the fuel rods with the plate-type bottom nozzle is shown in Figure 3.1.

As a result of these fuel assembly changes, 56 Region 1 fuel assemblies have a BOL cold fuel rod-nozzle gap of approximately [.] inches. As burnup accumulates the cold shutdown gap decreases due to irradiation growth of the Zircaloy-clad fuel rods while the stainless-steel thimbles do not grow due to irradiation. Using a conservative design growth rate for irradiated Zircaloy clad fuel rod data (includes upper 2 sigma bound of growth data), analysis of fuel rod growth shows that rod-to-nozzle cold interference (zero gap) is not expected to occur during nominal Cycle 1 exposure, approximately 13,000 effective-full/power-hours (EFPH).

Nine of the 65 Region 1 assemblies have been designed for two cycles of operation by use of the shorter plate-type bottom nozzle. Using the con-servative design growth rate for Zircaloy cladding, fuel rod-to-adaptor plate cold interference is not expected at the nominal EO cycle 2 (21,000 EFPH), due to a larger initial rod-nozzle gap than the other 56 Region 1 assemblies.

Region 2 and 3 fuel assemblies have a BOL cold rod-nozzle gap greater than all the Region 1 assemblies. Using the conservative rod growth design equation, approximately 2 rods per assembly are predicted to be in interference at end of cycle 3 cold shutdown (nominally 29,000 EFPH). In such a case the rods would exert small, acceptable forces on the thimble-nozzle joints.

3.1.1.2 Bottom Nozzle Changes

As stated in the preceeding section, 9 Region 1 and all of the Regions 2 and 3 fuel assemblies use the plate-type bottom nozzles instead of the bar-type described in Section 3.2.3 of the FSAR.*

The bottom nozzle is a box-like structure which serves as a bottom structural element of the fuel assembly and controls the coolant flow distribution to the assembly. The square nozzle is fabricated from type 304 stainless steel and consists of a slotted plate and four angle legs with bearing plates as shown in Figure 3.1. The legs form a plenum for the inlet coolant flow to the fuel assembly.

Coolant flow through the fuel assembly is directed from the plenum in the bottom nozzle upward through the penetrations in the plate to the channels between the fuel rods. The penetrations in the plate are positioned between the rows of the fuel rods.

Axial loads (holddown) imposed on the fuel assembly and the weight of the fuel assembly are transmitted through the bottom nozzle to the lower core plate. Indexing and positioning of the fuel assembly is controlled by alignment holes in two diagonally opposite bearing plate which mate with locating pins in the lower core plate. Any lateral loads on the fuel assembly are transmitted to the lower core plate through the locating pins.

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*These changes were described previously in a letter dated October 8, 1971, from Consolidated Edison to the AEC. The AEC accepted the new plate-type nozzles as an "equivalent design" in a letter to Consolidated Edison dated February 25, 1972.

3.1.1.3 Fuel Rod Modifications

The description and physical parameters of the fuel rods are given in Section 3.2.3 of the FSAR, except as modified in this section.

Due to Zircaloy growth considerations, the length of most of the fuel rods (all except 9 Region 1 assemblies) have been decreased, as described in Section 3.1.1.1 and shown in Table 3.1. The fuel rod diameter and cladding thickness are unchanged.

To reduce the effects of fuel densification, the fuel pellets in Regions 2 and 3 have been increased to a nominal average density of 95% of theoretical. The Region 2 and 3 pellet diameters have been reduced 1 mil and the pellet-clad diametral gap increased from 6.5 to 7.5 mils. The Region 1 fuel retains its original 94% T.D. and fuel dimensions. The earlier selection of lower fuel densities for Regions 2 and 3 was based upon a conservative interpretation of fuel swelling data. Re-interpretation of this data, as well as new data, indicates that swelling is not as strong a function of density as expected during the three fuel cycles. The higher fuel densities for Regions 2 and 3 for the replacement core will minimize the potential adverse effects of fuel densification which are discussed in Reference 2.

Fuel densification results in the formation of axial fuel column gaps if pellet(s) hangup prevent fuel stack settling. The revised higher fuel densities will minimize the length of such gaps, should they occur. This in combination with Helium prepressurization of all fuel regions assures that clad flattening into potential fuel gaps will not occur during the planned fuel lifetime in the core. The conservative estimates on clad flattening for each fuel region are given in Section 2. The Helium pressurization and fuel density changes necessitates fuel rod design changes in order to satisfy the design basis and criteria stated in Section 3.1 of the FSAR. The fuel rod plenums are sized to assure during normal operation and anticipated transients that the internal rod pressure for the core life of the fuel is less than the nominal 2250 psia coolant pressure. Factors considered in this sizing are the initial Helium pressure, fuel densification, lead burnup and maximum power rods with fuel shuffling for succeeding cycles, and the effects of design model and manufacturing tolerance uncertainties. The Regions 2 and 3 plenums are increased, which assures the rod internal pressure does not exceed 2250 psia for a "worst-case" analyses using model and tolerance uncertainties. As a result of the requirement for an increased plenum length, the Region 2 and 3 fabricated fuel stacks are reduced from the original 144 to 141.7 inches. The Region 1 fuel stack dimensions are unchanged, since the Region 1 plenum length is sufficient to prevent exceeding 2250 psia rod pressure during cycle 1 for the worst-case analyses. If used during all of cycle 2, it is predicted that Region 1 fuel rods would not exceed 2250 psia internal pressure based on a best-estimate analysis (excluding model and tolerance uncertainties).

3.1.2 NUCLEAR DESIGN

Major design changes that affect the nuclear characteristics of IPP-2 are the enrichment, density and initial pressurization. These parameters are shown in Table 3.2. The regionwise fuel loading pattern remains unchanged and is shown in Figure 3.2. In order to accommodate the higher enrichments and to meet the power peaking design requirements the number of burnable poison rods had to be increased from 1160 to 1412 and the pattern revised. Figure 3.3 illustrates the revised poison rod pattern by assembly.

In addition the nuclear enthalpy rise hot channel factor, $F^N_{\Delta H}$, has been reduced to 1.65 and the total heat flux design factor has been reduced to 2.70, which includes the engineering factor, F^E_q, of 1.03. The original $F^N_{\Delta H}$ was conservative and has been reduced in line with current technology. The revised F^{tot}_q has been derived by considering all allowable operating situations and the effects of densification. These factors along with other nuclear design data are tabulated in Table 3.3. Tables 3.4 and 3.5 are also enclosed which show control rod reactivity requirements and worths.

References for Section 3

1. "Final Facility Description and Safety Analysis Report - Indian Point Nuclear Generating Unit No. 2," Docket No. 50-247.

2. Eng, G., et al., "Fuel Densification Penalty Model," WCAP-7984, October, 1972.

TABLE 3.1

CORE MECHANICAL DESIGN PARAMETERS[1]

Active Portion of the Core	
Equivalent Diameter, in.	132.7
Active Fuel Height, in.	144.0 R1, 141.7 R2/R3[2]
Length-to-Diameter Ratio	1.09
Total Cross-Section Area, Ft^2	96.06
Fuel Assemblies	
Number	193
Rod Array	15 x 15
Rods per Assembly	204
Rod Pitch, in.	0.563
Overall Dimensions	8.426 x 8.426
Fuel Weight, (as UO_2), pounds	217,800
Total Weight, pounds	273,000
Number of Grids per Assembly	9
Number of Guide Thimbles	20
Diameter of Guide Thimbles (upper part), in.	0.545 O.D. x 0.515 I.D.
Diameter of Guide Thimbles (lower part), in.	0.484 O.D. x 0.454 I.D.
Fuel Rods	
Number	39,372
Outside Diameter, in.	0.422
Diametral Gap, in.	0.0065 R1; 0.0075 R2/R3
Clad Thickness, in.	0.0243
Clad Material	Zircaloy
Region 1 Overall Length, in.	149.6/149.4 (9/56 F. Ass.)[3]
Region 1 Length of Bottom End Cap, overall, in.	0.688/0.488 (9/56 F. Ass.)
Region 1 Length of Top End Cap, overall, in.	0.688
Region 1 Length of Top and Bottom Caps, inserted in rod, in.	0.250
Region 2 & 3 Overall Length, in.	149.4
Region 2 & 3 Length of Top and Bottom End Caps, overall, in.	.688/.502 (Top/Bottom)
Region 2 & 3 Length of End Cap, inserted, in.	.250
Fuel Pellets	
Material	UO_2 sintered
Density (% of Theoretical)	
Region 1	94 (10.3 g/cc)
Region 2	95 (10.4 g/cc)
Region 3	95 (10.4 g/cc)
Feed Enrichments w/o	
Region 1	2.2
Region 2	2.8
Region 3	3.3
Diameter, in.	0.3669 R1, 0.3659 R2/R3
Length, in.	0.600

TABLE 3.1 (Cont'd)

Rod Cluster Control Assemblies

Neutron absorber	5% Cd, 15% In, 80% Ag
Cladding Material	Type 304 SS - Cold Worked
Clad Thickness, in.	0.019
Number of Clusters	
Full Length	53
Part Length	8
Number of Control Rods per Cluster	20
Length of Rod Control, in.	156.436 (overall) 149.136 (insertion length)
Length of Absorber Section, in.	142.00 (full length) 36.00 (part length)

Core Structure

Core Barrel, in.	
I.D.	148.0
O.D.	152.5
Thermal Shield, in.	
I.D.	158.5
O.D.	164.0

Burnable Poison Rods

Number	1412
Material	Borosilicate Glass
Outside Diameter, in.	0.4395
Inner Tube, O.D., in.	0.2365
Clad Material	S.S.
Inner Tube Material	S.S.
Boron Loading (natural) gm/cm of glass rod	0.0429

(1) All dimensions are for cold conditions.

(2) R1 = Region 1; R2/R3 = Regions 2 and 3.

(3) 9/56 F. Ass.: First tabulated valve is for 9 fuel assemblies, second tabulated valve is for 56 fuel assemblies in Region 1.

TABLE 3.2

FUEL ASSEMBLY DESIGN PARAMETERS

Region	1	2	3
Enrichment	2.2	2.8	3.3
Geometric density (% theoretical) - as built region average	93.6	*	*
Initial Helium pressurization (psia)	[		]

* As built values not available for Regions 2 and 3 at time of analysis; therefore, 94.3% assumed for this evaluation of densification effect for the specified design value of 95%.

TABLE 3.3

NUCLEAR DESIGN DATA

STRUCTURAL CHARACTERISTICS

1.	Fuel Weight (UO_2), lbs.	217,800
2.	Zircaloy Weight, lbs.	44,600
3.	Core Diameter, inches	132.7
4.	Active Fuel Height, inches	144 (Region 1) 141.7 (Regions 2 and 3)
	Reflector Thickness and Composition	
5.	Top - Water Plus Steel	∿ 10 in.
6.	Bottom - Water Plus Steel	∿ 10 in.
7.	Side - Water Plus Steel	∿ 15 in.
8.	H_2O/U, (Cold) Core	4.16
9.	Number of Fuel Assemblies	193
10.	UO_2 Rods per Assembly	204

PERFORMANCE CHARACTERISTICS

11.	Heat Output, MWt (initial rating)	2758
12.	Heat Output, MWt (maximum calculated turbine rating)	3216
13.	Fuel Burnup, MWD/MTU	16,700
	First Cycle	
	Enrichments, w/o	
14.	Region 1	2.2
15.	Region 2	2.8
16.	Region 3	3.3
17.	Equilibrium Enrichment	3.2
18.	Nuclear Heat Flux Hot Channel Factor, F_Q^N	2.62
19.	Nuclear Enthalpy Rise Hot Channel Factor, $F_{\Delta H}^N$	1.65

TABLE 3.3 (Cont'd)

CONTROL CHARACTERISTICS

Effective Multiplication (Beginning of Life) with Rods in; No Boron

20.	Cold, No Power, Clean	1.12
21.	Hot, No Power, Clean	1.06
22.	Hot, Full Power, Clean	1.04
23.	Hot, Full Power, Xe and Sm Equilibrium	1.01
24.	Material	5% Cd; 15% In; 80% Ag
25.	Full Length	53
26.	Partial Length	8
27.	Number of Absorber Rods per RCC Assembly	20
28.	Total Rod Worth, BOL, %	(See Table 3.4)

Boron Concentration for First Core Cycle Loading With Burnable Poison Rods

29.	Fuel Loading Shutdown; Rods in (k = .90)	1615 ppm
30.	Shutdown (k = .99) with Rods Inserted, Clean, cold	900 ppm
31.	Shutdown (k = .99) with Rods Inserted, Clean, Hot	625 ppm
32.	Shutdown (k = .99) with No Rods Inserted, Clean, Hot	1455 ppm
33.	Shutdown (k = .99) with No Rods Inserted, Clean, Cold	1420 ppm
	To Maintain k = 1.0 at Hot Full Power, No Rods Inserted:	
34.	Clean, BOL	1210 ppm
35.	After 100 EFPH	930 ppm
36.	Shutdown, All But One Rod Inserted, Clean Cold (k = .99)	965 ppm
37.	Shutdown, All But One Rod Inserted, Clean Hot (k = .99)	730 ppm

TABLE 3.3 (Cont'd)

BURNABLE POISON RODS

38.	Number and Material	1412 Borated Pyrex Glass

KINETIC CHARACTERISTICS

39.	Moderator Temperature Coefficient At Full Power (°F^{-1})	$-.35x10^{-4}$ to $-3.25x10^{-4}$
40.	Moderator Pressure Coefficient (psi^{-1})	$+.2x10^{-6}$ to $+3.00x10^{-6}$
41.	Moderator Density Coefficient, $\Delta k/gm/cm^3$	-.1 to .30
42.	Doppler Coefficient (°F^{-1})	$-1.1x10^{-5}$ to $-1.8x10^{-5}$
43.	Delayed Neutron Fraction, %	.50 to .70
44.	Prompt Neutron Lifetime, sec.	$1.9x10^{-5}$

TABLE 3.4

REACTIVITY REQUIREMENTS FOR CONTROL RODS

Requirements	Per Cent Δρ Beginning of Life	End of Life
Control		
Power Defect	1.54	2.10
Rod Insertion Allowance	.70	.70
Void	.08	.08
Redistribution	.30	.85
Total Control	2.62	3.73

TABLE 3.5

CALCULATED ROD WORTHS, $\Delta\rho$

Core Condition	Rod Configuration	Worth	Less 10%*	Design Reactivity Require-ments	Shutdown Margin
BOL, HFP	53 rods in	9.77%			
	52 rods in; Highest Worth Rod Stuck Out	8.27%	7.44%	2.62%	4.82%
EOL, HZP (1st Cycle)	53 rods in	8.76%			
	52 rods in; Highest Worth Rod Stuck Out	7.28%	6.55%	3.73%	2.82%**

BOL = Beginning of Life

EOL = End of Life

HFP = Hot Full Power

HZP = Hot Zero Power

* Calculated rod worth is reduced by 10% to allow for uncertainties.

