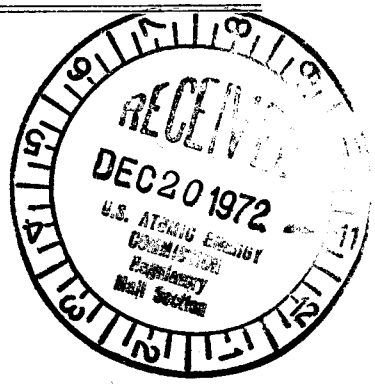
UNITED STATES ATOMIC ENERGY COMMISSION



IN THE MATTER OF:

CONSOLIDATED EDISON COMPANY
OF NEW YORK, INC.

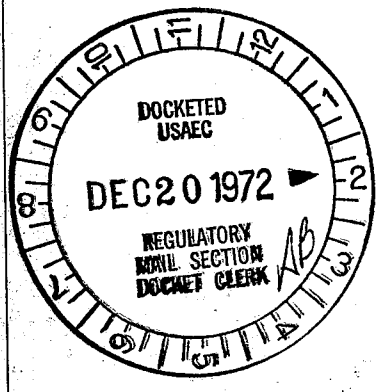
(Indian Point Station, Unit No. 2)

Docket No. 50-247

Place - Washington, D. C.

Date - Tuesday, 12 December 1972

Pages 7039 - 7245



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UNITED STATES OF AMERICA

ATOMIC ENERGY COMMISSION

In the matter of:	
CONSOLIDATED EDISON COMPANY OF NEW YORK, INC.	Docket No. 50-247
(Indian Point Station, Unit No. 2)	

Tariff Commission
Third Floor, 8th and E Streets, N. W.
Washington, D. C.

Tuesday, 12 December 1972

The above-entitled matter came on for further hearing, pursuant to adjournment, at 9:30 a.m.

BEFORE:

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

DR. JOHN C. GEYER, Member.

MR. R. B. BRIGGS, Member.

APPEARANCES:

LEONARD M. TROSTEN, Esq. and EDWARD L. COHEN, Esq., 1821 Jefferson Place, N. W., Washington, D. C., 20036; on behalf of the Applicant.

MYRON KARMAN, Esq. and EDWARD LYLE, Esq., Office of General Counsel, United States Atomic Energy Commission, Bethesda, Maryland; on behalf of the AEC Regulatory Staff.

APPEARANCES (Continued):

BRUCE L. MARTIN, Esq., 112 State Street, Albany, New York; on behalf of the Atomic Energy Council of the State of New York.

ANGUS MACBETH, Esq., Finney Farm, Croton-on-Hudson, New York; on behalf of the Intervenor, Hudson River Fishermen's Association.

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WITNESS:	DIRECT	CROSS	REDIRECT	RECROSS
Gerald Lauer		7046		
John P. Lawler		7046		

EXHIBITS

NUMBER	FOR IDENTIFICATION	IN EVIDENCE
Hudson River Fishermen's Association:		
I	7067	7067
II	7107	7107
III	7178	7179
IV	7202	7203

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PROCEEDINGS

CHAIRMAN JENSCH: Please come to order.

It is the recollection of the Board that this session of our evidentiary hearings will be cross-examination of the Applicant's witnesses by the Hudson River Fishermen's Association. Is that correct?

MR. MACBETH: That's correct.

CHAIRMAN JENSCH: Are you ready to proceed, Mr. Macbeth?

MR. MACBETH: I am, Mr. Chairman.

MR. TROSTEN: Mr. Chairman, may I make a few preliminary observations?

CHAIRMAN JENSCH: Yes.

MR. TROSTEN: First I have distributed to the parties this morning -- may I try it without a mike -- I have distributed to the parties this morning a document entitled Applicant's Listing of Items from the Hearing Sessions held on December 4, 1972, which are to be submitted by the Regulatory Staff.

With the Board's permission, I would ask that this be included in the transcript merely for reference purposes so that we would have a record of the open items, if you will, to be submitted by the Regulatory Staff, and I would ask that the Regulatory Staff submit these prior to the resumption of cross-examination of Dr. Goodyear.

December 12, 1972

Consolidated Edison Company of New York, Inc.
Indian Point Station, Unit No. 2
AEC Docket No. 50-247

APPLICANT'S LISTING OF ITEMS FROM THE HEARING SESSIONS HELD ON DECEMBER 4-7, 1972 WHICH ARE TO BE SUBMITTED BY THE REGULATORY STAFF

	Item	Page	Due From
1.	List of references answering principal questions concerning reduction of recruitment due to entrainment.	6522	Goodyear
2.	Document to be provided on Hudson River shad relative to control of populations by density independent mechanisms.	6669	Goodyear
3.	Estimate of number of Hudson River spawned striped bass caught each year by commercial fishermen.	6671	Goodyear
4.	Additional assumptions important to Staff conclusion that effect of plant operation for even a short period is expected to reduce future Mid-Atlantic landings in the neighborhood of 30-50%.	6705	Goodyear
5.	Review of pages 6515 to 6525 to determine if modification is needed in light of answer given on page 6713.	6713	Goodyear
6.	Estimate of total population of striped bass in the Chesapeake Bay.	6761-62	Goodyear

	Item	Page	Due From
7.	References to additional tagging studies relied upon by Staff.	6772, 6774	Goodyear
8.	Response to question stated on page 6801.	6801-02, 6824	Goodyear
9.	Response to question on parallel trends in fishing effort for Hudson River and Atlantic waters of New York State.	6850	Goodyear
10.	Set forth the factors which caused the Staff to change conclusions in the Draft Detailed Statement.	7020	Knighton

CHAIRMAN JENSCH: Is there any objection to that inclusion in the transcript?

MR. KARMAN: We have no objection, Mr. Chairman. Dr. Goodyear is looking over that list now to see whether we have everything with us at the moment.

CHAIRMAN JENSCH: Any objection?

Hearing no objection, the request of the Applicant's counsel is granted, and the statement of Applicant's listing of items as identified by Applicant's counsel may be physically incorporated in the transcript at this place.

(The document follows.)

MR. TROSTEN: Mr. Chairman, I would also like to note that we have received from counsel for the Intervenors a document entitled Proposed Subjects for Cross-Examination. This document was submitted to us in accordance with the agreement among the parties. Yesterday I received from Mr. Karman a list of several subject areas on which the Regulatory Staff desired to cross-examine Dr. Lauer. I have not received any other listing of subject areas on which cross-examination by the Regulatory Staff or any other party is desired as of this time.

We are prepared --

MR. MACBETH: With the exception of the rather complete list from me?

MR. TROSTEN: The one I mentioned previously, yes, of course. Thank you.

Mr. Chairman, we are prepared for cross-examination this morning of our witnesses in accordance with my conversation with Mr. Macbeth. We propose that Dr. Lauer be cross-examined first, followed by Dr. Lawler. Since the testimony of these two witnesses is closely interrelated, I would propose that they sit here together in case there is one question that should more properly be referred to another, this could be done without any loss of time.

I have also agreed with Mr. Macbeth that the cross-examination will proceed following conclusion of the cross-

examination of Dr. Lawler to the cross-examination of Dr. Raney, Dr. McFadden, and Mr. Newman. It may be that Mr. Macbeth will also wish to cross-examine Mr. Woodbury. I rather suspect he will, judging by the list of the areas in which he tentatively says he wishes to cross-examine, particularly the reference to Con Edison's Appendix G, the details of the proposed research program.

So we will have Mr. Woodbury available here, and any other witness who is appropriate for such cross-examination.

I have also agreed with Mr. Karman that cross-examination of each witness will be begun and concluded so that Mr. Macbeth will cross-examine and then the Regulatory Staff will cross-examine, and then it will be possible to excuse that witness so that people do not have to stay throughout the entire hearing in order for cross-examination to resume.

CHAIRMAN JENSCH: Yes. That would appear a very feasible procedure. The Board would just remind the attorneys that the Board will expect the attorneys to keep informed among themselves as to the anticipated length of cross-examination so succeeding witnesses will be available without delay.

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: With that preface, then, Dr.

Lauer and Dr. Lawler have resumed the witness stand.

Whereupon,

GERALD LAUER

and

JOHN P. LAWLER

resumed the stand as witnesses on behalf of the Applicant and, having been previously duly sworn, were examined and testified further as follows:

CROSS-EXAMINATION

BY MR. MACBETH:

Q I would like to begin with Dr. Lauer's testimony of October 30th. The experiments were conducted to determine maximum safe temperature for striped bass eggs and larvae. It appears in the text of Dr. Lauer's testimony at about page 42.

Dr. Lauer, would you give a description of the experimental apparatus and the conditions under which the experiment was conducted?

A (Dr. Lauer) Okay. As indicated in the text, these experiments were carried out at Moncks Corner, South Carolina Hatchery.

MR. KARMAN: I can't hear you, Doctor.

WITNESS LAUER: These experiments were carried out at Moncks Corner, South Carolina Hatchery. Primarily for the reason that it was found to be virtually impossible or was

anticipated that it was going to be virtually impossible to get a sufficient supply of eggs and larvae in good condition for all of the developmental stages by collecting them from the Hudson River with nets.

It was uncertain as to whether or not they could be gotten with -- by the process of trying to get males and females and spawning --

(Discussion off the record.)

CHAIRMAN JENSCH: Will you start again? You started at the North Carolina Hatchery for some reason.

WITNESS LAUER: In the first place we anticipated having difficulty getting a sufficient supply of the various developmental stages of the eggs and larvae to carry out these types of experiments, and we also had uncertainties as to whether or not we could obtain these developmental stages by way of collecting males and female fish from the Hudson and getting them to spawn at appropriate times in order to get these developmental stages.

So we went to Moncks Corner, South Carolina to the hatchery down there where they had stocks of striped bass eggs and larvae coming off at different developmental stages, and had previously developed an experimental device similar to what ~~Mahirsky~~ Mahursky has used and has reported on in the literature which involves a metal block approximately four feet long, six feet wide, six inches wide and four inches deep in which holes

were bored in a series of four holes in line with each other, across the length of this metal block, 17 rows of these along the length of the block, and each end of the block fitted with a circulating water device connected to controlled temperature appartus so one end of the block could be made cooler and the other end of the block made warmer, and in effect then this provided a temperature gradient over the length of the block.

The gradient of that could be controlled by adjusting temperatures as desired at each end of the block. The block was insulated to inhibit heat exchange to the outside to maintain stability. This provided the possibilities of exposing a given life stage of organisms to an array of 17 different temperatures, individual temperatures simultaneously, four replicates for each temperature, or other experimental designs could be used like one could expose a developmental stage -- and this was done -- of the eggs and larvae to the range of temperatures in the block that pull an individual sample of larvae out of each of the four whole replicates at different time intervals so one could get different times of exposures at the same temperature using the same array of eggs and larvae that had been introduced into the block simply by removing them at the desired time.

This provided an opportunity to get a replication of a large number of experiments involving each of the life

stages, developmental stages of the eggs and larvae over a substantial range of temperatures.