** The design basis minimum shutdown margin is 1.95%.

3978-1

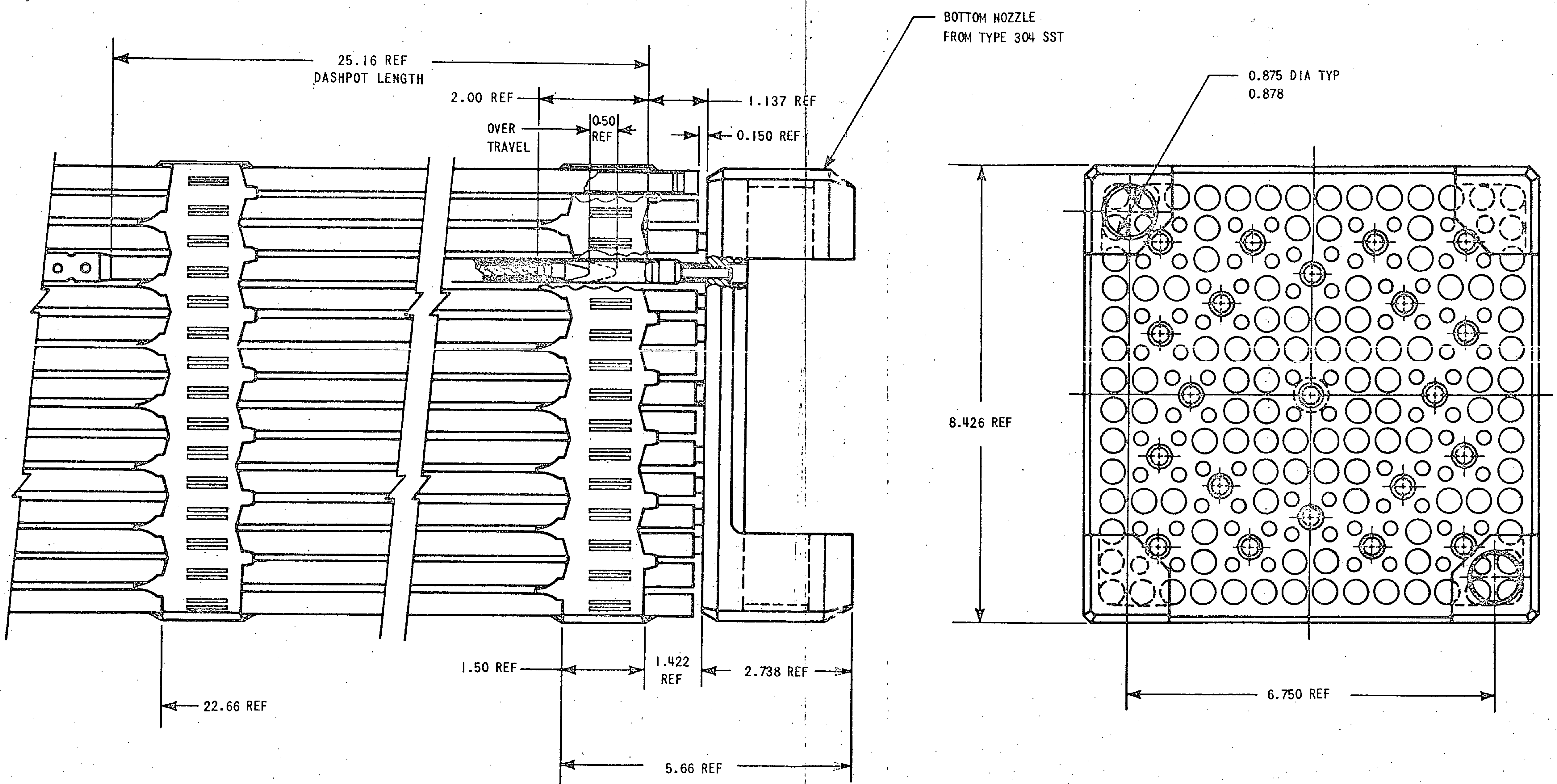


Figure 3.1. Bottom End of Fuel Assembly Showing Plate Type Bottom Nozzle

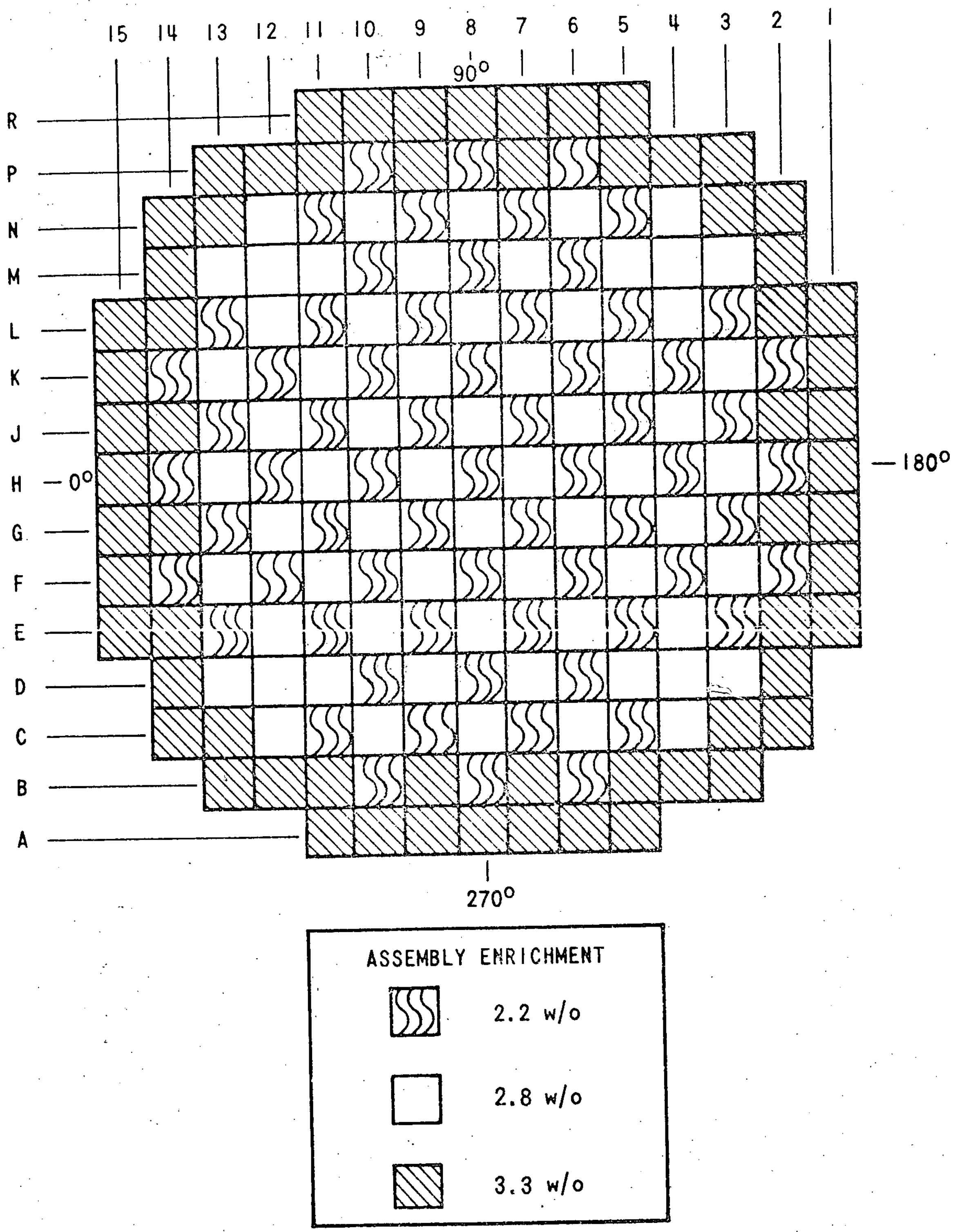


Figure 3.2. Fuel Loading Diagram Indian Point Unit No. 2 Cycle 1

FIGURE 3.3

BURNABLE POISON PATTERN

					9		7		9					
		8		12		16		16		12		8		
	8		20		12		16		12		20		8	
		20		20		16		16		20		20		
	12		20		16		16		16		20		12	
9		12		16		20		20		16		12		9
	16		16		20		16		20		16		16	
7		16		16		16		16		16		16		7
	16		16		20		16		20		16		16	
9		12		16		20		20		16		12		9
	12		20		16		16		16		20		12	
		20		20		16		16		20		20		
	8		20		12		16		12		20		8	
		8		12		16		16		12		8		
					9		7		9					

TOTAL = 1412

4.0 FUEL PERFORMANCE LIMITS

4.1 LOCAL POWER PEAKING DUE TO FUEL DENSIFICATION

For the purpose of re-evaluating fuel performance limits including the effects of fuel densification on local power peaking, a series of conservative assumptions have been made. These are based on visual observation of fuel clad flattening and on in-core flux traces in the Beznau, R. E. Ginna, Point Beach 1 and H. B. Robinson reactors. The data on which these assumptions are based and the methods of determining local power peaking due to gaps in the fuel column are given in detail in WCAP-7978[1]. The major assumptions for determining local power peaking are:

a) For the purpose of determining acceptable operation it has been conservatively assumed that full fuel pellet densification takes place at zero burnup.

b) The local power peaking effect is calculated with the following assumptions relative to the characteristics of axial gaps:

1. All fuel rods are subject to densification.

2. The frequency distribution of gaps by axial position is based on in-core flux traces from Point Beach 1 and H. B. Robinson. It is assumed that the frequency of occurrence of significant gaps increases linearly with height from the bottom of fuel.

3. The distribution of gap size is based on rod flattening observed in the R. E. Ginna reactor between the 120 in. and 140 in. elevations. This distribution is the same as that recommended by the AEC in their "Technical Report on Densification of Light Water Reactor Fuels".[2]

4. The maximum size for a gap increases linearly with height from the bottom of fuel (H) according to

$$\left(\frac{0.965-\rho}{2} + 0.004\right) H$$

where ρ is the initial density of the fuel.

5. The distribution of gap size at any elevation is the distribution from (3) with the scale multiplied by the appropriate factor to produce a maximum gap size equal to that given by (4).

d) The criterion chosen for an acceptable design value of power peaking due to fuel densification, including the power spike, is that less than one fuel rod will exceed F_Q^N at a 95% confidence level.

The local peaking due to gaps in the fuel column as a function of axial position has been calculated for the Indian Point Unit 2 reactor - Cycle 1 and the results are given in Figure 4.1.

4.2 TOTAL POWER PEAKING FACTOR, F_Q

Since the magnitude of the local power peaking varies as a function of height in the core, it has been applied to the basic power shape data which determines the design value of F_Q. The basis for protection against exceeding local power density limits is given in topical reports which describe the use of excore detector signals (axial offset) in the over-power protection logic.

An upper bound on F_Q is set as a function of axial offset by consideration of all allowable operating situations. When F_Q is increased locally by the height dependent local power peaking, the individual points are increased in F_Q by different amounts. The result is a revised plot of F_Q vs. axial offset which requires a revised upper bound different in shape and magnitude from the previous upper bound.

For the fuel cycle under consideration, the results are given in Figure 4.2 which incorporates the flux peaking penalty of Figure 4.1. Figure 4.2 also includes a 5% margin for uncertainty and 3% for manufacturing tolerances. In the calculations it was assumed that the horizontal peaking factor, F_{xy}, at the plane of the peak local power was no lower than 1.44. A high F_Q could occur as a result of control rod insertion to the control rod insertion limits in the Technical Specification. For this reason, the plot contains points at large axial offset evaluated for less than full power, with an F_{xy} appropriate to the plane of the peak F_Q. An additional operating limit is that no part length control rods are permitted in the bottom third of the core at full power. The boundary is used to define protection set points and operating limits for all conditions. The figure indicates that a total peaking factor, F_Q, of 2.70 can be maintained by reference only to the ex-core detector axial offset.

4.3 THERMAL AND HYDRAULIC PARAMETERS

Table 4.1 presents a summary of the pertinent design parameters which have been changed from those presented in the FSAR. An explanation or description of each change is presented below.

a) $F^N_{\Delta H}$ has been reduced from 1.75 to 1.65 in order to provide additional margin for DNB to offset the penalty for fuel densification. This is discussed in Section 3.1.2.

b) F_Q has been decreased to 2.70. This allows for local power peaking due to fuel densification. This is discussed in Section 4.2.

c) The reference axial power distribution for DNB analysis has been changed from a chopped cosine with a 1.79 peak to one with a 1.55 peak. This has an impact on the permissible range of axial offset under normal operating conditions and is reflected in the overtemperature ΔT protection set points. The tighter control of power distribution that is required for DNB protection is not inconsistent with the restrictions already imposed by operation to meet the revised peak power density limits.

d) Based on Westinghouse latest rod bundle DNB data, the W-3 DNB correlation for both a typical cell (all channel walls heated) and a thimble cell (partial channel walls heated) is applied in the present analysis. (See Section 4.5)

e) F^E_Q for DNB evaluations has been increased from 1.03 to 1.05 due to fuel densification considerations. Fuel pellet shrinkage increases the fuel/clad gap and the factor to account for non-uniform azimuthal heat flux. This non-uniform flux is caused by the clad developing an ovality and contacting the pellet at two points. Reduced fuel rod circumference and heat transfer area is also considered in the value.

f) The fuel densities differ from the FSAR values. For Regions 2 and 3, the analysis assumes a geometric density is 94.3% for the nominal design value of 95%. For Region 1, the as-built density 93.6% is employed in place of the design value of 94%.

g) The core average active fuel height and heat transfer surface area have been revised to include i) the new design values for the fuel stack height as described in Section 3.1.1.3 and ii) the effect of core average pellet densification and thermal expansion.

4.4 EFFECT OF FUEL DENSIFICATION ON FUEL TEMPERATURES

In addition to the local power peaking, fuel densification may result in an increase in the radial gap between the fuel and the clad, leading to a decrease in gap conductance and an increase in fuel temperature. Densification also leads to an increase in linear heat generation rate due to the reduction in the fuel length. Later in life, the cladding creeps down onto the fuel and the heat transfer performance of the fuel is improved.

Fuel column length changes have been measured by gamma scan as discussed in Reference 3. The reduction in pellet stack height conservatively determined from these data is used in the determination of the linear heat generation rate. That is

$$\frac{\Delta L}{L} = \frac{0.965-\rho}{2}$$

where ρ is the initial density of the fuel.

The densification of the fuel pellet is assumed to occur immediately and the effect of the resulting increase in the fuel-clad gap on the fuel centerline and average fuel temperature was determined with the models discussed in Reference 3.

Calculations of the linear power increase and of the fuel temperature, with densification, utilize the as-fabricated average density of the Region 1 fuel. This is the lowest density and most limiting region in the core.

The effect of the statistical variations in the pellet density on the densified pellet length and radius are accounted for by increasing the power spike used in the temperature calculations. This is done by including equivalent thermal effects of pellet density variations in the probability analysis for the power spike as described in Reference 1. The evaluation is based on; (a) the spike probability and the x-y power census as presented in Reference 1 with modifications per Reference 2, (b) the variability in the pellet density obtained from Region 1 pellet density measurements, and (c) the relationship between a change in initial fuel density and the associated change in the loss of coolant linear power limit, or in the thermal overpower limit.

This analysis results in the power peaking being increased 0.7% for loss of coolant evaluations and 2.0% for thermal overpower evaluations. The methods used in the probability calculation provide a confidence level of 95% that the actual power peaking in the reactor core will not exceed the maximum calculated peaking for more than one rod in the core. The values are different for LOCA and overpower because of different ratios of power limit to initial density variation [Item (c) above].

The criteria for overpower transients is that fuel pellet centerline melting will not occur. In addition, peak fuel rod power is not permitted to exceed 21.1 kw/ft during an overpower transient.

Figures 4.3 and 4.4 give the results of fuel centerline and fuel average temperature calculations as a function of burn-up for the most limiting region, that is, Region 1 with the lowest density fuel. The calculation of clad creep was for a fuel rod in that region having low burnup, thereby, the clad creep rate and associated temperature reduction are minimized.

A temperature limit of 4700°F is applied for the first several thousand hours of operation when centerline temperature can be limiting. The difference between this value and the UO_2 melting temperature (5080°F at BOL and decreasing by 58°F per 10,000 MWD/MTU) provides margin for uncertainties in the evaluation.

As seen in Figure 4.3, the limit of 21.1 kw/ft is reached slightly before a fuel centerline temperature of 4700°F is attainable.

4.5 DNB EVALUATION

4.5.1 HIGH PRESSURE DNB DATA

With the conclusion of the Westinghouse ESADA DNB program[4], where data was obtained from rod bundle geometries, mixing vane grids, non-uniform axial heat flux distributions and pressures from 1490 to 2400 psia, Westinghouse has removed an uncertainty factor which was applied to the W-3 correlation previously used in all reactor designs, including that presented in the FSAR for the Indian Point Power Station. This factor was applied for conservatism because of the small amount of DNB data previously available at higher reactor operating pressures.

4.5.2 Definition of DNB Heat Flux Ratio

The DNB heat flux ratio as applied to this design when all flow cell walls are heated is

$$\mathrm{DNBR} = \frac{q''_{DNB,N}}{q''_{loc}} \tag{1}$$

where

$$q''_{DNB,N} = \frac{q''_{DNB,EU}}{F} \tag{2}$$

and $q''_{DNB,EU}$ is the uniform DNB heat flux as predicted by the W-3 DNB correlation[5] when all flow cell walls are heated.

F is the flux shape factor to account for nonuniform axial heat flux distributions[5] with the "C" term modified as in Reference (6).

q''_{loc} is the actual local heat flux.

The DNB heat flux ratio as applied to this design when a cold wall is present is

$$DNBR = \frac{q''_{DNB,N,CW}}{q''_{loc}} \tag{3}$$

where

$$q''_{DNB,N,CW} = \frac{q''_{DNB,EU,Dh} \times CWF}{F} \tag{4}$$

where

$q''_{DNB,EU,Dh}$ is the uniform DNB heat flux as predicted by the W-3 cold wall DNB correlation[7] when not all flow cell walls are heated (thimble cold wall cell).

$$CWF^{(7)} = 1.0\text{-}Ru \left[13.76-1.372e^{1.78x}-4.732 \left(\frac{G}{10^6}\right)^{-.0535} -.0619 \left(\frac{P}{1000}\right)^{.14} -8.509Dh^{.107}\right] \tag{5}$$

and $Ru = 1 - De/Dh$

4.5.3 F-Factor Evaluation

The W-3 DNB correlation includes a factor to account for axially nonuniform heat fluxes.[8]

An evaluation of the ability of this factor to predict DNB behavior of short heat flux spikes was done by comparing the measured DNB heat fluxes from two experimental test sections[9,10] to that calculated. The mean of the measured to predicted evaluations was 0.98. The scatter of the points about the mean was +18, -14 percent. No trends within the scatter were noted, and it was concluded that the non-uniform factor in the W-3 adequately describes the DNB behavior of heat flux spikes.

4.5.4 DNB Method

The DNB evaluation method with densification is summarized in Table 4.2. For all analyses the power peak and engineering hot channel factor described previously are applied at the axial location of minimum DNBR.

The heat flux spike shape, i.e., axial heat flux distribution in a given fuel rod, is the sum of the contribution of fuel pellet separation in neighboring rods within three rows of that rod. An examination of combinations of gap positions that would lead to the largest power spike was made. From this evaluation the combination of gaps was selected that resulted in the largest power spike over the greatest fuel rod length. This in turn gives the maximum DNB penalty.

The magnitude of the power spike used for DNB purposes is conservatively assumed to be the magnitude of the spike 4 inches from the top of the core. In other words, flux shapes which lead to a minimum DNBR in the top 4" of the fuel rod do not occur. A conservative trapezoidal approximation to the heat flux spike has been developed for DNB purposes and is applied at the point of minimum DNBR.

The standard DNB evaluation method (THINC I code) has been modified to evaluate the DNBR including the power spike. The reduction in active pellet height due to densification is considered by increasing linear heat generation rate by a factor of 1.02. Consideration of the "as fabricated" fuel length and axial thermal expansion as well as the densification are included in the evaluation.

Core DNB limits have been determined based on the design parameters listed in Section 4.3 and the DNB method outlined in Section 4.5.2. The results are given in terms of core power and T_{avg} limits in Section 5.3.

References for Section 4

1) George, R. A., W. D. Leggett III, "Power Spike Model", WCAP-7978, October 1972.

2) "Technical Report on Densification of Light Water Reactor Fuels", Regulatory Staff U.S.A.E.C., November 14, 1972.

3) Eng. G., et. al., "Fuel Densification Penalty Model", WCAP-7984, October 1972.

4) Rosal, E. R., J. O. Cermak, L. S. Tong, "Rod Bundle Axial Non-Uniform Heat Flux DNB Tests and Data", WCAP-7411-L Rev. I, May 1970, Westinghouse Proprietary Class II.

5) Tong, L. S., "Prediction of Departure from Nucleate Boiling for an Axially Non-Uniform Heat Flux Distribution", J. Nul. Energy, 21, pp. 241-248, 1967.

6) Trojan PSAR, Portland General Electric Co., Docket 50-344.

7) Tong, L. S., "Boiling Crisis and Critical Heat Fluxes", AEC Critical Review Series, TID-25887, 1972.

8) Tong, L. S., H. B. Currin, P. S. Larsen and O. G. Smith, Chemical Engineering Progress Symposium Series, Vol. 62, Number 64, 1966.

9) Styrikovich, M. A., R. L. Miropol'skii and V. K. Eva, "The Influence of Local Raised Heat Fluxes Along the Length of the Channel on the Boiling Crises", Soviet Physics Doklady, Vol. 7, 1963.

10) Weiss, A., "Hot Patch Burnout Test on a 0.097 inch x 1.0 inch x 27 inch Long Rectangular Channel at 2000 psia", WAPD-TH-388, 1957.

TABLE 4.1

COMPARISON OF THERMAL DESIGN PARAMETERS

	FSAR	Present Supplement
Nuclear Enthalpy Rise Hot Channel Factor for DNB evaluation, $F^N_{\Delta H}$ (ratio of the integral of the heat generation rate within the hottest rod to the heat generation rate in the average rod including an uncertainty factor.)	1.75 (10% uncertainty)	1.65 (10% uncertainty)
Total heat flux hot channel factor, F_Q (ratio of maximum core heat flux to average core heat flux)	3.00 (Interim Acceptance Criteria)*	2.70 (Includes densification penalty)
Heat flux engineering hot channel factor, F^E_Q		
a) kw/ft evaluation	1.03	1.03
b) DNB evaluation	1.03	1.05
Reference axial power distribution for DNB Evaluation	1.79 chopped cosine	1.55 chopped cosine
Region Densities, Geometric, %		
Region I	94	93.6
Region II	92	94.3
Region III	91	94.3
Core Average Active Fuel Height, inches	144.0	141.5
Heat transfer surface area, ft^2	52,200	51,300
DNB Correlation	W-3	W-3
Minimum DNBR for DNB core safety limits	1.30	1.30

* Presented in "Additional Testimony of Applicant Concerning Emergency Core Cooling System Performance", July 13, 1971, Docket No. 50-247.

TABLE 4.2

DNB EVALUATION METHOD

Local power peaking due to fuel column gaps is applied at location of minimum DNBR for all steady state and transient evaluations.

Heat flux increase due to total fuel pellet stack height reduction is included.

Maximum length of local power spikes is used.

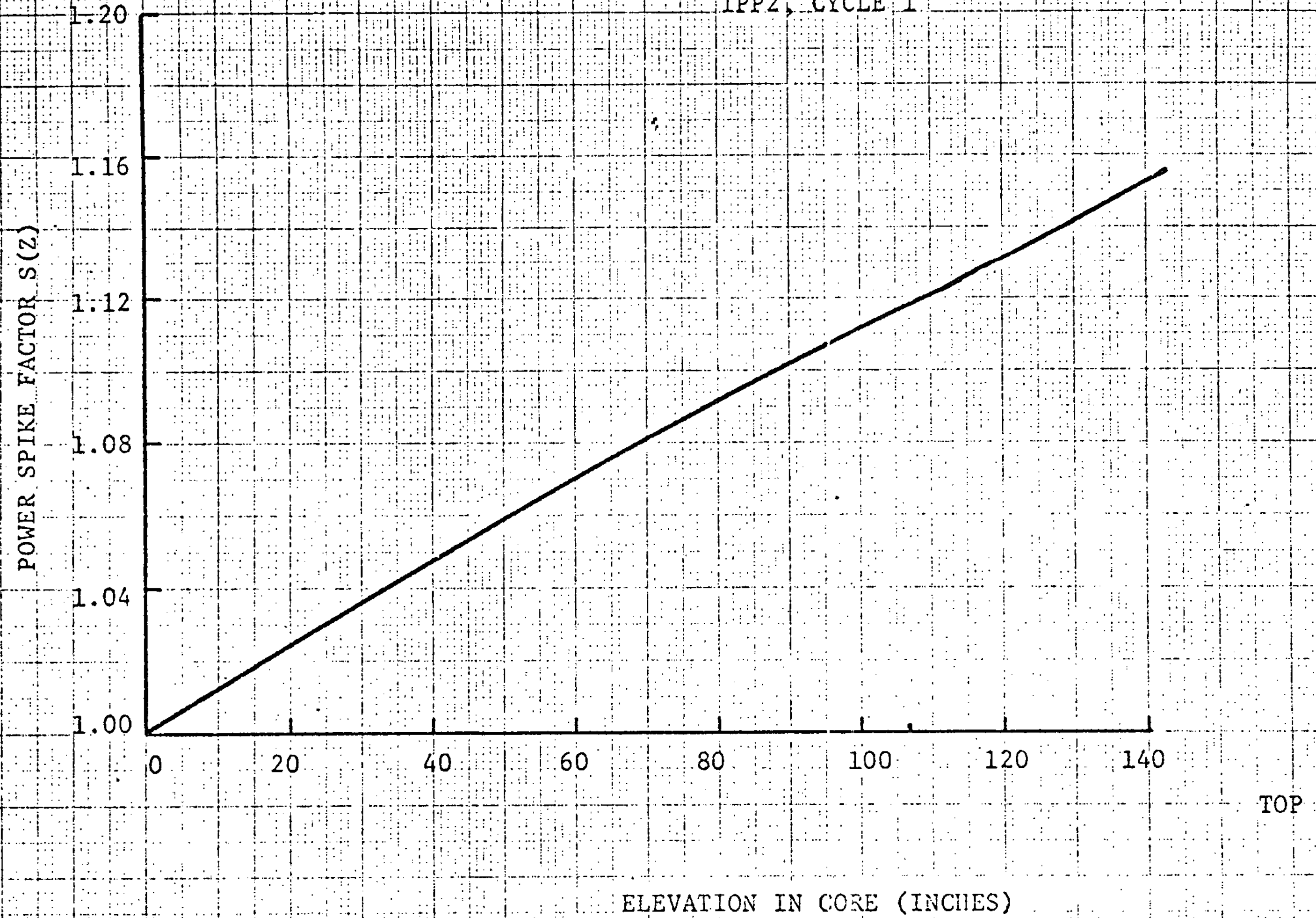
Additional hot channel factor due to increased pellet-clad eccentricity is applied, 1.019.

Standard DNB evaluation methods (THINC Code) have been modified to include above effects and are used in analysis.