The eggs and larvae were -- in order to be exposed to these temperatures in this block -- by the way, each of the holes in the block were lined with an inert plastic liner which then had water inside of it to keep the water from being in contact with the metal so the chambers for each lining had this plastic tube. And then they were filled with water and the water came to equilibrium in temperature with the block at each of these parallel rows of holes down the length of the block.

To introduce the specimens to the block for the temperature exposures we had a similar type of a gridded metal plate with all of the holes bored exactly for the same positions as the holes were bored for the block so that -- and this was mounted in an ambient temperature water bath. Each hole was filled with a plastic tube with a porous net bottom on it to allow for water exchange and each one of these tubes was then innoculated, if you will, or had on the order of 25 to 50 eggs and/or larvae, depending upon the size of the larvae.

Into each one of these tubes, the whole thing was loaded with these tubes with the test organisms intact and then the whole rack could simultaneously be lifted up, transferred over to the thermal block, and all simultaneously

immersed into the holes of the thermal block. All of the organisms had in it the same initial starting point in terms of exposure to the temperature, and this was essentially an instantaneous temperature increase, perhaps somewhat more abrupt than what they would experience going through the power plant because it was an instantaneous thing. There was no water being transferred with these organisms except the film of water that existed on the net or around the specimens themselves.

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BY MR. MACBETH:

Q I take it from your description that there were four organisms in each cell of the block in the sense that a cell would be restricted to 1 delta T increase of temperature?

A There were four cells with similar delta Ts in temperature in any given row along the length of this block. In each one of those cells, ordinarily had 25 test organisms in them except when the larvae were of larger size and we went to fewer numbers, usually 10 to avoid crowding.

Q And you said there were 17 intervals of temperature. What range of degrees Fahrenheit did that cover?

A Well, it could cover pretty much any range of temperatures desired just by regulating the two thermal control units at each end of the block and the gradations could be made greater or smaller depending upon how the temperatures at each end of the block were adjusted.

But the range of temperatures over the block normally would have included a range of about 15 to 20 degrees depending upon how each of the mperatures at each end were manipulated.

These things were set up such that we tried to have the median temperature of the block or the holes in the middle part of the block representative of the temperatures

that would be expected to occur at the time that these developmental stages would occur in the river, and then, of course, on either side of this we had temperatures lower on the one side and higher on the other than those temperatures would be expected to be.

So in general the tempeature range amounted to 15 to 20 degrees over the length of the block.

MR. TROSTEN: Dr. Lauer, if you feel that it would be helpful, we have an easel in the room now and you could draw these things if you think it would help to clarify what you are saying.

WITNESS LAUER: Okay. Thank you.

BY MR. MACBETH:

Q How long were the specimens kept for observation?

A (Dr. Lauer) You mean after the experiment was over?

Q Yes, sir.

A For variable periods of time. The eggs, of course, had to be held. One can't tell whether or not the eggs are just outright dead or not based upon their appearance. They turn opaque if they are dead, but in order to determine their subsequent successful hatching, the eggs had to be held until they hatched to see if the larvae hatched out normally without any deformities or abnormalities.

So it can be said that depending upon what stage of egg we are talking about, if there was a fertilization stage

they were held for 48 hours until the point of hatching and then subsequently for a period of up to 24 to 48 hours to look at the larvae themselves to be sure they were swimming normally and apparently behaving normally.

On the other hand, if we are talking about having tested say a 24-hour egg which was by then in the ~~gastrular~~ gastrula stage obviously that would have been held only 24 additional hours before hatching.

The time they were held varied upon the stage that they were in at the time they were exposed to the temperature. There is no absolute time because of that. We were looking at all these different developmental stages.

Q Would the eggs that you held for hatching, were there any distinctions made after hatching between those that were alive and swimming normally and those that may have been alive and showing abnormal behavior?

A There were, and the safe temperatures represented on this graph represent --

Q By the graph you mean figure 17?

A Figure 17.

-- represent temperature exposures which were not found to cause either any increased mortality or abnormal developments. At higher temperature ranges -- now -- well, either one of those things could be the limiting factor causing the positioning of a given point. We did expose eggs

to temperatures which did cause subsequent abnormal development wherein the larvae either had a crooked spine or swam erratically or otherwise were abnormal in their behavior.

This line on figure 17, both lines on figure 17 having to do with the different stages of egg development, represent points which -- of temperature exposure which did not either cause increased mortalities or increased abnormalities of development compared to controls.

Q In other words, let's look at figure 17 and take the last gastular stage.

With the 60-minute exposure, the -- there were no more organisms in the experimental group than in the control which had any abnormality above 85 degrees?

A I think I know what you mean. I am not sure I would have said it the same way. I will restate it the way I would state it and that is that at approximately this temperature of 85 degree exposure, we did not see either any increased mortalities or abnormal developments compared to controls that had not been exposed to temperature elevation above their ambient culture temperature. I think that is essentially what you said, but that is the way I would say it.

Q Could these various stages -- what percentage of the organisms in control show abnormality or death?

A Generally few to zero, almost never did they experience abnormalities or death in the short time frame

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when one looks at the -- you know, looked at the immediate effects.

Withholding of these things until hatching stage, the controls did exhibit a minor amount of mortality during the rest of the developmental stages, most of which occurred right at the point of hatching. This is a period of transition obviously and this is at one point in which some mortalities were observed.

As far as the exact percentage that existed in the controls, I don't have that information with me, but these points were taken and located on this curve as being those that represented no increased mortalities or abnormal developments compared to the controls and the experience in the controls is minimal. That is as far as I could go with that at the moment.

Q In the period when these organisms would be present in the Hudson River, the temperature of the Hudson River would vary between about 53 and 63 degrees Fahrenheit, would it not?

A Are you talking about the eggs?

Q This time period shown on figure 17. In other words, the time when the eggs of young larvae would be present in the river in the vicinity of Indian Point?

A Well, I think that would be true for the eggs and the early larvae. As far as the older larvae are concerned,

I think the ambient temperatures would be somewhat higher than 63 by the time the larvae got to the stage that the older larvae represented in figure 17 are concerned. That would occur somewhat higher than the ambient of 63 which is what you quoted.

Q Did you take any eggs or larvae at ambient temperatures or acclimation temperatures between 53 and 63 degrees and do any experiments of this nature on those organisms?

A No. As is indicated in my testimony, the temperatures that existed in their water system at the hatchery were virtually constantly 67 degrees Fahrenheit, so this is what there was to work with. There was no temperature that we had available to us lower or higher than that. They were using well water and it was a virtually constant temperature at 67 degrees. It was for that reason that in the interests of being conservative in estimating the temperature tolerance, maximum temperature tolerance, that we chose to at this point not having the opportunity to look at the organisms coming from 53 to 63 degrees range, that we used a 60-minute exposure time to help to compensate for the fact that we were starting from a higher ambient temperature in the first place.

The 60-minute exposure time being on the order of six times as long that -- than that experienced by an organism

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transported through the Unit 2 and Unit 1 cooling water systems when they are operating in combination. We used the 60-minute exposure time which would tend to reduce the temperature tolerance compared to a 10-minute exposure time in order to compensate for the fact that we had to start at a higher ambient temperature which ~~mgith~~ might have the effect of causing a higher estimated maximum tolerable temperature in the experiments.

Q Well, obviously there can be some dispute as to whether it is the absolute temperature that has the -- has an effect on these organisms or whether it is the thermal shock, the quick increase of 15 degrees.

Did you make any tabulations of what happened to these organisms -- to the organisms that were heated 15 degrees after 10 or 20 minutes?

A Yes, we did and that would be, for example, where we had an ambient temperature for these organisms at 67, adding 15 degrees to that would give you an 83 -- if my arithmetic in my head is correct -- and that 15 degrees over 67 obviously was a temperature which caused increased mortalities and abnormalities because the maximum safe temperature at that time was about 79 degrees, looking at the very early stages of egg after fertilization from figure 17.

However, it was also our experience from doing these experiments that involved something on the order of

50,000 speciments all told to produce this figure and there are some longer exposure times that we don't have the data worked up for yet that could be added to this -- it was our experience that it was the maximum temperature that was -- that seemed to be most important rather than the delta T involved, and if the delta T exceeded the maximum temperature, then you got an effect. If it did not, then you didn't. But the larvae themselves had a response that indicated that it was a maximum temperature for each of the developmental stages that was pretty much fixed and the delta T was of lesser importance unless it exceeded that maximum temperature, of course.

Q Do you have the tabulation of the other results of the 10 to 20-minute exposure to a delta T of 15?

A I don't have that here. We did considerably less experimentation for the 10 to 15-minute exposure times than for the 60-minute for the reason I just described and that was that we knew the ambient temperatures here --

Q I really just want to know if you have that date. I would like to see it.

A No, I don't have it. That is being worked up. This whole section is being worked up in the interests of producing a complete picture of the whole area. I don't have those data with me.

Q If those data become available before the end of

this proceeding, could they be provided to me?

MR. TROSTEN: Yes.

BY MR. MACBETH:

Q Let me turn to the pressure experiments.

Did you give -- do any experiments at negative pressures, pressures less than atmospheric?

A Yes, we did pressure experiments over a range of pressures from 7 pounds per square inch up to a hundred pounds per square inch, 7 pounds per square inch would be approximately half of atmospheric.

Q All right.

And did the negative pressures, the less than atmospheric pressures, go down as low as the pressure levels that would be experienced on an organism in the plant?

A We tried to gear it to approximately that, but I think maybe Dr. Lawler might want to comment on it. He has done a more detailed analysis of pressures going through the plant than I have. We did gear the experiement to what we were advised was the low pressures that might be experienced by organisms going through. I think it is approximately that.

Q Perhaps we could have a comment from Dr. Lawler.

What pressures would be experienced by an organism passing through the intake pumps in the water box?

End #2

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MR. TROSTEN: Dr. Lawler, is this information not contained in your testimony concerning the effects of dissolved oxygen -- on dissolved oxygen of Indian Point plant operations?

WITNESS LAWLER: That's right.

BY MR. MACBETH:

Q Does Dr. Lawler have it on the top of his head?

MR. TROSTEN: Do you want us to find the document and read this off, Mr. Macbeth?

MR. MACBETH: Yes, I think that would be helpful.

MR. TROSTEN: All right. We will do that.

May we take a brief recess, Mr. Chairman, while we look for this data? Or perhaps Mr. Macbeth would want to go on and we will find it.

MR. MACBETH: I could go on to another topic and pick this up later.

CHAIRMAN JENSCH: All right, proceed.

BY MR. MACBETH:

Q I would like to turn to the 1971 data on the position of the water column in which striped bass, larvae, eggs were to be found. Am I right in thinking that that is the same data that Dr. Lawler relies on in his testimony in the following page, 52?

A (Dr. Lauer) I think in part.

Q He refers there to 1971 NYU data. He is referring

to the same set of experiments that you described at page 42 in the following graphs?

A With respect to NYU data, I think that's correct. He also used other data. That's why I was qualifying the answer.

Q Yes. Do you have a complete tabulation of those tows on a station-by-station basis?

A I am not sure that I understand what you mean by a complete tabulation. You mean when we took them, where we took them and so forth?