FIGURE 4.1

POWER SPIKE FACTOR VS ELEVATION

IPP2, CYCLE 1



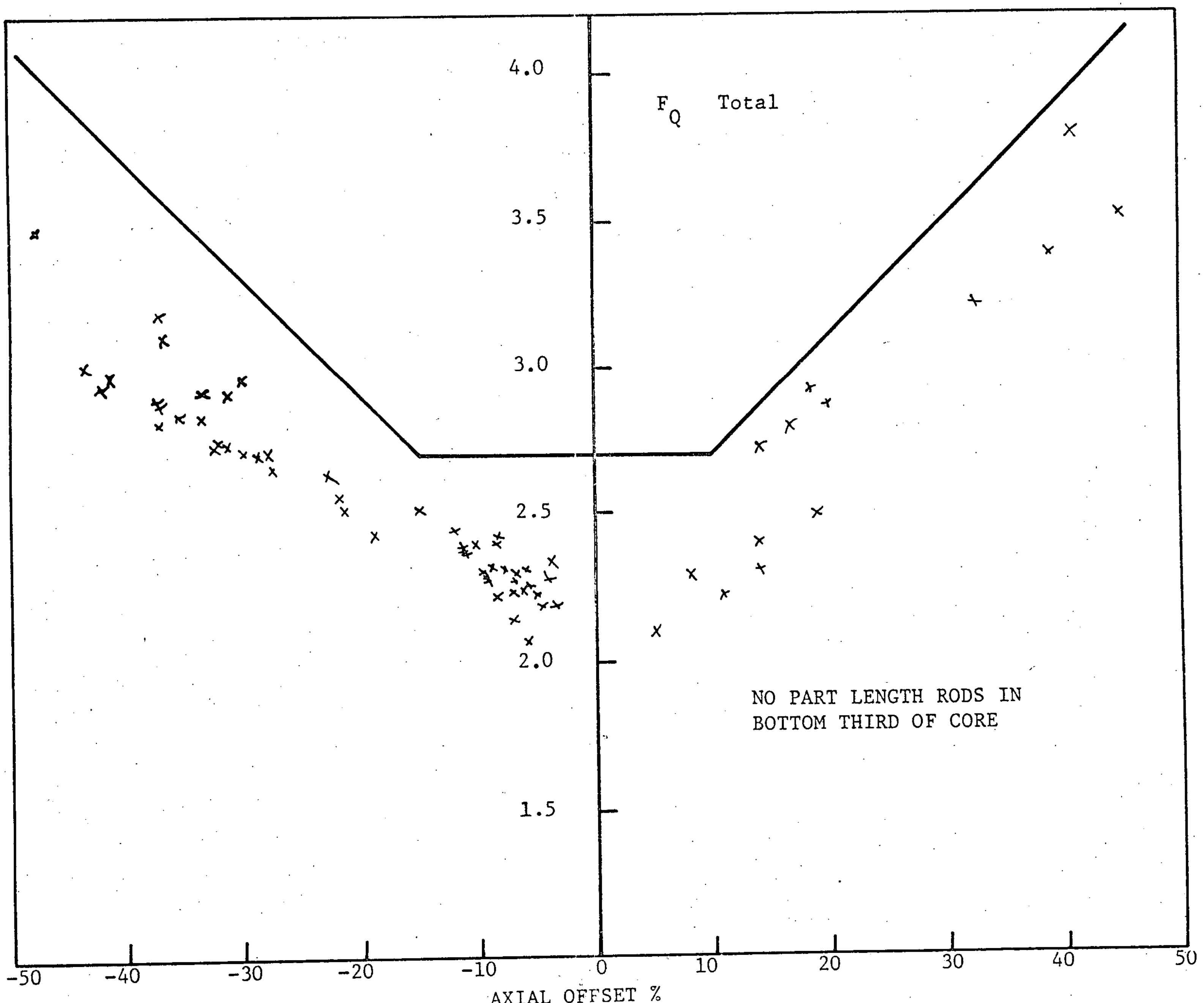


Figure 4.2 TOTAL HEAT FLUX PEAKING FACTOR - F_Q VS AXIAL OFFSET

FIGURE 4.3

INDIAN POINT UNIT 2
REGION 1
MINIMUM BURNUP ROD
WITH DENSIFICATION
FUEL CENTERLINE TEMPERATURE
VS
ROD POWER AT VARIOUS OPERATING TIMES

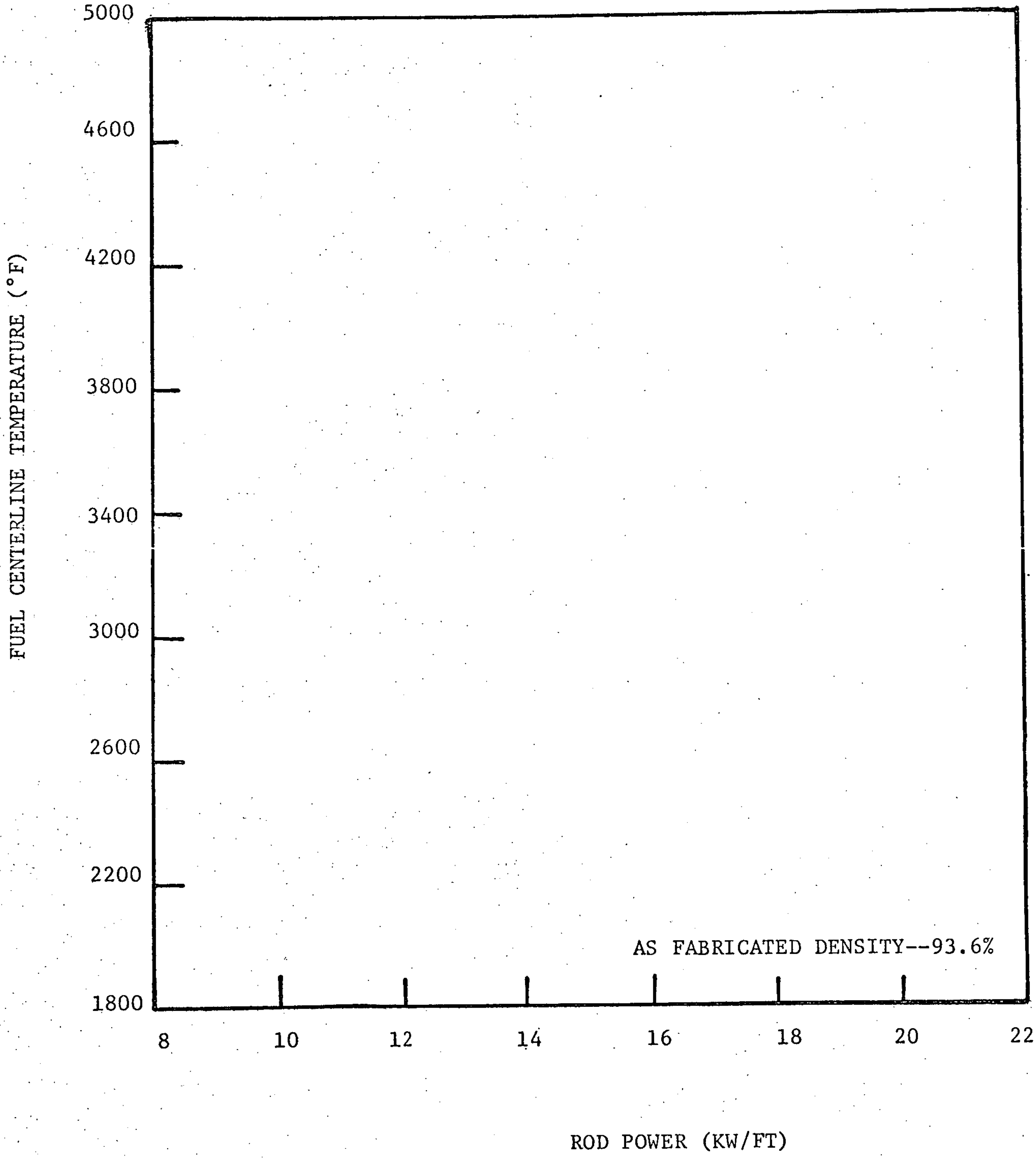
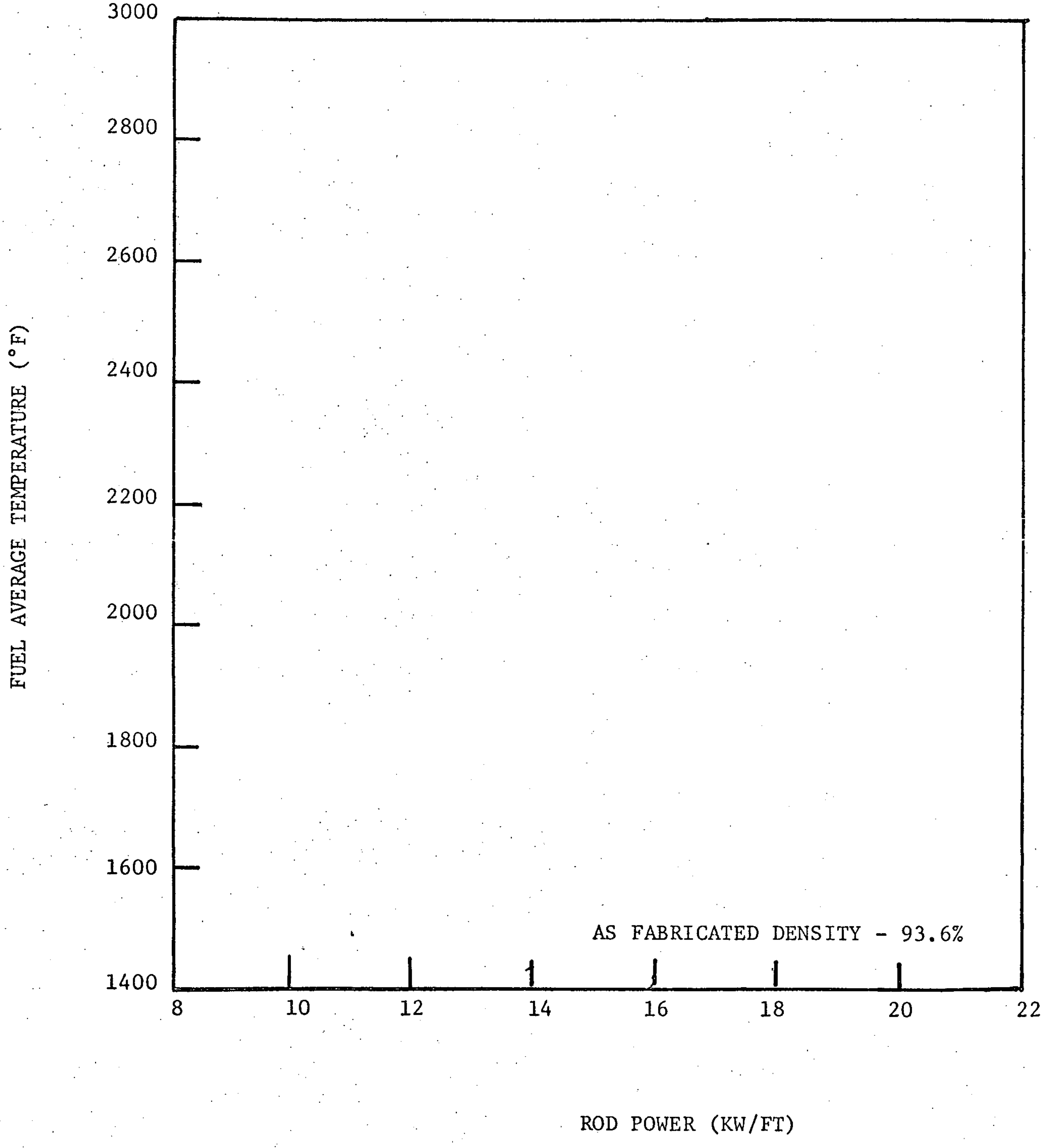


FIGURE 4.4

INDIAN POINT UNIT 2
REGION 1
MINIMUM BURNUP ROD WITH DENSIFICATION
FUEL AVERAGE TEMPERATURE
VS
ROD POWER AT VARIOUS OPERATING TIMES



5.0 POWER CAPABILITY

5.1 GENERAL

The effects associated with fuel densification can be separated into three categories as below:

1) Reduction in pellet stack height due to densification. This effect increases the average linear rod heat flux by an amount equivalent to the percentage reduction in pellet stack height.

2) Power spikes caused by axial gaps in a fuel rod and in surrounding rods. This effect increases core peaking factors by the value of the power spike.

3) Increase in pellet-cladding gap due to radial densification. This effect increases fuel pellet temperatures.

The first two of these effects cause an increase in local rod power (kw/ft). The third effect causes, as noted, higher pellet temperatures and thus increases stored energy in the fuel.

These three phenomena have some effect on most of the design bases transients and postulated accidents analyzed in the FSAR. However, the effects of fuel densification can be accommodated in the design and operation of Indian Point Unit No. 2 without any loss of power capability. For some transients, the penalties incurred may be absorbed without requiring additional restrictions on operation to meet design basis criteria. These transients are discussed in Section 6. The types of incidents which are most inclined to impose restrictions on operation are those involving overpower transients, those affecting DNB safety limits, and the Loss of Coolant accident and are addressed here in Section 5. The requirements imposed to meet design basis criteria for these incidents thus determine plant power capability. These requirements are discussed below.

5.2 OVERPOWER TRANSIENT LIMITS

The criterion for overpower protection requires that the maximum fuel temperature be limited to a value less than the fuel centerline melting temperature for normal operation and anticipated transients. This protection is provided by the Overpower ΔT trip and the nuclear overpower trip. As a basis for establishing overpower protection system setpoints, a calculated centerline fuel temperature of 4700°F has been selected as the overpower limit. In addition, peak fuel rod power is not permitted to exceed 21.1 kw/ft during an overpower transient.

Fuel centerline temperatures have been calculated for the most limiting region (Region 1) as a function of local linear power and fuel burnup using the methods discussed in Section 4; these results are shown on Figure 4.3.

Considering the effect of radial fuel densification on fuel temperature at BOL the maximum centerline temperature at the design overpower limit of 21.1 kw/ft is 4640°F.

To provide margin for operation and allowance for instrument errors, the maximum overpower limit should be about 20% of rated power greater than the allowable operating power. For this core with a total peaking factor F_Q of 2.70 (as discussed in Section 4.2) the maximum overpower limit at BOL is:

$$\text{Maximum overpower limit} = \frac{21.1}{5.7 \times 1.02 \times 2.7 \times 1.02} = 131.8\%$$

where the factors above include a 2% allowance for axial fuel stack height change from the design value (due to shrinkage and thermal expansion) which is a direct multiplier on the average linear rod power at rated power, and an additional 2% allowance for the effect of local pellet density variations on fuel centerline temperatures as discussed in Sec. 4.4.

Since fuel centerline temperatures for a given linear rod power decrease with burnup due to clad creep down, the BOL conditions are the most restrictive with respect to overpower transient limits. Since the transient overpower limit is 131.8% of rated power, the margin to this limit is more than sufficient to allow operation at full rated power.

5.3 DNB LIMITS

5.3.1 DNB CORE SAFETY LIMITS

The criterion for DNB protection requires that the minimum DNBR will be no less than 1.30 for normal operation and anticipated transients. The primary DNB protection is provided by the Overtemperature ΔT trip.

The DNB core safety limits have been recalculated to allow for the effects of fuel densification. The recalculation of these limits includes DNB penalties for increased pellet eccentricity, local power spikes, local pellet density variations, 10% uncertainty in $F_{\Delta H}^{N}$, a reference cosine with a peak of 1.55 for axial power shape, and current DNB technology as discussed in Section 4.

The recalculated DNB core safety limits have been found to be less limiting than those previously presented in the FSAR, i.e. the reduction in design peaking factors more than offsets the effects of fuel densification on the DNB Ratio. Thus the DNB core limits are adequate and conservative as presented in the FSAR and the Overtemperature ΔT reactor trip setpoints need not be revised.

5.3.2 DNB PROTECTION ANALYSIS

The loss of reactor coolant flow accident is a rapid transient which is not terminated by the Overtemperature ΔT trip. This transient has been analyzed on a conservative basis and it has been determined that the limiting criteria of DNBR $\geq$ 1.30 is met for the worst case. The analysis performed for the loss of reactor flow and other incidents is discussed in detail in Section 6.

5.4 LOSS OF COOLANT ACCIDENT LIMITS

The Westinghouse evaluation model has been utilized to evaluate the effects of fuel densification on the LOCA transient in accordance with the requirements of the Interim Policy Statement.

The limiting criterion is the limit on peak clad temperature, i.e. 2300°F. As shown by core cooling analysis presented in the FSAR, the worst break is the double-ended cold leg guillotine and this break has been reanalyzed considering the effects of fuel densification. The blowdown transient (SATAN code) was calculated at 102% of rated core power. The fuel temperature calculations were made with the LOCTA code at various initial hot spot pellet average temperatures to determine the maximum allowable linear rod power. The loci of initial pellet average temperature and kw/ft which meet the Emergency Core Cooling System criteria are shown in Figure 5.1. Figure 5.1 also shows the calculated average fuel rod temperature for Region 1 with densified fuel versus kw/ft at various values of fuel burnup determined as described in Section 4. The average fuel temperature decreases with reactor operation because of clad creep down and consequently higher peak local rod power is acceptable with increased burnup.

From Figure 5.1, the maximum linear rod power which meets the ECCS criteria at beginning of life is 17.35 kw/ft. The maximum linear rod power for operation at full rated power is

$$5.7 \times 1.02 \times 1.02 \times 2.70 \times 1.007 = 16.12 \text{ kw/ft}$$

The maximum linear rod power calculated above includes the following allowances:

(1) Calorimetric error - 2%

(2) Stack height shortening - 2%

(3) Effect of local pellet density variations on average fuel temperature at the hot spot (as discussed in Section 4.4) -0.7%

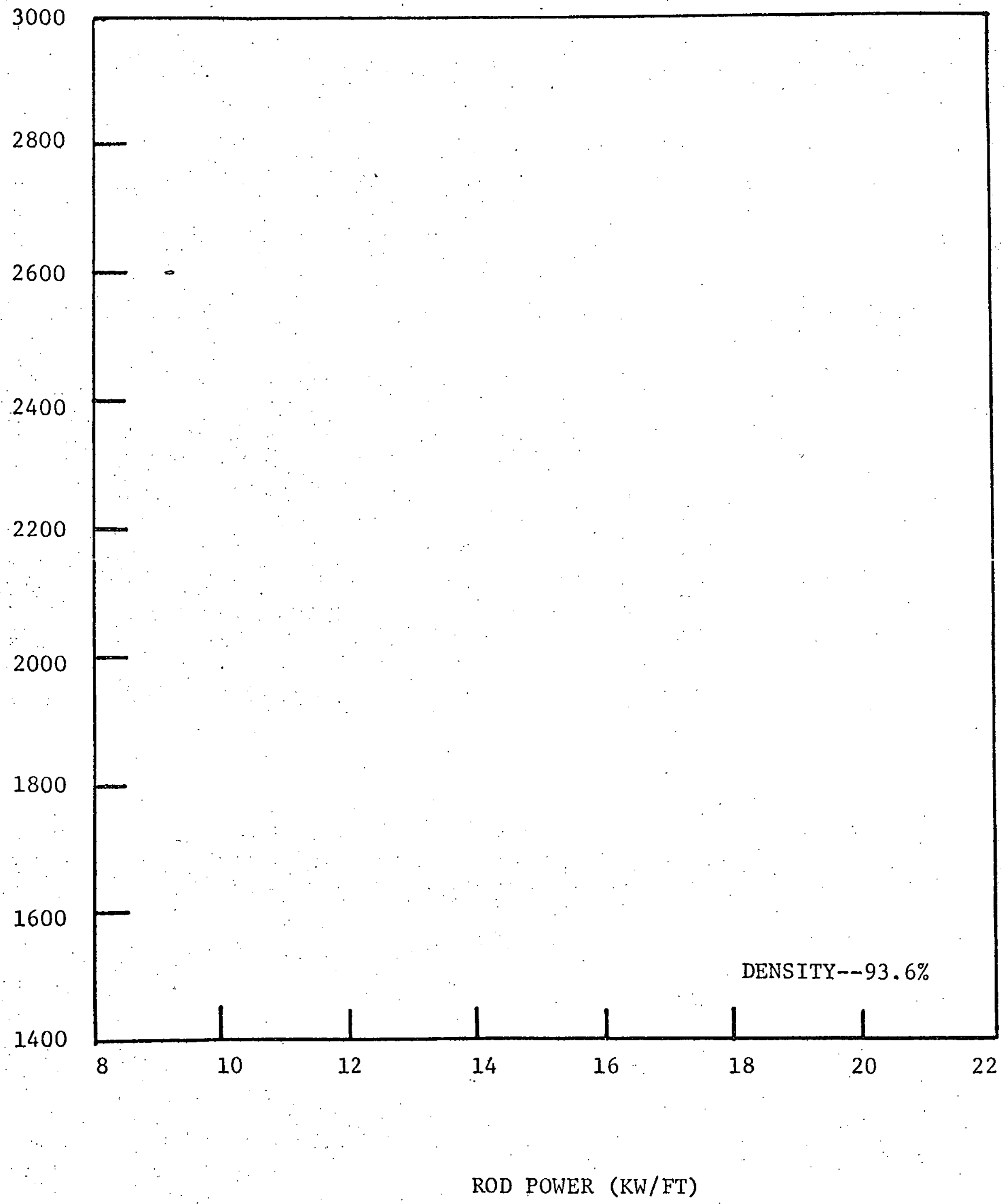
The loss of coolant accident becomes less restrictive with burnup due to clad creepdown. Since the maximum rod power at full rated power is less than the BOL LOCA limit, loss of coolant considerations do not restrict plant power capability.

5.5 CONCLUSIONS

Plant power capability is limited by LOCA, overpower, and DNB safety limits. Restrictions imposed by these limits have been determined with allowance for the effects of fuel densification. The results of these analyses show that the plant can be operated at full rated power without exceeding the limits imposed by a conservative consideration of the effects of fuel densification on plant operation.

FIGURE 5.1

INDIAN POINT UNIT 2
REGION 1
MINIMUM BURNUP ROD WITH DENSIFICATION
FUEL AVERAGE TEMPERATURE
VS
ROD POWER AT VARIOUS OPERATING TIMES



6.0 EFFECTS OF FUEL DENSIFICATION ON ACCIDENT ANALYSIS

6.1 GENERAL

The effects of fuel densification on the design basis and postulated incidents analyzed in the FSAR have been examined. For most cases the effects due to fuel densification can be accommodated within the conservatism used in the FSAR analysis or in the large margins to design basis limits demonstrated by the results presented in the FSAR. Those incidents which are most inclined to impose limitations on plant power capability have been discussed in Section 5.

Calculations have been performed to determine reactivity parameters based on the present core loading. The range of the doppler coefficient (-1.1×10^{-5} to -1.8×10^{-5} $\delta k/k/°F$) is the same as the range for the original design as presented in Table 1.4.1 of the FSAR. The moderator temperature coefficient has a range of $-.35 \times 10^{-4}$ to -3.25×10^{-4} $\delta k/k/°F$ which falls outside the range presented in Table 1.4.1 of the FSAR; however, the range assumed in the accident analysis is 0 to -3.5×10^{-4} $\delta k/k/°F$. Although the boron concentrations given in Table 3.2.1-1 are 50 ppm higher than the values given in Table 1.4.1 of the FSAR, they are well within the conservative values assumed for the Chemical Volume and Control System Malfunction analysis presented in the FSAR. Therefore, the analyses previously presented are valid and conservative with respect to the reactor kinetics characteristics of the present fuel.

6.2 OVERPOWER - OVERTEMPERATURE TRANSIENTS

As discussed in Section 5, anticipated overpower and overtemperature transients will be terminated by the protection system before a DNB ratio of less than 1.30, local linear power density of 21.1 kw/ft or fuel centerline melting temperature is reached. Transients of this type analyzed in the FSAR include:

1) Uncontrolled Control Rod Assembly Withdrawal at Power
2) Excessive Load Increase Incident
3) Excessive Heat Removal Due to Feedwater System Malfunctions

Therefore, the consequences of these accidents are not changed from those noted in the FSAR.

6.3 INCIDENTS WHICH DO NOT REQUIRE REANALYSIS

Uncontrolled Control Rod Assembly Withdrawal from a Subcritical Condition and Startup of an Inactive Loop are non-limiting transients as demonstrated in the FSAR. For these transients, core power and fuel temperatures are much less than the transient overpower limits discussed in Section 5 (i.e., peak heat flux of 45% of nominal for the RCCA Withdrawal incident and maximum nuclear power of 104% of nominal for Inactive Loop Startup, compared to the maximum transient overpower limit of 131.8%). Therefore, fuel densification effects on these transients will not require the imposition of further protection requirements and the consequences of these transients are unchanged from those stated in the FSAR.

Because of the radial pellet shrinkage, there is a resultant increase in core stored energy, which would tend to make the Loss of Normal Feedwater and Loss of AC Power accidents more severe. However, the amount of core stored energy, including densification penalties, has been recalculated, assuming BOL fuel temperatures, and found to be less than 75% of the conservative value assumed in the original FSAR analyses. This, along with the fact that fuel temperature will decrease with burnup, assures that the FSAR analyses as presented are sufficiently conservative and the conclusions are valid as presented.

The Loss of External Load accident presented in the FSAR demonstrated a large margin to DNB. The increase in core stored energy due to densification is considerably less than that for the case assuming a reduction of calculated overall fuel heat transfer coefficient (UA) at beginning of life by a factor of 2 (minimum DNBR, 1.6). The heat flux peaking effects of densification on the DNB ratio are offset by the reduction in design peaking factor, thus the results and conclusions of the previous analysis remain valid.

The Control Rod Assembly Drop incident analyzed in the FSAR demonstrated a large margin to a DNB ratio of 1.30, in terms of core radial peaking factor increase. While densification effects may reduce the allowed radial peaking factor, the effect on allowed change in the peaking factor will be offset by the reduction in design peaking factor as evidenced by the recalculated core DNB limit curves discussed in Section 5. Action of the rod drop detection circuit in reducing power will further increase margin to DNB and assures that the conclusions about the consequences of this incident as presented in the FSAR are valid.

As discussed in Section 6.1, the reactivity parameters of the revised core loading are within values allowed by the analysis for the various core loading; these parameters are not affected by fuel densification. Thus, the conclusions reached in the FSAR about the CVCS Malfunction accident and the small (credible) steam line breaks are still valid.

For the large (incredible) steamline breaks, the minimum DNB ratio for every case analyzed in the FSAR was greater than 2.0. Physics calculations for the modified core with allowance for fuel densification demonstrated larger shutdown margins and smaller peaking factors than the values assumed in the FSAR analysis. Therefore, the conclusions reached in the FSAR remain valid.

For the Fuel Handling Accident, an evaluation of the effects of fuel densification on the fission product release from the fuel to the gap indicates that the amounts of activity noted in the FSAR are sufficiently conservative to adequately describe any small effects due to relatively small changes in fuel temperature with some compensating effects due to the densification. Thus, the consequences of this accident are not changed from those noted in the FSAR and the Environmental Report.

The Technical Specifications establish the maximum coolant activity based upon limiting the off-site consequences of the assumed steam generator tube rupture. The coolant activity is not affected by fuel densification. Thus, the FSAR and Environmental Report analyses are still valid. Additionally, the results of analyses presented for accidental releases of recycle or waste liquid, waste gas, or a Volume Control Tank accident are also still valid.

6.4 LOSS OF COOLANT ACCIDENT

The effects of fuel densification on the limiting Loss of Coolant Accident have been discussed in Section 5 where it has been determined that the design basis criteria are met for operation at rated power.

The worst case of the spectrum of small break LOCA's has been reanalyzed at rated power to account for fuel densification effects. For this case, the clad temperature increase above the analyses previously submitted is about 60°F, still resulting in peak cladding temperatures well below the limiting value of 2300°F.

Additionally, the effect of increased core stored energy due to fuel densification on the containment energy release transient for the Containment Transient Analysis is more than adequately compensated for by the conservatism included in the FSAR analysis.

6.5 RUPTURE OF CONTROL DRIVE MECHANISM HOUSING, CONTROL ROD EJECTION

6.5.1 INTRODUCTION

The rod ejection transient analysis is performed in two stages; a nuclear power transient calculation and a hot spot fuel heat transfer analysis. As a result of fuel densification, only the hot spot calculation is affected significantly. The effects are:

a) The pellet shrinkage in the radial direction causes the gap heat transfer coefficient to decrease, resulting in an increase in the steady state fuel pellet temperature and therefore the stored energy for a given kw/ft.

b) The axial gap formation causes a local increase in the heat generation rate in an adjacent fuel rod, which may be represented by an increase in the steady-state and transient axial hot channel factor (F_Q^N). This effect increases the fuel stored energy before rod ejection (for at-power cases) and acts as a multiplier on the energy release at the hot spot due to the nuclear transient.

6.5.2 METHOD OF ANALYSIS

Both the nuclear power transient and hot spot heat transfer calculations were repeated for this plant. This was warranted by the core modifications discussed in Chapter 3. The ejected rod worths and hot channel factors were calculated taking into consideration the insertion limits for each case. The insertion limits, assumed for this analysis, are as shown in the Technical Specifications for this plant.