Q Yes. For instance, I have been shown tabulations of the number of striped bass larvae collected in the mid-depths during the day, broken out for each day in which such a tow is made and broken out station by station, seven columns going from A to G, following the stations on the various charts that appear both in your testimony and Dr. Lawler's testimony. Do you have such tabulations for all of the stations at all of the various sampling times and places?

In other words, night surface, night mid-depth, night bottom?

A Yes, we do. We make up a data sheet on each individual sample as it is taken relative to the location in the river, the depth at which it is taken, as close as we can estimate that, the time during which it is taken, and the temperature and oxygen conditions that exist at the time that

the sample is taken. So there is a sheet made up on each one of these so that I guess over a season's period that would mean we would have on the order of 1500, 2000 such sheets, because approximately the number of samples that are taken.

Q Well, I have been given sheets that -- perhaps I should show them to you and be able to clear up what I am speaking of.

(Handing document to witness.)

I have been shown sheets of that sort for a number of the different modes of collection, but not for all of them. Are sheets of that sort a tabulation of the -- perhaps a collection of the data for the -- from the numerous sheets that you have?

A Okay.

MR. TROSTEN: Dr. Lauer, would you identify this document for the record, please?

WITNESS LAUER: Okay. This is a document that is -- well, it is a compilation of tables indicating the abundance of striped bass and white perch larvae relative to the time they were collected for each of the seven individual stations that we have sampled, for each of the dates on which samples were taken through the season of occurrence for both the striped bass and the white perch.

They also have included with them the statistical analyses of those data on the bottom of each page.

MR. TROSTEN: This is a document you have prepared and shown to Mr. Macbeth per his request, is that correct?

WITNESS LAUER: That's correct.

BY MR. MACBETH:

Q I have --

MR. KARMAN: Is this going to be distributed to the parties, Mr. Trosten?

MR. TROSTEN: Mr. Macbeth?

MR. MACBETH: I think I would like to place it in the record, but first I would like to ascertain whether complete sets of these data are available. I have been given data sheets -- or data tabulations, perhaps, is better, which cover striped bass larvae during the day for the mid-depth, striped bass larvae during the day for the bottom, for the surface, white perch larvae during the day for the mid-depth, white perch larvae during the day for the bottom, striped bass eggs during the day for mid-depths, and striped bass eggs during the day for the bottom.

Now the -- I think one can work out what the missing sheets have to be. The most obvious ones, to my mind, are surface data during the day for striped bass and white perch and data for all the stations and types of fish for the night period.

BY MR. MACBETH:

Q Could you supply those tabulations?

MR. TROSTEN: I am not sure I understand the question, Mr. Macbeth. Are you saying has he got the tabulations or has he supplied them or -- would you please clarify your question?

MR. MACBETH: I thought my previous question established that such tabulations did exist.

BY MR. MACBETH:

Q Do they exist, Dr. Lauer?

A (Dr. Lauer) The ones you have exist and the tabulations that you talk of in the nature of their being missing do not exist. The tables, the nonexistent tables representing abundances of striped bass eggs at the surface and both day and night were never made, tables of that type that you have just shown me were never made, primarily because the whole table would be virtually zeroes. So we never went to that stage.

However, we did provide other tables that are the second generation tables after this one which do include the data for the surface in summary form as well as for the mid-depth and bottom depths.

Q I have those. What I am particularly interested in is being able to distinguish between the various stations A, B, C, D, E, F, and G. And the second generation tables do not make those distinctions. Could you provide the underlying data from which tables of this sort, which could

distinguish between the stations, could be drawn up?

A I couldn't at this point in time. The data do exist obviously because they went into preparing the summary tables; however, in the light of my previous comment, the tables themselves would have been virtually all zero, so there is not much to distinguish amongst the various stations when they are essentially all zeroes. There were a few positive numbers that occurred that obviously show up in the summary tables. Those data exist, but I could not give them to you now because I do not have the pile of data here. They do exist.

MR. MACBETH: I would like to request that I be provided those data sheets. The importance of it, I think, is perfectly obvious. Dr. Lawler has made a distinction between the various stations for his analysis of the susceptibility of striped bass eggs and larvae to the intakes at Indian Point, and unless we have the data which distinguish between the stations, we really can't analyze the basis for Dr. Lawler's conclusions.

MR. TROSTEN: We will provide you access to the data sheets; as Dr. Lawler indicated, they did not compare tables such as this. So we will simply provide you access to the raw data sheets.

In response to your earlier question, Mr. Karman, we will reproduce a copy of this and provide it to you.

MR. KARMAN: Thank you.

MR. MACBETH: Perhaps to make things simpler for the record, we could have this set that I have -- set of tabulations that I have been given identified and made an exhibit. I think I have laid a sufficient foundation for that. Could we have it marked as Exhibit A for the Hudson River Fishermen's Association and put in the record? I would ask the Applicant if he could provide copies, since unfortunately the one I have been given has been marked by us in the course of working over the data.

CHAIRMAN JENSCH: I think we tried to work out a numerical sequence of exhibits so that we wouldn't get differences among the parties, and if you will tell us what the next succeeding numerical number is, why, we will utilize that particular number.

MR. MACBETH: Does anyone know what the next succeeding numerical number is? I would be happy to use it if I knew what it was.

MR. TROSTEN: I don't think you have had any exhibits yet.

MR. MACBETH: I haven't, but I would be happy to join in the sequence everyone else has been using.

MR. TROSTEN: Each one has a different kind of sequence. ~~Would~~ Why not make yours Roman numeral one.

MR. MACBETH: That would be fine.

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CHAIRMAN JENSCH: Would you identify the document, please? How many sheets are contained in that collection you have there?

MR. MACBETH: Seven sheets, and the first sheet is headed Striped Bass Larvae (Day) Mid-Depth, Number Per Thousand Cubic Meters. There is no general title to the document.

CHAIRMAN JENSCH: Very well. The document which has been identified by counsel for Hudson River Fishermen's Association may be marked with the indication of Roman numeral number one.

(The document referred to was marked Exhibit No. I, for identification.)

CHAIRMAN JENSCH: Do you offer that in evidence?

MR. MACBETH: I do.

CHAIRMAN JENSCH: Any objection?

MR. TROSTEN: No objection.

MR. KARMAN: No objection.

CHAIRMAN JENSCH: There being no objection, Hudson River Fishermen's Association Roman numeral I is received in evidence.

(The document previously marked Exhibit No. I, for identification, was received in evidence.)

BY MR. MACBETH:

Q Dr. Lauer, what stage of the tide were the tows made that are from which the data on these tables were collected?

A (Dr. Lauer) I guess, considering the total array of the samples, probably virtually on every phase of the tide that exists out there. We timed the sampling, especially at nighttime, to come during the nighttime hours when we have experienced, at least with the mackerel invertebrate and also with the fish larvae, maximum abundances, we timed those samples relative to the time of day rather than the tide.

We do take the tide into consideration by way of towing against the tide, whatever that flow direction is at the time we are taking the samples. We don't gear the sampling to any particular phase of the tide, but we do react to what the tide is by way of towing against the flow of the tide.

Q Would it be fair to say, then, that these tabulations for each of the stations represent an average of the tides?

A Probably that's a fairly reasonable generalization.

Q Could you tell me what the efficiency of your gear in these experiments was?

A No, I can't. It is standardized type of gear that's generally employed for these studies, but so far as I know, nobody would be able to say precisely what the efficiency was of the nets relative to any particular organism or flow

velocity consideration. So these are estimates based upon using the same kind of gear in as similar a way as possible for all samples and making the comparisons on that basis. We don't know what the absolute efficiency is for each organism relative to each collection condition. To try to effect as much homogenity in the sampling effort as possible, this requires a three-man crew on the boats, and we have made it a practice never to have less than two of these be the permanent members of the crew.

On occasion we have somebody get sick or something, so we can't have all three being the same all the time, but I don't think we have ever had a situation in which we have less than two of the permanent crew on the boat to assure that the samples are taken in nearly the same fashion as possible on each collection date. But we just don't know what the absolute efficiency is.

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Q Does that mean these data tabulation tables give us an abundance from one station to the next but the absolute magnitude of concentration have organisms at any particular station?

A I think that is a fair statement. It is virtually impossible to, in these kinds of field biological samplings, to ever feel you have the absolute truth. We do use sampling devices to take these organisms and we can never know for certain that we have attained absolute truth in the situation. I think it is accurate to say that these are estimates of relative abundance by using the same gear types and methodologies at each of the collection sites.

Q I notice that in these sheets that were tabulated and have been marked as Exhibit I, that there were no tabulation of figures for yolk sac larvae. Why was that?

A Well, they have been tabulated and they are indicated on,the summary of those data are indicated on Figure 16 of my testimony on page 44. That probably -- the reason they don't show up in that particular set of tables, if you say they don't -- I didn't remember that they didn't but if they don't show up there, the probable reason for their not showing up is the same reason why the tables for the surface weren't prepared. The numbers of these are so small that most of the tables would be just a series of zeros.

MR. TROSTEN: Excuse me, Dr. Lauer. Would you like to

look at the tables again and refresh your recollection on this point?

WITNESS LAUER: That would be fine.

MR. MACBETH: I am sure that is the reason. I can show you this.

(Handing document to witness.)

CHAIRMAN JENSCH: Why don't you let him look at it for a few minutes.

WITNESS LAUER: Well, I would observe that there certainly are no tables here on the yolk sac stage. I would state that as a probable reason why there are not. I can't at this point state with certainty whether or not any such tables were made up. I am quite certain they weren't in that if they had been made up, they would have been given to you.

BY MR. MACBETH:

Q You really came to the answer I was driving at earlier, that the numbers would have been zero. I think if you look at Table 16 on page 44 of your testimony, that also is evident from that table.

On the other hand, there seems to be both more eggs and more larvae. Now, the yolk sac stage, of course, comes between the eggs and the larvae. Do you think that these lower numbers of yolk sac larvae represent a real situation in the river or is it some problem with the efficiency of the gear or

perhaps not this gear but gear in general?

A (Dr. Lauer) Well, I think it is probable that these smallest of the larvae probably do pass through the net. The nets probably are relatively inefficient in collecting them. This was determined to be the case in the so-called Cornwall studies and it was noted as a problem and we are using the same gear because of the need to overcome still other types of sampling problems. I have an idea that it probably represents a combination of things, one of which being that the yolk sac stage is a relatively short stage compared to the post-yolk sac larvae so at any given instant, you wouldn't tend to see as many there as you do, of the larva forms for example, just because of their rate of turnover, development into the larval stage.

It probably also has to do with the gear collecting them less efficiently.

Q Do you know the range of velocities that pass through the net during the tows?

A No, we do not. We don't measure the velocities into the net for each sample collected.

Q The charts and the testimony indicate that more eggs larvae, yolk sac larvae, striped bass tend to be found toward the top of thewater column during the night than during the day. Do you think that is a photo-tactic response or visual net avoidance?