The nuclear power transient was calculated at beginning of life, and end of life, full and zero power. The calculation included a conservative spatial weighting factor as described in Reference 1 applied only to the Doppler feedback, and a conservative choice of trip reactivity including the effect of a stuck rod adjacent to the ejected rod.

The hot spot fuel heat transfer calculation was made for this core using the FACTRAN[2] code. The increase in initial stored energy was taken into account by adjusting the code to give the same steady-state fuel temperature as predicted by a detailed design calculation for the case of densified fuel. The hot spot was assumed to occur in the region of greatest densification, which is the region with the highest fuel temperature for a given kw/ft.

The nuclear power-peaking effect was taken into account by multiplying the steady-state and transient hot channel factors (F_Q) times a peaking factor determined by a statistical study of the effect of distributed axial gaps. A factor to account for pellet stack height reduction was applied to the core average heat flux. For the full power cases a conservative initial hot channel factor F_Q of 2.75 including the densification factor was used.

Departure from nucleate boiling was assumed to occur early in the transient, and the Bishop-Sandberg-Tong correlation[3] was used to obtain the film boiling coefficient. The exothermic Zirconium-steam reaction was taken into account using the Baker-Just parabolic rate equation[4].

The basis for the calculations described above is given in WCAP-7588,[1] and is consistent with the analysis presented in the FSAR. Table 6.1 summarizes the parameters used in the analysis.

6.5.3 RESULTS

The results for the beginning of life and end of life cases are presented below. The effect of part length rods was considered in the analysis of each case.

Beginning of Life, Full Power

Bank D was conservatively assumed to be fully inserted. The worst ejected rod worth and hot channel factor was 0.27% Δk and 5.71, respectively. The peak hot spot clad average temperature was 2245°F. The peak hot spot fuel center temperature reached 4995°F.

Beginning of Life, Zero Power

For this configuration, banks D+C were assumed fully inserted. The worst ejected rod worth and hot channel factor was 0.74% Δk and 15.3 respectively. The peak hot spot clad average temperature reached only 1285°F, while the fuel center temperature reached 1920°F.

End of Life, Full Power

Again bank D was assumed fully inserted. The ejected rod worth was 0.23% Δk, and the hot channel factor was 4.84. The hot spot transient analysis gave a peak clad average temperature of 1645°F and a pellet center temperature of 4025°F.

End of Life, Zero Power

Again banks C and D were assumed fully inserted. The resulting ejected rod reactivity and hot channel factor was 0.67% Δk and 14.9 respectively. The peak clad average temperature reached 1575°F, and the peak fuel center temperature was 2380°F.

A summary of the cases presented above is given in Table 6.1. The nuclear power and hot spot fuel and clad temperature transients for BOL; HFP case are presented in Figures 6.1 and 6.2.

6.5.4 SUMMARY AND CONCLUSIONS

The cases calculated were beginning of life and end of life, full and zero power. The worst case proved to be the hot full power case at beginning of life. However, this case did not violate the limiting criteria presented in WCAP-7588.[1]

The results of this analysis show that the fuel and clad damage limits presented in WCAP-7588[1] are not exceeded. Therefore, there is no danger of sudden fuel dispersal into the coolant and no danger of consequential damage to the primary coolant loop. Fission product release (if any) will be within the guidelines of 10CFR100.

6.6 LOSS OF REACTOR COOLANT FLOW

6.6.1 LOSS OF FLOW ACCIDENTS

6.6.1.1 General

As demonstrated in the FSAR the most severe credible loss of coolant flow condition occurs upon simultaneous loss of electrical power to all reactor coolant pumps. This incident has been reanalyzed considering the effects of fuel densification on the minimum DNB ratio during the transient. These effects are as follows:

1) Increase in linear heat flux due to fuel stack height reduction.

2) Power spikes due to axial gaps between fuel pellets.

3) Increase in fuel temperatures (and stored energy) due to radial densification and thus, larger pellet clad gap.

Items 1 and 2 cause an increase in the local rod heat flux resulting in a decrease in the minimum core DNB ratio during steady state operation prior to initiation of the transient. Item 3 causes a slower decrease in rod heat flux following reactor trip, resulting in a larger change in DNB ratio during the transient.

6.6.1.2 Analysis and Results

The four pump loss of flow transient was reanalyzed using methods and assumptions consistent with those used in the FSAR with the following exceptions:

1) The core flow coastdown was altered to reflect a conservative representation of the results obtained in plant flow tests. The flow transient assumed is shown in Figure 6.3.

2) The FACTRAN[2] code was used to determine the rod heat flux decay for a range of initial linear rod powers (kw/ft) to determine the value which gives the slowest heat flux decay. This heat flux profile was normalized and applied to the core hot spot for calculation of the DNB ratio. This method is conservative since hot spot heat flux will decrease more rapidly following reactor trip resulting in a higher minimum DNB ratio.

3) Calculations of DNB ratios were made using the steady state THINC code at various time points during the transient using the instantaneous values of core inlet flow and rod heat flux as inputs. This approach is conservative with respect to minimum DNB ratio since it overpredicts fluid enthalpy, at any point in the core, as compared to a transient calculation.

   This results in the calculation of lower minimum DNB ratios than would be predicted by a transient calculation.

4) Calculations of DNB ratios during the transient were made using design basis peaking factors and methodology consistent with that discussed in Section 4.0.

The results of this analysis are shown in Figure 6.3 and Figure 6.4. As can be seen in Figure 6.4 the minimum DNB ratio during the transient does not fall below the limiting value of 1.30.

6.6.1.3 Conclusions

The analysis shows that for the most severe loss of flow transient, i.e., the four pump coastdown, the minimum core DNB ratio does not fall below the limiting value of 1.30. The other cases analyzed in the FSAR demonstrated larger margins to the limiting DNB ratio than the four pump incident (i.e., the FSAR analyses showed minimum DNBR of 1.52 for a 1/4 loss of flow vs. 1.42 for the 4/4 case; all other cases showed greater margins).

Thus we can conclude that all cases would remain above the 1.30 limit and that the conclusions as presented in the FSAR for this incident are still valid.

6.6.2 LOCKED ROTOR

6.6.2.1 General

The hypothetical locked rotor incident was evaluated in the FSAR to determine:

1) Extent of core damage, if any, due to this incident, i.e., number of fuel rods experiencing DNB and peak cladding temperature at the core hot spot.

2) Peak reactor coolant system pressure during the incident.

The effects of fuel densification on those criteria are discussed below:

Coolant System Pressure - The coolant system pressure transient is dependent primarily on core power level prior to the transient and core coolant flow reduction. Fuel densification has a minor effect on this portion of the transient response and the conclusions presented in the FSAR still hold.

Number of Rods Experiencing DNB - Fuel densification effects which contribute to an increase in the number of rods experiencing DNB during the transient are as follows:

1) Increase in fuel pellet temperature and stored energy due to radial densification and thus larger pellet-cladding gap. This effect causes a slower decrease in rod heat flux following reactor trip resulting in a smaller DNB ratio during the transient.

2) Increase in linear flux due to fuel stack height reduction. This effect increases the average rod heat flux by the percentage reduction in stack height.

3) Increase in linear flux due to power spikes caused by axial gaps between fuel rods.

Items 2 and 3 tend to decrease the minimum core DNB ratio in steady state operation prior to the transient.

Peak Cladding Temperature - The effects described above will also serve to cause an increase in peak cladding temperature during the transient. This is due to both the increased stored energy and higher local power peaking.

6.6.2.2 Analysis and Results

The four loop operation locked rotor case was analyzed assuming initial operation at 102% of rated power at BOL. The method of calculating the transient DNB ratio used the assumptions stated in the FSAR with exceptions as noted in the loss of flow analysis of the previous section. Core coolant flow was determined assuming an instantaneous seizure of a reactor coolant pump rotor with data based upon the results of the plant flow tests.

The resulting core flow transient is shown in Figure 6.5. The minimum DNB ratio was calculated to be 1.375 as shown in Figure 6.6. A clad temperature calculation was not performed since the minimum DNB ratio at the hot spot did not go below 1.30, thus DNB would not be expected to occur during the transient.

6.6.2.3 Conclusions

The benefits obtained from a reduction in the design peaking factors have more than compensated for the detrimental effects of fuel densification on the locked rotor transient. Since DNB does not occur during the transient, there will be no damage to the fuel or cladding, and hence no fission product release to the reactor coolant.

References for Section 6

1) D. H. Risher, Jr., "An Evaluation of the Rod Ejection Accident in Westinghouse Pressurized Water Reactors Using Spatial Kinetics Method," WCAP-7588, Rev. 1, December, 1971.

2) H. G. Hargrove, "FACTRAN - A FORTRAN IV Code for Thermal Transients in a UO_2 Fuel Rod," WCAP-7908, July, 1972.

3) A. A. Bishop, R. O. Sandberg, L. S. Tong, "Forced Convection Heat Transfer at High Pressure After the Critical Heat Flux," ASME 65-HT-31, 1965.

4) L. Baker, Jr. and J. C. Just, "Studies of Metal Water Reactions at High Temperature," ANL-6548, 1962.

TABLE 6.1

SUMMARY OF ROD EJECTION ANALYSIS PARAMETERS

Time in Life		Beginning	Beginning	End	End
Power Level		102%	0%	102%	0%
Ejected rod worth %Δk		.27	.74	.23	.67
Delayed neutron fraction %Δk		0.7	0.7	0.5	0.5
Feedback reactivity weighting		1.2	2.2	1.2	1.9
Trip rod shutdown %Δk		5.0	3.0	4.0	1.5
Prompt neutron lifetime	Microseconds	18	18	16	16
F_Q before rod ejection		2.75	--	2.75	--
F_Q after rod ejection		5.71	15.3	4.84	14.9
Number of operating pumps		4	2	4	2
Max, fuel pellet average temperature	°F	3840	1650	2905	2065
Max, fuel center temperature	°F	4995	1920	4025	2380
Max. clad temperature	°F	2245	1285	1645	1575

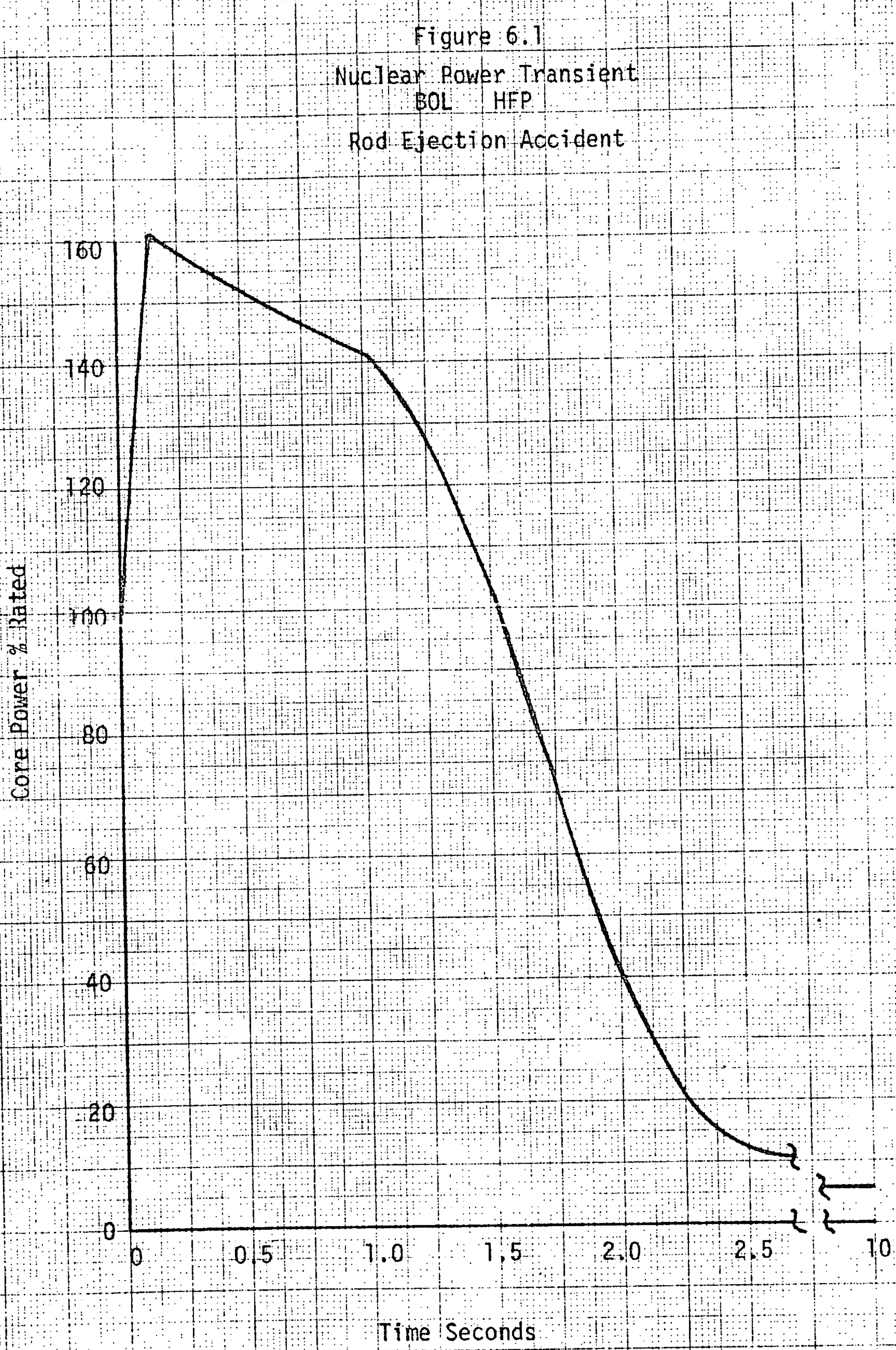


Figure 6.1
Nuclear Power Transient
BOL HFP
Rod Ejection Accident
Core Power % Rated
160
140
120
100
80
60
40
20
0
0
0.5
1.0
1.5
2.0
2.5
10
Time Seconds