A I don't quite agree with your characterization of what the data shows. I don't think it does show that the eggs ~~in~~ and the yolk ~~cycle~~ sac larvae are more abundant at the surface at night than during the day. It would be surprising to find them so since they are essentially immobile. It does show that the larvae are most abundant, more abundant at night throughout the water column than during the daytime especially at the surface in the case of the striped bass and that is has been true of the data for others who have studied the striped bass larval populations in the Hudson River. And, I think it is generally attributed to some type of ~~photo-tactic~~ phototaxis - type response.

Q I misspoke. I should have said they are more abundant during the night at the surface. Do you think that that could be visual need avoidance during the day rather than simply a photo-tactic response?

A Well, of course, this is a possibility, although, once you get below -- very below the surface, considering the ~~trepidity~~ turbidity of the Hudson River, there is not much light down there for them to react to. I don't know what their minimal levels of light intensity would need to be to avail themselves of the chance to react visually to the gear. It is a possibility that -- especially in the surface samples that there could be some of this involved especially with the larger forms that have some self-determination as to how

they can move relative to the river turbulence and currents and thereby can select the place that they would like to be and the place that they would not like to be.

Q I am looking now for the chart on which you had the various towing stations marked. Do you remember off hand where in the testimony that was?

A Sampling stations themselves?

Q Yes.

A That would be on page 15, Figure 6.

CHAIRMAN JENSCH: Of your testimony?

WITNESS LAUER: Of my testimony, yes.

MR. MACBETH: Thank you.

BY MR. MACBETH:

Q There were just a few things I wanted to check here. You have two arrows from -- one from line D toward a dot in front of Indian Point and one from the end of line E to another dot in front of Indian Point. What do those arrows represent?

A (Dr. Lauer) Okay, these -- those little circles represent the sampling stations for the microinvertebrates in chemistry and they are characterized by the same letter designations as are the towing stations for the larger forms including the fish eggs and larvae. So the arrows merely mean that the sampling location for microinvertebrates and the chemical characteristics of the water associated with those samples for

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stations E is located right at the plume coming out of the plant rather than along that line of tow, that is indicated for E for the macrozooplankton fish egg and larval sampling. That is a point sample, in other words, and the other one has to be taken by towing.

Q And the data marked E in the tabulations of tow data, in fact, come from the area marked with a line E on the chart.

A That is correct. For the fish eggs and larva.

CHAIRMAN JENSCH: As for Roman numeral Exhibit No.1?

MR. MACBETH: Yes.

CHAIRMAN JENSCH: Thank you.

BY MR. MACBETH:

Q Isn't it true that stations C, D, and E are those closest to the Indian Point Power Plant?

A (Dr. Lauer) That is correct.

Q And would they give a clearer indication of the concentrations of organisms near the plant than stations A, B, F, and G?

A Yes, they are closer to the plant and should give a more representative number than the ones farther away assuming there is any difference.

MR. TROSTEN: Mr. Macbeth, would the Reporter read the last question back, please?

(The reporter read the record as requested.)

MR. TROSTEN: Thank you.

BY MR. MACBETH:

Q I show you the second page of the Exhibit 1 which is entitled, "Striped Bass, Eggs, Larvae,Day at the Bottom," and draw your attention to the mean concentrations according to the seven stations. Does that show markedly higher concentrations at the Indian Point Stations than at the other four stations?

A (Dr. Lauer) The mean values for stations C and E are considerably higher than arethe mean values for A, B, F, and G. I would not consider the numbers at station D to represent any significant difference in concentration compared to A, B, F, and G. C and E do have higher values for the seasonal mean abundances.

Q Perhaps, Dr. Lauer, while I am here on this Exhibit, in front of me, you could explain to me the meaning of some of these mathematical calculations at the bottom, the dates, station error, the meaning of the stattion versus error, F numbers.

A Okay.

MR. TROSTEN: Would you identify the page of Exhibit 1 to which you are referring.

WITNESS LAUER: It is the top page. It doesn't have a page number on it.

These are simply statistical computations which in

one case shows the numbers of stations collected within -- the degrees of freedoms having to do with the stations, degrees of freedom having to do with the date, a sum of squares of -- involved with various quantities among dates and amongst stations and the mean squares and the errors associated with these and then it gives a computation of the stations versus error and gives an F factor indicating a value of 1.01 and it gives theoretical F factors that would be attached to these data for assuming statistically significant differences and the corrected F value does not exceed the theoretical F value and the conclusion to be drawn from this is that based upon the data on this sheet, there is no statistical significance between the stations.

BY MR. MACBETH:

Q And for there to be a statistical significance to the differences between the stations, the F factor -- the actual F factor would have to exceed the theoretical F value given on the bottom line.

A (Dr. Lauer) Yes.

Q Would you look through the exhibit --

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CHAIRMAN JENSCH: Excuse me. Was there an answer to the last question?

MR. MACBETH: I thought it was a question.

Would the reporter read that back and see if there was a question?

MR. TROSTEN: I thought there was a nod instead of a yes.

(The reporter read the record as requested.)

CHAIRMAN JENSCH: Thank you, Mr. Reporter.

BY MR. MACBETH:

Q Dr. Lauer, would you look through the sheets of Exhibit 1 and tell me on how many of these data tabulations the F factor exceeds the theoretical F value? And identify for me which charts those are.

A (Dr. Lauer) Well, the top page has to do with striped bass larvae, mid-depth location, and gives seasonal means for all of the seven stations and the F value, the observed F value does not exceed the theoretical F value in that case.

The striped bass larvae -- the second page has striped bass larvae for the bottom, for all of the seven stations, seasonal mean abundances. In that case the F factor does exceed -- the observed F factor, calculated F factor does exeed the theoretical F value by approximately 1.5.

I might observe with respect to these bottom -- these bottom ~~saples~~ samples that the difference appears to occur at

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two stations, one in the AB transect and one in the FG transect in which the tows are started at a location when the samples are being taken such that given the different tidal conditions and current velocities and wind and so forth, it can happen that the -- well, the depth profiles drop off very sharply off the end of those towlines and it can happen and does frequently that if the sample isn't completed before you get to those sharp drop-offs on those transects, then in effect the bottom net is not any longer towing approximately two feet off the bottom, but is now towing out over a depth of water of 75 to 100 feet.

This is one of the complications we found with those bottom samples taken in those locations which were specified by the policy committee's places that we should take the samples. That's one of the things that helps to add to the variance among these bottom abundances.

MR. TROSTEN: Dr. Lauer, when you refer to the policy committee, would you specify what you meant by that?

WITNESS LAUER: I am talking about the policy committee which is composed of the representatives of the State of New York Department of Environmental Conservation and representatives from the Hudson Bureau of Sport Fishery and Wildlife and from the state agencies and representative of New Jersey and previously Connecticut, which have the -- an overview over these studies, and in the earlier stages had

a direct role in establishing the format and station locations and design of the sampling program. At this point they are more of an overseer than they are a program manager. This is the group I refer to.

MR. MACBETH: Could we return to the question?

MR. TROSTEN: Thank you.

WITNESS LAUER: To proceed, the third sheet down in this Exhibit 1 has to do with white perch larvae at the surface. For each of the seven stations, seasonal means are given. The calculated F value does not exceed the theoretical F values. It is very much lower.

The fourth page down, also is for white perch larvae, but at mid-depth. For each of the seven stations, these are seasonal mean abundances given. The F values calculated do not exceed the theoretical F value.

The fifth sheet down, which is white perch larvae collected at the bottom during the day, also has data for each of the seven stations, gives a mean abundance for each of the seven. The calculated F value is approximately one-half of the theoretical F value.

The -- I think it is the sixth sheet from the top --

BY MR. MACBETH:

Q Just tell us what the title is.

A (Dr. Lauer) Collected during the day at

mid-depths, the calculated standard error exceeds the theoretical F value by a factor -- by a margin of .15.

The last sheet is striped bass eggs collected during the day on the bottom. It also gives the data for each of the individual stations and mean abundances for the seasonal occurrence of eggs at those stations. The calculated value is 0.97, which is less than the theoretical value of 2.32.

Q So how many of those charts had statistical significance in distinguishing between the stations?

A One did with some degree of difference, that being the bottom for striped bass larvae. And the other which came close was the one for striped bass eggs at the mid-depth during the day in which the calculated value exceeded the theoretical F value by .15.

Q And was-- the one that you said clearly did have statistical significance the one that I showed you and you told me that there were greater concentration for the Indian Point stations than for the other stations, clearly a greater abundance, and these two were the Indian Point stations?

A They do, although it turns out that the stations that make the difference in the calculated F value compared to the theoretical are not those two stations, it appears, but it is the other stations that I referred to that have this problem of the deep water associated with the collection of

the samples wherein the bottom nets tend to be pulled off of the bottom during the latter part of the tows, but --

Q In other words --

A Amongst --

Q -- it is your opinion that the difficulty with the charts is that not enough organisms were collected at some of the other stations, rather than at too many that are representative of the Indian Point stations?

MR. TROSTEN: I don't think Dr. Lauer has referred to any difficulty with the charts, Mr. Macbeth. Could you rephrase your question, please?

MR. MACBETH: Well, maybe "difficulty" is too strong a word. I did think Dr. Lauer was referring to certain kinds of experimental difficulties that are reflected in the charts, namely that he couldn't keep the net within two feet of the bottom on two of the transects, one being either A or B, and the other being either F or G.

BY MR. MACBETH:

Q Did you identify which of those transects you had that problem with?

A These are transects A and F, it is my recollection. I might want to double-check that to be sure.

Q Would you double-check it?

A Okay.

Q Now let me return to the previous question. Is it

true to say that you had kind of an experimental difficulty in those two transects and that you did not have it at the Indian Point transect, and therefore it was your opinion that fewer organisms than in fact existed were collected in the two transects of the experimental difficulty, while more representative sample of what was collected at Indian Point?

A I would just say that it is the -- those two stations where this problem with sampling occurs occasionally are the ones that it is my recollection that these are the ones that appear to -- when you try to determine the statistical significant differences among stations, these are the two stations that turn out to be statistically different than the rest.

The rest are not statistically different from each other. All I am saying is that that appears to be a possible cause for contributing to the variance that exists at those two stations which, amongst other things, causes them to be statistically significantly different from the rest, whereas the others are not statistically different from their companion stations.

Q Does that mean you have serious doubt as to whether there is a statistical difference in fact between those two stations and the other stations?

A I think there is a doubt raised as far as we are concerned because of that observation that we have had that

the bottom nets are pulled off the bottom if the tow extends out over this deeper water.

Q I just want to get some sort of reading on your level of concern here. Is that a serious doubt? You have gone to the extent of pointing it out and raising it in the testimony. Is it just a kind of fleeting feeling?

A It is something we feel like needs to be examined more thoroughly to the point where we have gone about getting a sonar depthfinder out there so we could more clearly define the depth contours for those particular stations so that we could go about then taking the tows without having this possible introduction of error into the collections taken on those particular transects for the future sampling. We feel that something we know is a possible interference and in the interests of trying to remove as many of these possible doubts as possible, we are going to go about trying to arrange so that we can monitor the depths so that we know we don't extend out over the deep water with the tow.

Q Does that mean that you would have doubts about any distinctions that were drawn among the stations that had to rely on the figures in the two stations where you have had this problem with the tows?

A I don't think I understand your question about --

Q Well, if someone were to make distinctions in the abundances among the various stations, for instance, as I was

earlier pointing out that there seemed to be a greater concentration at least two of the Indian Point stations, and if part of the foundation evidence which those distinctions were drawn were tows from these two stations, would you have serious doubts as to the validity of the distinctions that were drawn as a conclusion?

A It depends upon the conclusions drawn. For one thing, I think the -- considering the variability amongst the results for collecting on any particular day, that the best use of this data would be to take the data in toto as a characterization of the abundance relative to day and night, relative to depth for that sector of the river, taking all of the collections combined rather than to make -- to try to make too much out of the abundance at any given station, and the reason I say that is this: For example, picking out one of the days of higher abundances, just to give an idea of the ranges of numbers that are collected,this is for the striped bass larvae at mid-depth, for June 14th, for example, going from stations A through G. We get numbers like at station A, 100; station B, 633; station C, 201; station D, 254; station E, 18 organisms; station F, 6 organisms; station G, 151 organisms.

On that particular day, obviously, E and F were the lowest amongst the stations. On the succeeding day, on June 17th, by comparison, at station A there was 698 organisms; station B, there were only 25; station C, there were 434;

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station D, there were 6; station E, there were zero in that particular sample; station F, there were 52; and station G, 39.

All I am giving this for is to indicate the extreme variability that exists amongst an array of stations and it exists amongst an array of samples taken at the same station on any given day. This is a characteristic of plankton populations and therefore the more samples and collection data that can be combined, the more confident one can be about the representative of this as representing the distributions out in the river over an areal basis. That's the reason I say that I think the best use of this data is to characterize the abundances in this sector of the river relative to day and night, surface, middles and bottom, taking all of the stations together more so than to try to pick out individual data for a particular station in a given day or for a particular station, even amongst the seasonal means.

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Q Let me see if I have this clear in my own mind. You are saying that you would have a good deal of confidence in the variations that are shown for this sector for bottom, mid-depths, and surface and for day and night, but that you would have serious doubts as to the validity of any other kinds of distinctions that were made between the variations?

A Well, yes. The further you get down into the specifics of the individual stations and the individual samples, the fewer numbered samples you have to deal with. Therefore when you treat these to statistical analysis, the wider will be your surveillance and standard of deviation figures that result; and the less precision statement you can make about any of these smaller pieces of data.

Q Is there a -- I just want to see if I have this right: You feel there is a real, as it were, a breaking point, a real kind of change between the day, night, surface, mid-depth, and bottom figures and any other kinds of distinction?

That was a nod indicating yes, I think.

MR. TROSTEN: Would you repeat the question? I don't know if there are two questions or one.

CHAIRMAN JENSCH: Let him reread the question.

(The reporter read the pending question.)

MR. MACBETH: Would you like that rephrased?

MR. TROSTEN: I think it would be well ~~to rephra~~se to rephrase it

MR. MACBETH: I would be happy to.

BY MR. MACBETH:

Q Am I right in saying -- is it true that in your opinion you have considerably more confidence in the distinctions between abundances at the bottom and the mid-depths and the surface between day and night than you would in any other kind of distinctions that would be drawn from this data?

MR. TROSTEN: I believe that question is overly vague, Mr. Macbeth. You use the phrase "any other kind." Could you be more specific about that?

BY MR. MACBETH:

Q Well, we have been talking about distinctions between individual stations or groups of stations. For instance, I was suggesting a distinction between the Indian Point station C, D, and E and the other stations.

Dr. Lauer didn't think such distinction could be drawn. We want to get through a number of permutations of other combinations at other stations, but Dr. Lauer seemed to be indicating that he thought the data should be matched and all the stations should be looked at together for distinctions between bottom abundances, mid-depth abundances, surface abundances and between day and night.

I wanted to see whether he thought he had really a different level of confidence in that kind of variation in

the data than he did between any grouping of stations such as the three for Indian Point.

MR. TROSTEN: Well, he responded to that question. If you want to try a particular distinction, I am sure Dr. Lauer will address himself to the particular distinction which you want to contrast with day and night and surface, mid-depth, and bottom.

MR. MACBETH: I may have missed his response. Was his response that there was a considerable difference between the abundance -- the distinctions for bottom, mid-depth and surface and day and night than there was between any other kind of grouping of stations?

BY MR. MACBETH:

Q Is that your opinion, Dr. Lauer?

MR. MACBETH: Perhaps we could have Dr. Lauer answer this question. It might be a little quicker than too much more discussion between the applicant's counsel and myself.

MR. TROSTEN: The problem I am having with this is that the question is vague. If you want to ask Dr. Lauer a question, he has answered your question about the distinction between the day and night and the surface, mid-depth and bottom.

If you wish to address a question to Dr. Lauer concerning the confidence that he has in that distinction,

rather in those distinctions, versus the confidence that he has in some other particular distinction that you want to draw, then he will address himself to that.

I object, however, to a question which asks him to say whether he has more confidence in the distinction between day and night or between surface and mid-depth and bottom and some other undesignated, unspecified distinction that you are seeking to ask him about.

MR. MACBETH: I was simply trying to --

CHAIRMAN JENSCH: Excuse me. Excuse me. I think the witness has the matter in mind and I think his previous indication -- the context as I inferred from the answers, was that he had in mind the kinds of distinction applicant's counsel had just stated. I think that's what you intended to do.

He has greater confidence in the composite rather than in the characterization of the single station, isn't that your question?

MR. MACBETH: Yes, or some subgrouping of stations.

CHAIRMAN JENSCH: Well, that latter I think gets into the vagary that applicant's counsel is objecting to. Let's take the first phase of it first.

Will you state it in the light of the statement by applicant's counsel as well as your subsequent explanation about your question? Will you restate your question?

BY MR. MACBETH:

Q Do you think that -- do you think that subgroupings of stations can be made in which you would clearly have the same level of confidence as you do in grouping all the stations together to draw a distinction between surface, mid-depth, bottom, and night and day?

A Well, it depends upon the premises being examined and the purposes for doing the lumping of the data. I would just like to say in this regard it isn't my opinion regarding whether one -- whether a -- one type of lumping, if you will, of data is more amenable to statements with confidence than another. This is simply what the data analyses show. The data analyses show that there's statistically significant difference between say the surface abundance and the bottom abundance.

The data analyses also showed, as we just went through in some detail, that for the most part there is no statistically significant difference when one compares the mean abundance of organism collected at one station versus any other station with the two exceptions that we identified.

So I can't really answer your question any more specifically than that. The reason I say that the data is used in a way to categorize or compare abundances, surface, middle, and bottom yield statements that can be made with confidence is that that's what the statistical -- the results

of the statistical analyses were.

I can't go on further into theorizing what level of confidence would come out of other theoretical groupings of data. It depends upon how those groupings were made and the purposes for which they were being made.

Q That's fine.

Thank you, very much. Can I have that document back?

CHAIRMAN JENSCH: Would this be a convenient place to recess and let us take a look at this Roman numeral one?

MR. MACBETH: Yes.

CHAIRMAN JENSCH: Is this a convenient place to recess?

At this time let's recess and reconvene in this room at 11:05.

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(Recess.)

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CHAIRMAN JENSCH: Please come to order.

Mr. Macbeth?

MR. MACBETH: I am ready to proceed.

I would like to turn to a different chart.

BY MR. MACBETH:

Q I am referring to table 19 of Dr. Lawler's testimony which follows page 60 in the testimony and in describing the experiments on page 60 it says the intake in the general vicinity of the plant conducted by --

MR. TROSTEN: Do you have the page of your testimony before you, Dr. Lawler?

WITNESS LAWLER: Excuse me?

MR. TROSTEN: Do you have the page of your testimony before you?

WITNESS LAWLER: Table 19?

MR. TROSTEN: Yes.

BY MR. MACBETH:

Q It says sampling in the intake in the general vicinity of the plant conducted by NYU establishes the presence of this mechanism, mechanism described above.

MR. MACBETH: I want to try and work through the material with Dr. Lauer. I just would like to see what it was that NYU people did in relation to this chart and try and get a little of it straightened out.

BY MR. MACBETH:

Q Dr. Lauer, could you describe to me what responsibilities NYU had in collecting and preparing the data which are reflected in table 19?

A (Dr. Lauer) NYU as part of their regular -- as part of our regular sampling program conducted our sampling at the intake and we also had sampling going on in the river during this time using the standard gear that we use normally to collect organisms for abundance and conditions, assessment, intake and discharge, and for abundance in the river. These data from this -- from that particular sampling period were -- well the samples were taken and processed and the organisms sorted out of those and enumerated and the numerical data obtained from those samplings were then submitted to ~~Brook-~~ Quirk Lawler and Matusky ~~lawn and Matuski~~ and they took those data from there and generated the information in page 19.

Q Table 19?

A In table 19.

NYU did not participate directly in the preparation of the table other than to supply the numerical data.

Q Then perhaps we could start by your drawing on the easel where roughly these east and east channel tows were made. I just had a little trouble trying to figure out all the different terms on the chart.

A Okay. Dr. Lawler advises me that they did not

use any ~~or~~ of our river sampling data from that time probably because it still -- we are still sorting the samples and don't have that available. The river sampling data at east channel designations are ~~Brooklawn and Matuski's~~ Quirk, Lauer and Matusky data.

The only data represented on that table 19 that came from original data from NYU is apropros the intake concentrations.

Q All right.

How many -- could you describe to me how you arrived at the intake concentrations?

A The intake -- we measure -- we sample organisms coming into the intake by sampling simultaneously with gear of the same type that is used out in the river at the surface, at intermediate depth, usually the mid-depth and at the bottom.

MR. TROSTEN: Dr. Lauer, would it be helpful if you described this on the easel?

WITNESS LAUER: Okay.

MR. TROSTEN: This might be helpful.

WITNESS LAUER: I am not an artist so excuse me.

We have Indian Point out at the face of the intake canal, a fine fixed screen to keep out the debris and fish from the intake system. (Indicating.)

I will characterize that as this straight line of this nature. Then back in the intake canals for Unit 1 there is the vertical traveling screens which -- through which

the water again passes before it gets to the pumps which pump the water through the cooling water system of the plant, and the water level -- first of all the overall depth of this area is in the order of 25 to 26 feet depending upon the high light. So we can represent the water as coming into this structure like this. (Indicating.)

When we found that the vertical distribution of organisms coming into the plant and out in the river varied so much with the depth, we had to go about designing a sampling procedure and a rig that made it possible for us to sample at the various depths in the intake canals.

So we had a limited space to work with. There is an opening -- the building sort of comes down in this fashion. (Indicating.)

The vertical traveling screens are inside. So that we had a space essentially from here to about here which was an open area looking down into the intake canals for access to the intake canals. (Indicating.)

We had a problem of trying to take samples in this relatively confined area. There is no possibility of towing in here. So what we did was to place a frame consisting of metal pipe connected together at the top and fastened to the bulkhead here in this opening. (Indicating.)