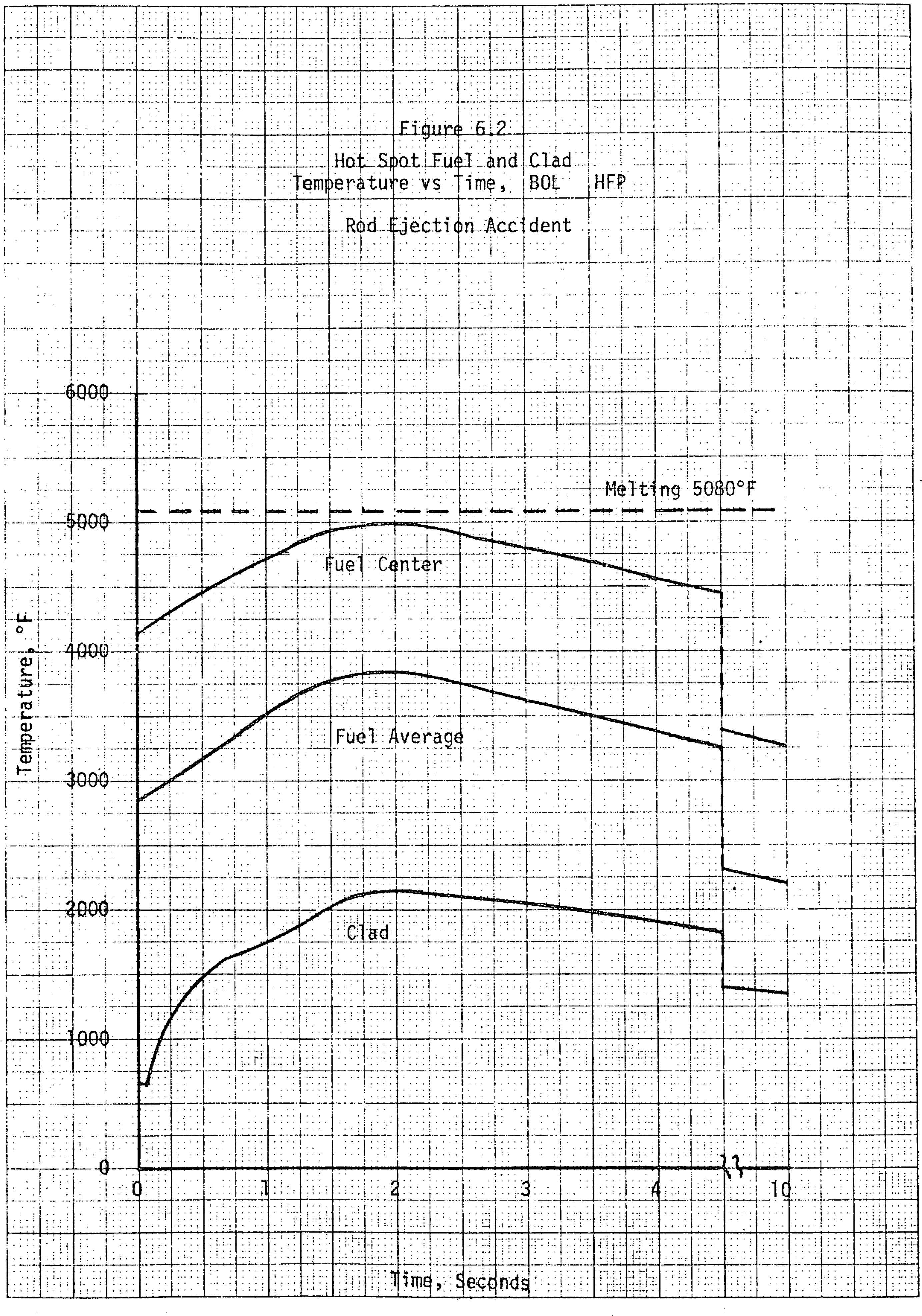
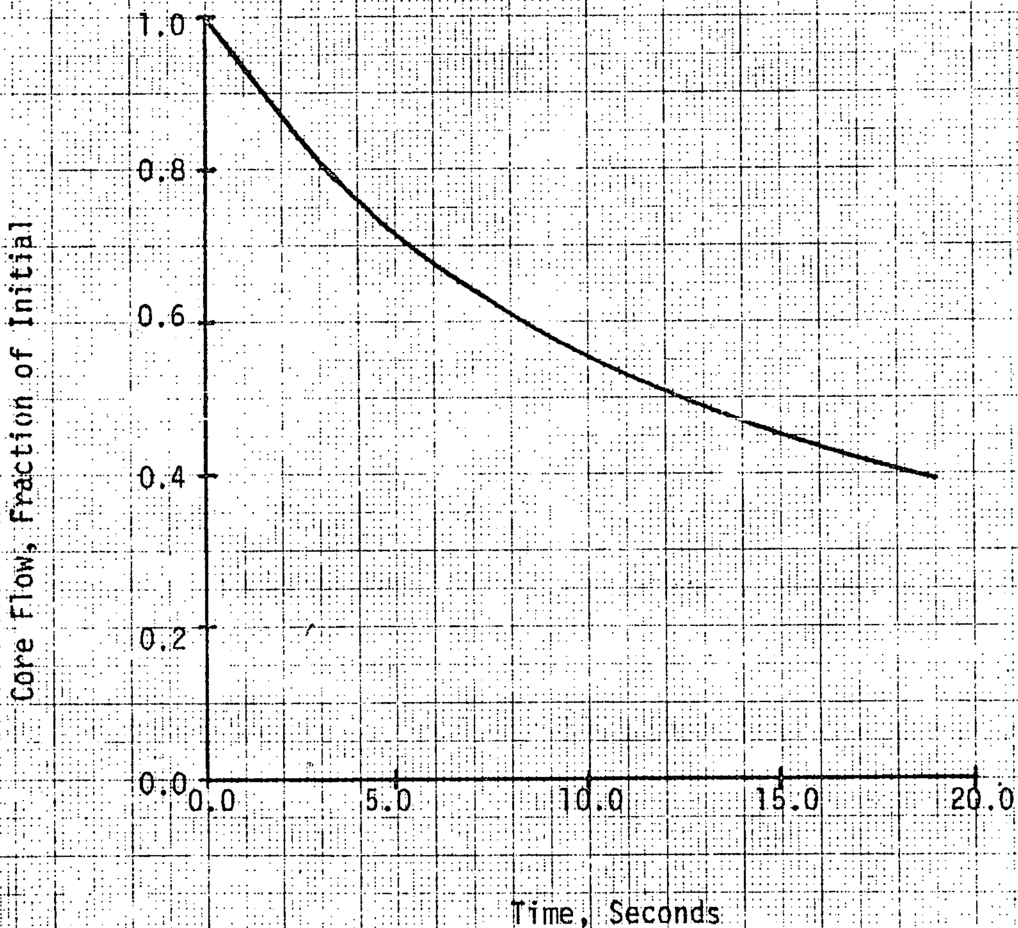


Figure 6.2
Hot Spot Fuel and Clad
Temperature vs Time, BOL HFP
Rod Ejection Accident
Melting 5080°F
Fuel Center
Fuel Average
Clad
Temperature, °F
6000
5000
4000
3000
2000
1000
0
0
1
2
3
4
10
Time, Seconds