On that we could then mount a polyvinyl chloride composition frames. These were two pipes constituting a

trout running from the water level down to the bottom of the intake canal.

We constructed frames that could slide up and down on these pipes so we could position the sampling net any place we chose to to take samples. We had two of these frames installed on each of these track devices in two of the intake base and the same kinds of structures are also being used in discharge canal sampling stations.

So the procedure is we then lower this frame down into position with the net attached so we essentially have this frame sitting down here now and the net attached to the frame and the bucket back here. (Indicating.)

We had to have this frame to keep the net from going back into the traveling screens because of the confined space.

Our normal positioning then of the middle depth was relative to the tide depth. We had a gauge-type reference core going from top to bottom in here from which we could tell what the surface level of the water is and based upon what the surface level of the water is at any given time when we are taking samples, we determine what the mid-depth is and we send this next unit down to that mid-depth.

So here we have another net suspended in the water like this. (Indicating.)

Then we adjust to the side of this frame, because we

don't have to deal with these nets going back into the screen up here, we can control the location. We also mount a -- or put a net right at the surface like this. (Indicating.)

That is supported by a rope which is anchored to the steel bulkhead down here.

In order to work down in here we have also built up -- had to build up a sampling platform which extends down like this to provide a floor down here to work from. (Indicating.)

It is from this platform that we maneuver these up and down in the water column in the intake structures. We have a ladder that gets down int o there.

So the samples to which you refer were taken with this general schematic of sampling gear relative to depth involved. The procedure is that we go out and take three buckets with us, put in water into the buckets from the intake canal in which we are going to place the sample that is collected here.

We time the positioning of the -- of these nets at these depths synchronously so the bottom net goes down first and this one and this one. We time when they go down. Then we keep them down there for -- generally for five minutes. But the time varies depending upon whether we are trying to look at condition or abundance.

If we want to get abundance estimates, it is

desirable to have a much longer position time for the position net than if we want to look at condition because the netting itself has effectson the netting of the organisms.

Generally, and in this case, we would have used five-minute positionings of the nets at each of these three depths.

At five-minute time period these nets are again raised individually and the time is recorded when they are pulled up. They are rinsed down so we get all of the materials that might be stuck along the sides into the bucket, into the plankton bucket. These are removable plankton buckets on these nets. The buckets are taken off and emptied into each of the previously prepared water buckets so we redilute the sample a little bit to provide a water cushion for the organisms to reside in until we carry them back into the laboratory where they are sorted.

They are sorted in the -- the fish eggs and larvae are sorted out of these samples in a wet lab facility that is approximately 20 to 60 feet away from these intake base. We just carry them in, pour the samples carefully into glass trays with black bottoms on them which we then position in a trough which has river water running through it so we don't have changes of temperature taking place while we are examining the sample and sorting the fish eggs and larvae and other organisms out of them.

Then all of the collecting information for these samples, for each of these positions, is recorded on a summary field data sheet as are the numbers for the kinds of fish eggs and larvae and other organisms that are found in these.

There is a limitation to how far one can go in sorting these out to species visually. This involves striped basss and white perch larvae. You have to look at each one of those individually under a microscope so that those separations into the two species are then done at a later date. That is a laborious process. That is why all of my testimony currently is described as white perch and striped bass or the morone group. You just can't sort those things out visually on the site. This has to be done later.

These samples are taken in then and the eggs and larvae are enumerated relative to the position that the nets were taken and we also have these metered so we record the revolutions that the meter turned during the time in which the sample was taken; and by using calibration factors for the meters, we can then calculate back what the volume of the water was that flowed through each of these nets; and having determined that and knowing the number of organisms collected from the net, we can then compute the concentrations of organisms per unit volume of water whatever that is.

We generally use thousand cubic meters as the unit

of volume for our data.

This describes one sequence that is followed to collect this -- these organisms at the intake and then we follow repeatedly through that same sequence throughout the period.

Most of this is done at night because that is when we tend to get the higher abundance of organisms. Most of our data, but not all by any means, is done during the nighttime periods between -- generally between about 9 o'clock and 3 o'clock in the morning.

At the same time we are doing this, we are also collecting with different mesh nets to get the smaller organisms and we are collecting whole water samples to look at phytoplankton populations and we are sampling the water for -- in reference to looking at chlorination effects. We are sampling the water to do chlorine analysis on it so that at any given time when we are out there on any given night, we would generally collect on the order of between 40 and 60 samples throughout the cooling water system of which approximately half of those would be in the intake structures, and we have anywhere from 8 to 12 to 13 people involved in doing these intake discharge canal assessments relative to the different organism groups that we have to sample for.

We do have to use an array of different kinds of gear because of the fact that the different gear is amenable

to catching different size organisms.

So I think that is about as thorough description as I can give.

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BY MR. MACBETH:

Q Thank you. Now the figures that are given on table 19 for intake concentration, how many samples were taken to develop those particular numbers?

A (Dr. Lauer) I don't recall offhand how many were taken for those two numbers that come out of NYU's data. Maybe Dr. Lawler knows. If he doesn't, well, then, the records of it -- I just don't know offhand in my head what those numbers are in terms of the numbers of samples involved.

Q Dr. Lawler, do you know?

A (Dr. Lawler) No, I don't know offhand. We can get that information.

Q I would appreciate it if you would provide that information. I take it from your response that it isn't all the samples from the entire seller, but some subgroup samples?

A That's correct.

Q This may make a number of questions that follow somewhat difficult to pursue, but let me go over them. If it means providing more information, we can try to do it all at once.

Do you know how many days of sampling are included in the data base for these intake concentrations?

(Witnesses conferring.)

WITNESS LAWLER: Okay. The numbers ~~intake~~ for the concentration, 1.4 and 3.77 to which I think you are referring?

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BY MR. MACBETH:

Q That's correct.

A (Dr. Lauer) Is based on a set of samples, the total number of which I don't know, but which were taken during the -- one 24-hour period.

Q One 24-hour period? Do you know which 24-hour period it was?

A I believe it was July 25th.

Q July 25th?

Dr. Lawler, let me show you another document which has a title saying Intake-Discharge, and consists of 11 sheets. Is that a tabulation of the data that you collected at the intakes and discharges of Indian Point 1 in the past summer indicating dates on which the collections were made, the stations at which they were made, the times at which the collections began and ended, the depth at which the collections were made, the meter reading through the net, the temperature of the intake and discharge, and the number of white perch and striped bass that were collected alive, dead, or under a column marked "S" which I assume means "stunned"?

MR. KARMAN: Where did this information come from, Mr. Macbeth?

MR. MACBETH: I obtained this from the Applicant last night about 7:00 o'clock.

WITNESS LAWLER: What was the question?

MR. TROSTEN: Would the reporter read the question back, please?

(The reporter read the pending question.)

WITNESS LAWLER: The answer to that is that the data included on these sheets are data as described that characterized sampling done by NYU, not by myself, but by NYU team during this past summer, insofar as we have gotten through with the processing of the data up to this point. We do have additional samplings, considerable number of samples, most of which would have been taken after the striped bass, white perch, larval season ended which are not included here. This doesn't represent the totality of all of the samples taken by NYU, is all I am saying. This represents what we have processed up to this time.

BY MR. MACBETH:

Q Could you give us a date after which most of the samples would fall -- additional samples would fall? You said it was -- would that be after the 1st of August?

A (Dr. Lauer) Yes, that would include most of the samples to which I referred that were collected after what are represented on these sheets.

Q And this is a fair and accurate tabulation of the data collected by NYU?

A To the best of my knowledge, yes.

MR. MACBETH: Mr. Chairman, I would like to offer

this in evidence as Exhibit Roman numeral two from the Hudson River Fishermen's Association. Again the Applicant has the original and I think it might be helpful if a fair copy could be obtained from the Applicant for the record.

CHAIRMAN JENSCH: I assume you are asking for extra copies and you are going to furnish them to the reporter, three, and one to each party, to the Board?

MR. MACBETH: I will do that at our expense if the Applicant will do the Xeroxing for me. I have certain problems in actually obtaining the documents from the Applicant to do the Xeroxing.

If the Applicant will send me a reasonable bill for the Xerox, I will be happy to pay it.

CHAIRMAN JENSCH: Certainly leaves it wide open.

(Laughter.)

Any objection to the identification of the document as identified by the witness and -- to be received in evidence?

Applicant?

MR. TROSTEN: No. We have no objection to this, Mr. Chairman. I will say that this document was prepared in order to respond to a request for data which Mr. Macbeth made of us. It represents a reduction of raw data. I would like -- we would like to have the opportunity to double-check all the numbers since this was preapred in haste, as Mr. Macbeth indicated. This was given to him at 7:00 o'clock last night.

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Subject to that, I have no objection.

MR. KARMAN: No objection.

CHAIRMAN JENSCH: Very well. The document identified by Hudson River Fishermen's Association counsel and marked for identification as Roman numeral number two is received in evidence.

(The document referred to was marked for identification as Exhibit II, and was received in evidence.)

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BY MR. MACBETH:

Q Am I correct in assuming that the entries here under the 25th of July, 1972, on sheet nine of Exhibit II reflect the data which was used to develop the intake concentrations in table 19 of Dr. Lawler's testimony?

A (Dr. Lauer) Well, there are data beginning on page 9 and extending over to page 10 for the date of July 25th, 1972, which identify samples, sample locations and times of samples and depth at which samples were taken, meter readings, et cetera.

I would presume that these include those used by Dr. Lawler, at least the results from these were used by Dr. Lawler. I couldn't at this point, without checking in more detail to know whether this is the complete and accurate list which the -- the results from which he used specifically or

not. I think that would require going back and requiring the data -- acquiring the data. I think it represents most of the samples, if not all of the samples that were taken, the data of which were turned over to Dr. Lawler for his work. I just can't state that with absolute certainty.

Q I'd appreciate your looking and informing us if there is any other data or data that was omitted earlier just so we can be sure what it was that Dr. Lawler had before him.

I am afraid I will have to ask you to give me the document again.

(Witness hands document to counsel.)

Could you identify for me which of the samples of the intake are included under the day samples and which under the night samples?

A Okay. As far as the day samples are concerned, since these were all approximately five minute net placements, I'll just identify them by the beginning time.

Q Thank you.

A As far as the day samples were concerned, there were samples taken from the intake at 1453 -- this is 2:53 in the afternoon; 1451 and 1450; one sequence, surface mid and bottom.

There was another sequence taken at 1451, 1450, 1450 in the intake tube rig of the type I described above (indicating).

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There was another sequence of samples taken surface, middle and bottom at 1610, 1619, 1619. This was taken at intake one station.

There was another set of samples taken at intake one station at time 1956 --

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Q This was really the point I was coming to. 1956, is that included for day or night?

A For that time of the year, this is one of the longest day periods of the year, it would still be a daytime sample, still be daylight.

These would be -- I would categorize these as daytime. We don't see the vertical redistribution of organisms taking place until just about darkness and generally it is on a significant upswing about an hour after dark. So we generally characterize our nighttime samples as those samples taken approximately one hour after darkness.