FIGURE 6.3
LOSS OF FLOW ANALYSIS
4/4 PUMPS COASTING DOWN
CORE FLOW VS. TIME



10 X 10 TO THE CENTIMETER 46 1510

FIGURE 6.4

LOSS OF FLOW ANALYSIS

4/4 PUMPS COASTING DOWN

DNB RATIO VS. TIME

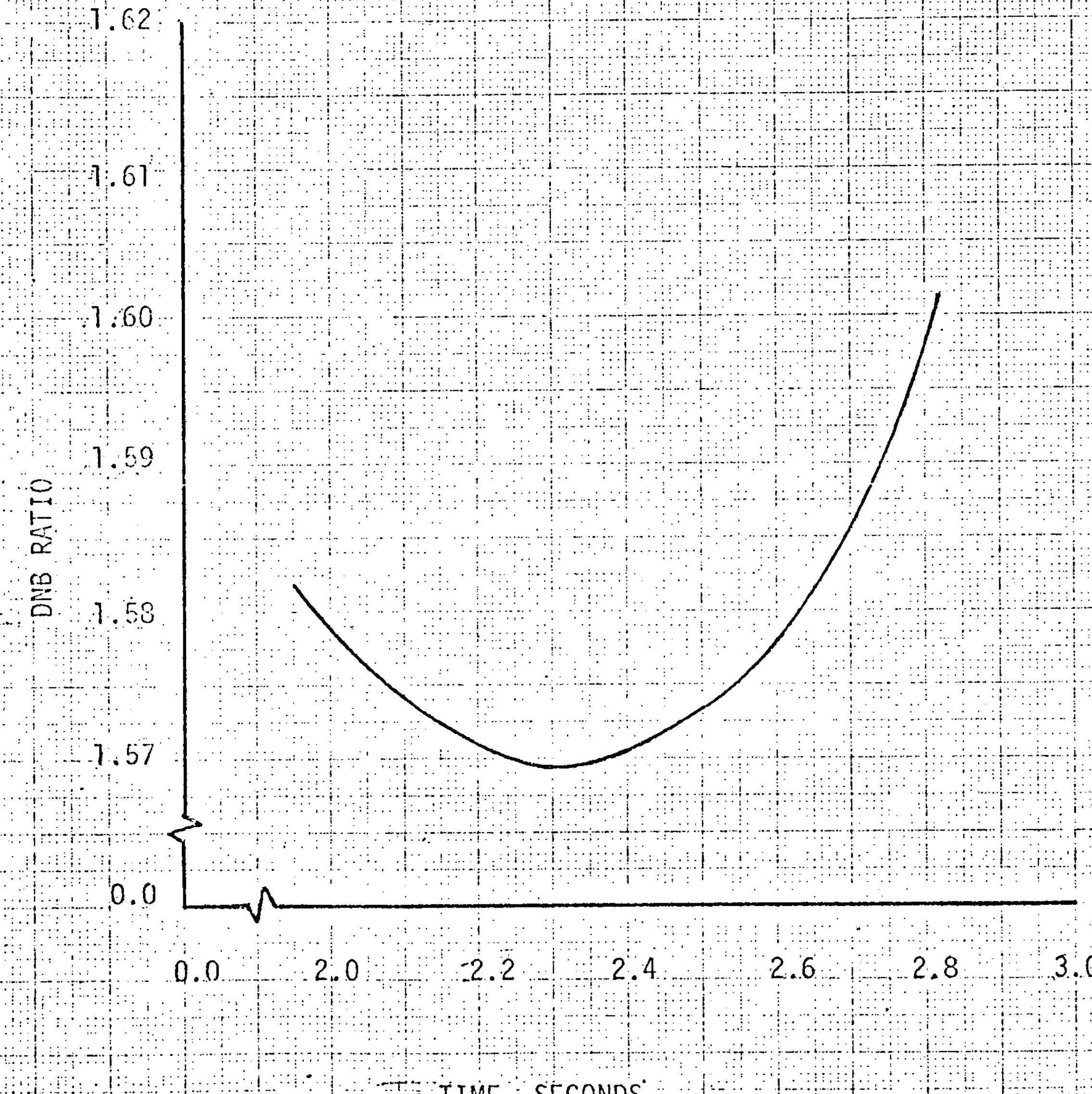


FIGURE 6.5

LOCKED ROTOR ANALYSIS

CORE FLOW VS. TIME

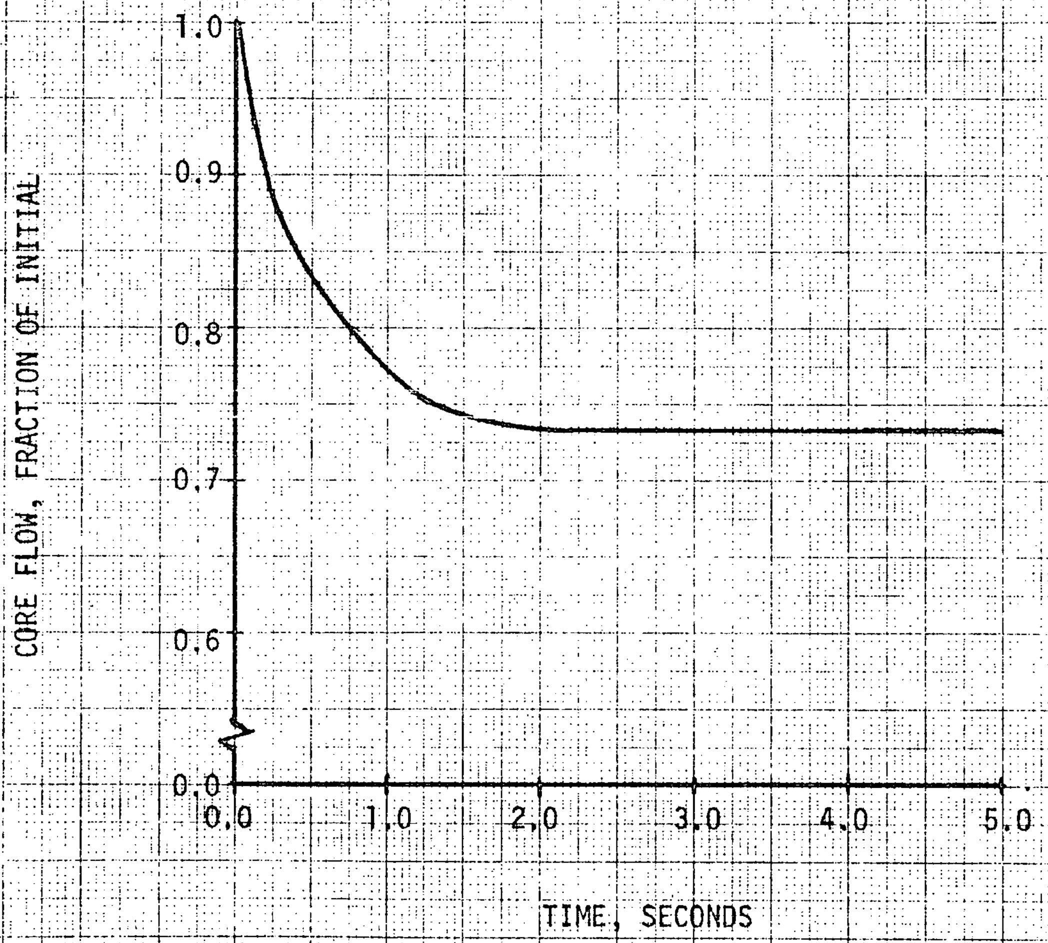
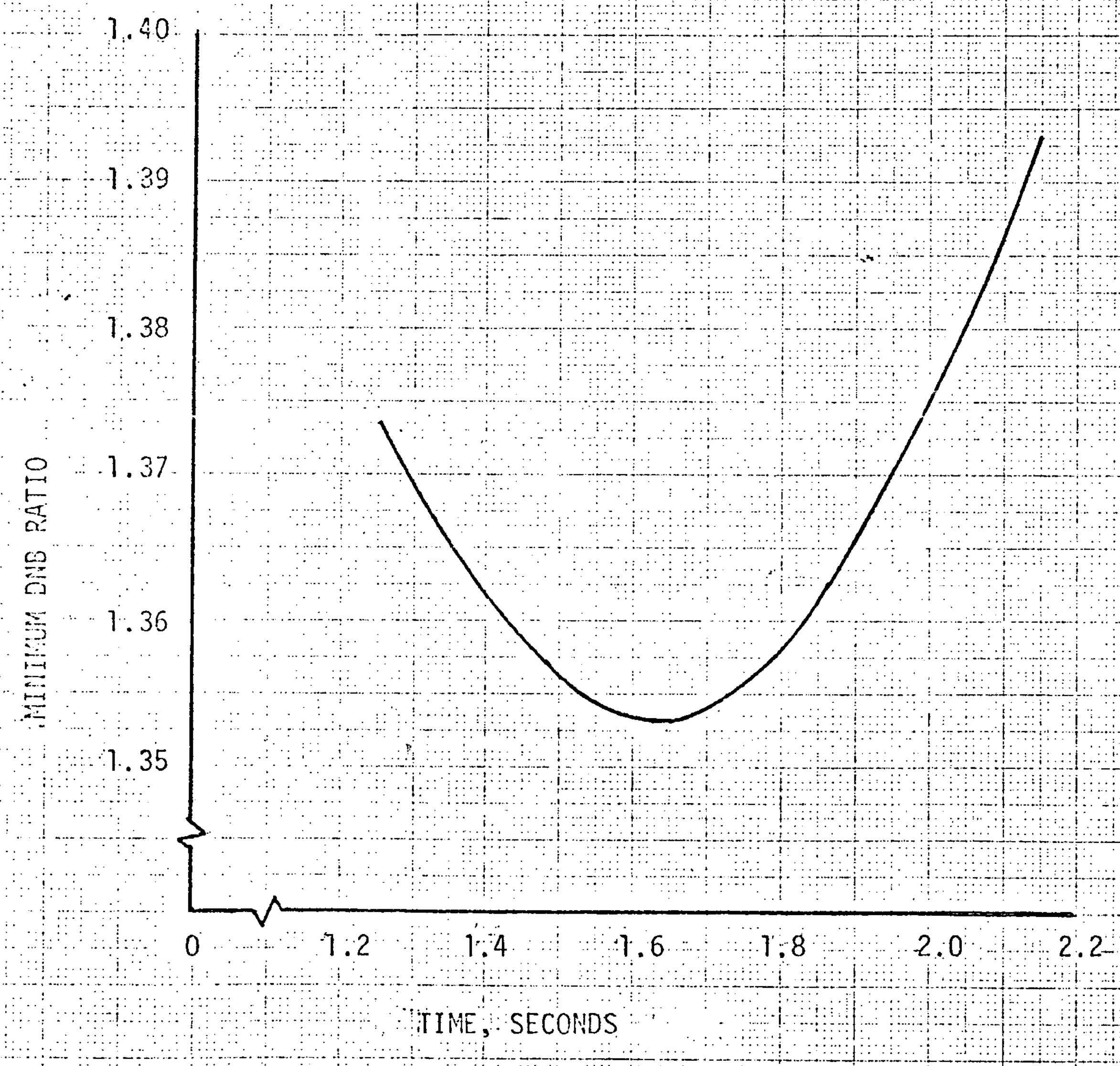


FIGURE 6.6

LOCKED ROTOR ANALYSIS

MINIMUM DNB RATIO VS. TIME



7.0 REVISIONS TO TECHNICAL SPECIFICATIONS

In order to allow for the effects of fuel densification during plant operation, the following revisions shall apply to the Technical Specifications as previously submitted. All specifications not explicitly revised shall remain as stated in the original submittal. The paragraph numbering of the original Technical Specifications are retained for convenience of referencing.

Section 2.3 LIMITING SAFETY SETTINGS, PROTECTIVE INSTRUMENTATION

Specification:

Protective instrumentation for reactor trip settings shall be as follows (referenced part of specification noted in parenthesis);

[1.B. (5)] Overpower ΔT

$$\leq \Delta T_o \left[K_4 - K_5 \frac{dT}{dt} - K_6 (T - T') - f(\Delta I)\right]$$

where

ΔT_o = Indicated ΔT at rated power

T = Average temperature, °F

T' = Indicated T_{avg} at nominal condition at rated power, 570°F

$K_4 \leq 1.19$

K_5 = Zero for decreasing average temperature

$K_5 \geq 0.188$, for increasing average temperature (sec/°F)

$K_6 \geq 0.0019$ for $T \geq T'$; $K_6 = 0$ for $T < T'$

$\frac{dT}{dt}$ = Rate of change of T_{avg}

and $f(\Delta I)$ is a function of the indicated difference between top and bottom detectors of the power-range nuclear ion chambers; with gains to be selected based on measured instrument response during plant startup tests such that:

1. For $(q_t - q_b)$ within the range between ΔI_1 and ΔI_2 given in the table below, $f(\Delta I) = 0$ (where q_t and q_b are percent power in the top and bottom halves of the core respectively, and $q_t + q_b$ is total core power in percent of rated power)

2. For each percent that $(q_t - q_b)$ is less than ΔI_1 or greater than ΔI_2, the Delta-T trip set point shall be automatically reduced by 2% of its value at rated power.

ΔI_1 and ΔI_2 are linear functions of the gain K_4. The proper limits on ΔI_1 and ΔI_2 shall be obtained from the following table which gives the allowable values corresponding to the actual value of K_4.

K_4	ΔI_1	ΔI_2
$\leq$ 1.01	$\geq$ -21	$\leq$ +16
1.04	$\geq$ -19.5	$\leq$ +14.5
1.07	$\geq$ -18	$\leq$ +13
1.10	$\geq$ -16.5	$\leq$ +11.5
1.13	$\geq$ -15	$\leq$ +10
1.16	$\geq$ -13.5	$\leq$ +8.5
1.19	$\geq$ -12	$\leq$ +7

Basis for Revision:

The $f(\Delta I)$ function in overpower and overtemperature protection system setpoints have been revised to include effects of fuel densification on core safety limits. Thy revised setpoints as given above will ensure that the safety limit of centerline fuel melt will not be reached and DNBR of 1.30 will not be violated.

Section 3.10 CONTROL ROD AND POWER DISTRIBUTION LIMITS

Specification:

The referenced portion of the previous specification is noted in parenthesis.

(3.10.1) Control Rod Insertion Limits

(3.10.1.5) The part length rods shall not be more than 70% inserted.

(3.10.2) Power Distribution Limits and Misaligned Control Rod

(3.10.2.1) (Change 50% to 75%)

(3.10.2.2-b) The hot channel factors shall be determined and maximum allowable power shall be reduced one percent for each percent the hot channel factors exceed the design values of:

$F_Q^N \leq 2.62\ [1 + 0.2(1-P)]$ in the indicated flux difference range of +7 to -12 percent

$F_{\Delta H}^N \leq 1.65\ [1 + 0.2(1-P)]$

where P is the fraction of full power at which the core is operating.

For every percent outside of the indicated flux difference range +7 to -12 percent, the allowed F_Q^N may be increased above 2.62 by two percent.

The measured values, with due allowance for measurement error, must be corrected by including a penalty as shown on Figure 3.10-4 (at the approximate core location) to account for fuel densification effects before comparison with the limiting values above.

(3.10.2.6) Except during physics tests, the following power distribution restrictions must be maintained:

a. At rated power, the indicated axial flux difference must be maintained within +7 percent and -12 percent.

b. If, at rated power, the indicated axial flux difference exceeds the permissible range defined above for a period of more than eight hours, the situation shall be corrected or the reactor power shall be reduced 2 percent, for each percent the flux difference exceeds the permissible range.

c. For every 2 percent below full power, the permissible flux difference range is extended by 1 percent.

Basis for Revision:

Part length rod insertion has been limited to eliminate certain adverse power shapes.

Two criteria have been chosen as a design basis for fuel performance related to fission gas release, pellet temperature and cladding mechanical properties. First, the peak value of linear power density must not exceed 21.1 kw/ft. Second, the minimum DNBR in the core must not be less than 1.30 in normal operation or in short term transients.

In addition to the above, the initial steady state conditions for the peak linear power for a loss of coolant accident must not exceed the values assumed in the accident evaluation. This limit is required in order for the maximum clad temperature to remain below that established by the Interim Policy Statement for LOCA. To aid in specifying the limits on power distribution the following hot channel factors are defined.

F_Q, Heat Flux Hot Channel Factor, is defined as the maximum local heat flux on the surface of a fuel rod divided by the average fuel rod heat flux, allowing for manufacturing tolerances on fuel pellets and rods.

F_Q^N, Nuclear Heat Flux Hot Channel Factor is defined as the maximum local fuel rod linear power density divided by the average fuel rod linear power density, assuming nominal fuel pellet and rod dimensions.

F_Q^E, Engineering Heat Flux Hot Channel Factor is defined as the ratio between F_Q, and F_Q^N and is the allowance on heat flux required for manufacturing tolerances.

$F_{\Delta H}^N$, nuclear Enghalpy Rise Hot Channel Factor, is defined as the ratio of the integral of linear power along the rod on which minimum DNBR occurs to the average rod power.

It should be noted that $F_{\Delta H}^N$ is based on an integral and is used as such in the DNB calculations. Local heat fluxes are obtained by using hot channel and adjacent channel explicit power shapes which take into account variations in horizontal (x-y) power shapes throughout the core. Thus the horizontal power shape at the point of maximum heat flux is not necessarily directly related to $F_{\Delta H}^N$.

It has been determined by analysis that the design limits on peak local power density on minimum DNBR at full power and LOCA are met, provided:

$$F_Q^N \leq 2.62 \text{ and } F_{\Delta H}^N \leq 1.65$$

These qualities are measurable although there is not normally a requirement to do so. Instead it has been determined that, provided certain conditions are observed, the above hot channel factor limits will be met; these full power conditions are as follows.

1. Control rods in a single bank move together with no individual rod insertion differing by more than 15 inches from the bank demand position.

2. Control rod banks are sequenced with overlapping banks as described in Technical Specification 3.10-1.

3. The control bank insertion limits are not violated.

4. Axial power distribution guide lines, which are given in terms of flux difference control, are observed. Flux difference refers to the difference in signals between the top and bottom halves of two-section excore neutron detectors. The flux difference is a measure of the axial offset which is defined as the difference in power between the top and bottom halves of the core. Calculation of core average axial peaking factors have been correlated with axial offset. The correlation shows that an F_Q^N of 2.62 and allowed DNB shapes, including the effects of fuel densification, are not exceeded if the axial offset (flux difference) is maintained between -15 and +10 percent.

For operation at the fraction, P, of full power the design limits are met, provided,

$F_Q^N \leq 2.62\ [1 + 0.2\ (1-P)]$ in the indicated flux difference range range of +7 to -12 percent.

and

$F_{\Delta H}^N \leq 1.65\ [1 + .2\ (1-P)]$

For every percent outside of the indicated flux difference range +7 to -12 percent, the allowed F_Q^N may be increased above 2.62 by two percent.

The permitted relaxation of F_Q^N and $F_{\Delta H}^N$ allows radial power shape changes with rod insertion to the insertion limits. The allowed increase in F_Q^N for large flux differences is consistent with power shapes assumed in setting the overpower and overtemperature ΔT setpoints. It has been determined that provided the above conditions 1 through 4 are observed, these hot channel factors limits are met.

For normal operation and anticipated transients the core is protected from exceeding 21.1 KW/ft locally, and from going below a minimum DNBR of 1.30, by automatic protection on power, flux difference, pressure and temperature. Only condition 1 through 3, above, are mandatory since the flux difference is an explicit input to the protection system.

Measurements of the hot channel factors are required as part of start-up physics tests and whenever abnormal power distribution conditions require a reduction of core power to a level based on measured hot channel factors.

In the specified limit of F_Q^N there is a 5 percent allowance for uncertainties[1] which means that normal operation of the core within the defined conditions and procedures is expected to result in $F_Q^N \leq 2.62/1.05$ even on a worst case basis. When a measurement is taken experimental error must be allowed for and 5 percent is the appropriate allowance for a full core map taken with the moveable incore detector flux mapping system.

In the specified limit of $F_{\Delta H}^N$ there is a 10 percent allowance for uncertainties[1] which means that normal operation of the core is expected to result in $F_{\Delta H}^N \leq 1.65/1.10$. The logic behind the larger uncertainty in this case is that (a) abnormal perturbations in the radial power shape (e.g. rod misalignment) affect $F_{\Delta H}^N$ and (b) the operator has a direct influence on F_Q^N, through movement of part length rods, and can limit it to the desired value, he has no direct control over $F_{\Delta H}^N$ and (c) an error in the predictions for radial power shape, which may be detected during startup physics tests can be compensated for in F_Q^N by tighter axial control, but compensation for $F_{\Delta H}^N$ is less readily available. Five percent is the appropriate allowance for a full core map taken with the movable in-core detector flux mapping system.

Section 5.3 REACTOR

Applicability

Applies to the reactor core, reactor coolant system, and emergency core cooling systems.

Objective

To define those design features which are essential in providing for safe system operations.

A. Reactor Core

1. The reactor core contains approximately 87 metric tons of uranium in the form of slightly enriched uranium dioxide pellets. The pellets are encapsulated in Zircaloy-4 tubing to form fuel rods. The reactor core is made up of 193 fuel assemblies. Each fuel assembly contains 204 fuel rods.[1]

2. The average enrichment of the initial core is a nominal 2.8 weight per cent of U-235. Three fuel enrichments are used in the initial core. The highest enrichment is a nominal 3.3 weight per cent of U-235.[2]

3. Reload fuel will be similar in design to the initial core. The enrichment of reload fuel will be no more than 3.4 weight per cent of U-235.

4. Burnable poison rods are incorporated in the initial core. There are 1412 poison rods in the form of 7,8,9,12,16 and 20-rod clusters, which are located in vacant rod cluster control guide tubes.[3] The burnable poison rods consist of borated pyrex glass clad with stainless steel.[4]

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6