Q And on the 25th of July, that would be roughly when?

A That would be roughly between nine and ten o'clock, something like that, in the evening.

Q Thank you.

A So to continue through on the page 10, the -- there was another set of samples taken at intake at -- I think I left off something -- I don't know where I left off exactly before you raised the question.

Q 1956, I think.

A This was another set taken at the intake at 1956, Station 1 intake; 1955 at middle and 1955 at the bottom.

There were additional samples taken at the intake 2

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Station at 1955, time 1955, surface and middle depths and then there was a delay until nighttime set in and then there were intake samples taken at time at intake Station 1 at the time 0030. This would be 12:30 in the morning, one taken at 29 minutes after 12 in the morning and one taken at 18 minutes after 12 in the morning at intake 1.

And there was a similar set taken at intake 2 at 0030, 0029 and 0018 representing nighttime samples.

Q Thank you.

That means that there were six samples taken during the nighttime, is that correct?

A Well, to be sure of that, I would have to look at that again.

(Counsel hands document to witness.)

That is correct, yes.

Q And in those six samples, how many fish did you take?

A Looks like one fish cateogrized as dead.

Q What confidence level do you attach to that experiment as a basis for predicting the intake concentration of young juveniles during the night?

A I don't know. We haven't run statistics on this to know what the confidence level would be. I think in general we would say that this means that there were very few fish coming through but I wouldn't know what the confidence level would be.

Q Couldn't really be many fewer?

A That is right.

Q At least you couldn't have caught many fewer.

Do you know what the sampling efficiency of your gear was that night?

A Well, as I indicated earlier, we don't know what the sampling efficiency of the gear is with regard to any particular sample. We simply take the samples in the same way and compare them accordingly.

Q Do you have problems with the gear in the intakes from time to time with clogging of the nets or failure of the nets to unfill properly?

A We don't have any problems with clogging unless we take extraordinarily long sets of the nets. We are sampling much, much less water per set of the net in these nets than we do out in the river when we are towing the nets behind the boat.

One fish isn't going to clog up much as you pointed out. There are other organisms, of course, in the system, but we haven't had any problems with clogging. Occasionally, we have had some problems with getting the apparatus to slide up and down smoothly on the pipe structures. This is generally been most frequently of occurrence when Unit 2 pumps have been tested which is greatly accelerated velocities through the discharge canal. That increases the pull on the nets involved

in the riggings; and we have to add weights or otherwise correct for this so we can get the nets to go up and down smoothly so we don't have a time problem with getting the nets up and down.

Q Do you know if you had any problem with the nets that night, the night of July 25th?

A The only thing that I could judge that by would be that apparently not because we were able to get the nets to the middle and bottom depths and the times given for the lengths of the net sets were regularly five minutes; so if there had been difficulties getting the nets up and down, that would have been reflected in the time periods in which the nets were in the water.

DR. GEYER: May I ask a question here?

This one dead fish was of what size?

WITNESS LAUER: I don't really know. I can't look at the sheets to give that. The sizes aren't on there. For that time of the year, I could give you an approximate figure of what we were finding in general. That was getting towards the end of the morone group occurrence coming into the intake and out in the river, as well as as far as the river samples were concerned, and we generally caught larvae up to a maximum length in these nets ranging between about a half of an inch and three-quarters of an inch.

Beyond that we seldom caught a fish of larger size

than that. I would guess that this fish was of that size or less.

DR. GEYER: Pretty small fish?

WITNESS LAUER: Yes.

DR. GEYER: In this connection, were the screens, the three-eighths inch fixed screens out in front of your sampling station in place at the time you took these samples?

WITNESS LAUER: Yes, there was.

DR. GEYER: Thank you.

BY MR. MACBETH:

Q Dr. Lauer, let me just read from this for a moment and check my figures. Going over these nighttime samples, the six samples taken, the first shows that intake 1 at the surface, that the test began at 0030 and ended at 0031. At intake 1, it started at 0029 and ended at 0030. At intake 1 at bottom at 0019 and ended at 18.

At intake 2 at the surface, it started at 0030 and ended at 0034. The intake 2 at mid-depth it started at 0029 and ended at 0030. At intake 2 bottom, 0019 and ended -- started 0018 and ended 0019, is that correct?

A (Dr. Lauer) From where were you reading from?

MR. TROSTEN: Dr. Lauer, do you want to have the tabulation read back to you? Were you listening carefully enough.

CHAIRMAN JENSCH: Then I think if you do read from a document like that and ask him to check it, the document should

be before the witness and you can point out precisely where you are reading so he can check it without having it reread.

MR. MACBETH: I will do that in the future.

CHAIRMAN JENSCH: Do you desire to have the question reread, Dr. Lauer?

WITNESS LAUER: Are you talking to me?

CHAIRMAN JENSCH: Do you want the question reread to check the figures?

WITNESS LAUER: I would like to have it identified which particular ones Mr. Macbeth was reading. I see some that were minute totals. I am not sure of all the ones he was pointing out.

CHAIRMAN JENSCH: Let the question be reread, please.

(The reporter read the record as requested.)

WITNESS LAUER: Okay. With respect to -- from what you read back, with respect to an intake 1 bottom sample, I think it was said that it started at 0019 and ended at 0018. That is reversed as it was with the last one until you corrected it.

But -- yes, in general that is correct. There was a series when I was referring to the five-minute tows before I was looking at the intake samples on the previous page 9, all of which were for five minutes, but back here at the tail end of these samples in the intake for the nighttime, there were for the most part, one-minute tows. This is what I had

indicated earlier above that if we are trying to sample for condition, we frequently reduce the time of the set in order to try to get the organisms in as good a condition as possible to minimize sampling damage and so obviously that is what was going on here. We were taking one-minute tows to try to minimize sampling damage to be able to look at the condition of the organisms.

Therefore, the tows were of shorter duration than I previously described.

BY MR. MACBETH:

Q And the flip side of what you previously described is when you are sampling for abundance you take longer tows, is that correct?

A Well, we get abundance out of both of them. But when we want to be especially careful about trying to minimize the collecting effort, we reduce the sampling time in the interests of trying to get more reliable data for the condition; but we get -- we calculate abundance information out of both of them; but in general the condition factor things we like to try to take some short tows to minimize sampling damage.

Q Could you just tell me generally how much confidence you have in this -- this comes to about 11 minutes of towing for predicting the abundance of white perch and striped bass in the juvenile stage over the course of the summer at the intakes to Indian Point 2?

A I didn't make any kind of statement regarding these as representative of intake abundance during the course of the summer in any way at all nor have I indicated any confidence limits which we attached to these data.

Q No, I realize you haven't.

A Excuse me. As I indicated earlier, in regard to your previous question about confidence limits, we haven't computed statistics on this individual block of data to determine confidence limits and I haven't spoken of any such.

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Q Well, let's leave confidence limits. Just your general opinion from the experience you have had with on-the-site studies. Do you think you can make an accurate prediction, a meaningful prediction from this 10 or 12 minutes of tow data on the night of July 25th as to the abundance of white perch and striped bass in the intakes?

MR. TROSTEN: Would the reporter read the question back?

(The reporter read the pending question.)

MR. TROSTEN: In the intakes for what period, Mr. Macbeth?

MR. MACBETH: For the period when early juvenile and striped bass and white perch would be present in the vicinity.

WITNESS LAUER: Well, that's for a period of about two months.

As I indicated earlier, I think a sample is most representative of the abundance that existed at the particular time the sample is taken. I certainly would not indicate that you could take any set of samples from a given time and place and use that to characterize the abundance of those organisms throughout the season of occurrence in the river in any way at all. I never have said that.

BY MR. MACBETH:

Q I realize that. I just wanted your opinion about it.

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CHAIRMAN JENSCH: Excuse me. I wonder if I could understand that answer. Is it your thought then that -- as I understood your answer, that a sample is representative of the abundance only for the precise time at which the sample was taken, and so for this period to which attention has been directed of 11 to 12 minutes, all that would show is that for 11 minutes at those times, that where the abundance is shown by the statistics, you reflect it? Is that correct?

WITNESS LAUER: No, I wouldn't say that is correct in that that was the total time that nets were in the water with respect to that particular date. Of course, a sample is most representative of what you get in a sample. Those samples were spread over a period of time that particular night and so the abundance estimates could be broadened, and they are, to estimates covering that particular nighttime period over which the samples were taken, not just the 11 minutes during which the samples were taken. And then the further you try to extrapolate away from that, of course,the less confidence you would have that the numbers coming into the net are representative of the organisms in the vicinity and that just depends upon the timeframe that you would extrapolate these to.

That's why I indicated that I certainly would not characterize these abundances as representative of the abundance of morone group larvae in this vicinity on a

seasonal basis. I haven't done that and I wouldn't.

CHAIRMAN JENSCH: Well, what is the significance of the samples you did take in 11 minutes, then, in your view?

WITNESS LAUER: In terms of what -- I don't really understand the question. In terms of what they represent?

CHAIRMAN JENSCH: What information do you feel is derivable out of these 11 minutes of samplings?

WITNESS LAUER: The use we have put these numbers to from this 11 minutes of sampling is to take this value as representative of the abundance that occurred at that time and combine these pieces -- that piece of information with other pieces of information from sampling at other nights in the same place over more extended periods of time to draw -- in fact, we haven't gotten to this point yet. We haven't gotten to the point of calculating ourselves what the relative abundance coming into the plant is compared to the abundance out in the river.

But this is the use we will make of them, is to take these bits of information from the sampling times when we did take samples and combine them with similar data from other sampling dates and times and thereby draw some conclusions as to the average abundances of organisms coming into the intake during the season of their occurrence in the intake samples as well as, we hope to get some information on the matter of their abundance relative to depth coming into the

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intake canals. We haven't found those computations yet. This is all 1972 data, as you are well aware; and we are processing the data and reporting to you as we get data processed, and so this is sort of a continual progress report system that we are on here. We haven't gotten to the point of making use of those data in that way. That's the intended use that we have for the data aside from looking at the condition of the organisms in the samples as to whether they are alive, stunned or dead.

CHAIRMAN JENSCH: If I may digress just a moment, when did you start making tests like this on the Hudson River? Is this something new? My point is this, Dr. Lauer: I don't know whether you were here at one of our earlier sessions. Here it is 1972. Indian Point No. 1 started about 1961 -- 1960. What's been going on for 10 or 12 years that we are just now finding some data? You say give us five more years and this is all we need; we will have all the answers. Why haven't you had the answers since 1960?

WITNESS LAUER: Well, I think -- we had a little discourse on this in one of the previous hearings. We also were not having these kind of hearings in 1965, either.

CHAIRMAN JENSCH: What difference does that make as to the kind of sampling you should be undertaking?

WITNESS LAUER: Well, I think it has a lot of relevance in that there just was not the -- there just was not

the concern or the crystallization of what kinds of concerns there ought to be relative to power plant operations back in that time period.

Up until about two years ago, the word "entrainment" was hardly even coined in this particular usage. It had existed and was used in other respects. Most of the concern, both amongst the Regulatory agencies and the technically competent people in the field studying power plants' effects on aquatic life, had to do with what the effect of the thermal plume would have on the aquatic ecology of the receiving waters and we were all concentrating on that, me included.

This was the concern that everybody addressed themselves to. All of the criteria, as you know, really are established on the basis of the temperature of the receiving water. This was the focus of concern and the regulations even to this day still focus on that. There aren't any criteria for impingement or entrainment in the books. It is all on the thermal plume. This has been a historical development kind of thing wherein first there had to be some recognition of a potential problem; and then there had to be a development of the foundation on which you studied what's important and what isn't important to look at; and this took some time.

I guess it wasn't really probably until some very good publications in the 1970s that this matter of possible

effects on aquatic organisms going through the plant was really crystallized.

At that time we began our first-shot efforts at looking at some of these entrainment effects, and with time and as we learned more and more about what we needed to look for and how we needed to look for it, we have gotten involved in much, much more sophisticated and much, much more manpower-demanding kinds of studies to the point that we are now where we are.

When we laid out our program design in 1970, at that point we felt like that this matter of the entrainment was just beginning to be crystallized as a possible concern and was being publicized as such, and we began to have some meetings within the technical community on this subject and we came back and designed a program and submitted it to Con Ed, and they agreed to begin funding these kinds of studies.

At that particular time we thought we had at least a good year's lead time on the -- on the requirements for this kind of data and we thought we had that kind of lead on -- as far as studies were concerned having to do with power plant effects.

Then the Calvert Cliffs decision came along which precipitated these kinds of hearings on the aquatic environmental issues and we found out where we thought we had been a year ahead, we turned out suddenly to be a year behind.

That brings us to the status quo now wherein we have got a very significant level of effort being applied to studying these kinds of effects at one power plant. As I indicated earlier, we have got a staff of -- on the order of 12 to 13 people spending their full time on this, and we have got anywhere from seven -- it goes up to 20 occasionally temporary and part-time people working on this, and I think with a couple of rare exceptions that involve studies that have focused just primarily, say, on fish larvae, and not on the rest of the things. I don't know of an exception that I can think of that involves the level of manpower effort and time and devotion to trying to determine what kind of effects organisms are experiencing coming through this power plant that exists up here.

We are talking about the size of staff that I mentioned, budget that probably exceeds the total environmental budget that exists for most power plant studies; and our budget simply has to do with studying the entrainment effects and population abundances of these organisms that exist out in the river that are susceptible to being entrained.

It is unfortunate that either the technical community didn't foresee this a year or two before it was crystallized by the Calvert Cliffs decision so we had all of this data available, but that just happens to be a fact of history that the public interest and -- combined with the

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technical disciplines, awareness of the potential problems, and the needs for this kind of definitive data all came up essentially in 1970 and '71. So here is where we are. I don't like this any more than you do. I abhor having to be in a position as a scientist of generating data out of bottles one week and running in here with numbers the next. Certainly it would be preferable to have a chance to sit on these data, adjust them, analyze them in as many possible ways as you can analyze them for, write them up in a reasonably professional way with due consideration being given to what you are saying, so that we find it uncomfortable as well.

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CHAIRMAN JENSCH: Well, let me ask you, I don't know when this word "entrainment" became popular, if I may use that word, but I don't know what the word should be used, but when was the first fish kill on Indian Point 1 that led to the trucking activity and so forth? Couldn't that have been the starting point? That was back about 1965 or so, wasn't it?

MR. TROSTEN: Mr. Chairman, may I address that for a moment not as a biologist but just from a historical point of view?

The fish impingement problem arose in 1965 and at thattime the focus of attention of everybody I would say was on the matter of the impingement of large fish on these intake screens. There was no consideration being given of which I am aware to the matter of the very small fish, one inch perhaps, going through the screens and going through the plant. These were two distinct problems and the whole attention of everybody was being focused on impingement of these very large fish.

Now at that time of course as the Chairman has indicated, there was a problem with the killing of these very large fish. That problem itself has shifted. I would say it has shifted completely over the period of years from being a problem of the impingement of very large numbers of large fish to the impingement of these very, very small fish and the

numbers of fish that have been impinged have been very, very significantly reduced so that these fish are being carried away, I would say, in handbags, Mr. Chairman, under normal circumstances rather than trucked away.

But that -- to answer your question, sir, the impingement problem, the impinging of these fish on these large screens, was not seen in any way as related to this problem of the small fish going through the screens. That is what DR. Lauer was saying.

CHAIRMAN JENSCH: Yes. As I understand your statement it is one problem for large fish and wone problem for small fish. You trucked away the large fish but if some are still going through the screens, you must have had to pick up the small fish somewhere. The only point I am asking really is this: For the moment as I understand it, we are talking about fish distribution.

My question was couldn't these previous fish kills, regardless of the particular area of concentration, whether you are more interested in smaller or larger fish, but wouldn't that have led to the location of the fish so that when you were getting these large kills at different times of the year, you begin to wonder what is out in the river that is coming in and when are they going to be there and what can you do to shut your plant down when you can't stop it any other way? That sort of thing.

If we are looking for fish distribution, couldn't these data have been developed earlier so we don't have to think we are going to start now to find out where the fish have been? We have seenthem come into our plant but now we are going to find out where they came from. It seems to me some of that where they came from could have been developed sooner.

MR. TROSTEN: I think Dr. Lauer should address the technical considerations and the thinking in the technical community that led up to this and why the data has been developed on the schedule that it has.

Would you do that, Dr. Lauer?

CHAIRMAN JENSCH: Particularly about distribution. Weren't you interested in finding out where the fish were in the river before 1971?

WITNESS LAUER: I still seem to detect some confusion about the kind of fish we are talking about and I have been addressing myself in my testimony to those small fish that come through. They are really never visible to anybody as a fish kill. That was one of the reasons why there was not a public concern raised about these and the technical community also up until about the time period of 1970, there no doubt wereindividuals who wondered or had concern about this previously, but that is when it got to be a real crystallized matter of concern as far as the effects on these

small organisms passing through the plant is concerned.

But there were studies done on distributions of these eggs and larvae out in the river that were done and this was done in the period '65 through '68 I believe having to do with the Cornwall -- so-called Cornwall studies and these are the foundation data that are now being used by not myself because I haven't been involved in these modeling efforts, but are being used by the individuals, including the AEC Staff and Mr. Clark and Dr. Lawler here as a foundation material for describing the possible susceptibility to entrainment of organisms coming through this system.

So there are thosekinds of data that existed and what we are reflecting here now is that those data were collected, they are what they are, but as we go down this path involving these environmental concerns, the questions get more penetrating and we get more of a focus on what kinds of questions we ought to be asking ourselves, and then we go about trying to identify these.

It is no different than any other kind of a research program where you design a program based upon a foundation of data you have at that particular time and then you go about executing that program and as you execute it, you learn a whole lot about what you did right and also a whole lot about what you did wrong.

Then you turn around and adjust things in the next

year to try to further improve and get more specific.

What we are getting to now is an effort to be as precise and specific as we can try to be, both as to the precise numbers of organisms coming through and precisely what kind of condition they are going through. The biggest problem we have with this is that we are handling a very non-homogeneous, nonrandom distributed organism and the upshot of that is that to get at these kinds of numbers we have been learning that we have to apply more and more manpower and sampling effort and sampling design and location of nets, et cetera, to try to accomplish those objectives.

So it is a developing field of research just like electronics or the space program or anything else. You know, we have to live with what stage we are at this place in time and make adjustments on that basis. That is what we are doing.

MR. MACBETH: Mr. Chairman, could I ask Dr. Lauer a few questions about the --

CHAIRMAN JENSCH: Excuse me, just a minute.

MR. BRIGGS: I would like to find out whether I understand the answer to what I think was the original question. I believe the original question dealt with some 10 minutes of sampling on one particular night and as I understand it, the data that were taken are used in table 19 here for the intake concentration.

Now I understood you to say that the samples represent the intake concentration on that night and that that is essentially what one got from them; is that right?

WITNESS LAUER: That is generally right. I said most of all they reflect concentrations during the time period from which the samples were taken.

MR. BRIGGS: Yes.

WITNESS LAUER: And then with some lesser degree of certainty you could extrapolate this to the concentrations characteristic of that evening or night and then beyond that you get less and less certainty as to what they represent.

MR. BRIGGS: All right. Thank you.

CHAIRMAN JENSCH: Excuse me. Just a minute.

DR. GEYER: Dr. Lauer, it seems like what is at issue here is the value for the F-2 for intake concentration; is that based just on one night's sampling?

WITNESS LAUER: I think Dr. Lawler -- since this is his project, I think he would answer the question as to what went into these tables. I didn't prepare them and had no direct role in preparing them.

DR. GEYER: I understood at the outset you said some of your data were used in developing these two numbers for intake concentration but it wasn't clear then whether that was all the data and it seems what we have been talking about all this time is just some samples taken on the night of

July 25, 1972.

WITNESS LAUER: I think since it was not the product of NYU, these tables, Dr. Lawler would be more appropriate to question [redacted] as to whether or not there was additional foundation forthe F factors.

DR. GEYER: May I ask Dr. Lawler the question then?

WITNESS LAWLER: Dr. Geyer, these are -- this analysis on table 19 does represent data taken only on [redacted] July 25th. The reason forthat is that this was the only set of data that we had at the time this testimony was prepared where there was simultaneously samples taken in the river as well as in the intake and discharge.

On the question of the number of fish taken during the nighttime sample, I would have to check the data in the computation procedure I used, the data that I used did not appear on the precise sheet that you have seen here today. It was -- I am not suggesting it was a different set of data, I am simply suggesting that at the time I received this data, which was several months ago, it was on another piece of data paper. So I can't identify instantly that the pieces of data used to compute, for example, the intake concentration, were at night or in fact what has been discussed here.

DR. GEYER: I realize these fish have grown up by now, but are there any other data you could confirm this with

now?

WITNESS LAWLER: You will note throughout this testimony I have used the expression current, best estimate and I use that for a very definitive purpose knowing that as time goes on, as Dr. Lauer has just indicated, additional data is being developed. I don't at the moment know whether I have additional simultaneous intake and river data. We do have it for later on in the year, but once you get past this period you see virtually nothing in the intake.

DR. GEYER: Yes. It seems to me this is already kind of late perhaps?

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CHAIRMAN JENSCH: Well, just to follow that up, would you check your additional data to answer Dr. Geyer's question? You said it is still on a data sheet you would have to review in order to answer his question. Would you do that?

WITNESS LAWLER: Yes. I will be able to do that. I rather doubt I can do it today.

CHAIRMAN JENSCH: I don't think there is any great hurry to do it. At your convenience.

Go ahead.

MR. MACBETH: I would like to proceed with Dr. Lauer about the research effort and knowledge about entrainment.

BY MR. MACBETH:

Q Dr. Lauer, are you familiar with the Hudson River Fisheries investigation conducted between 1965 and '68, and the report made by Carlson-McCann?

A (Dr. Lauer) I am, from having read it. I was not here in the Hudson River area doing research on the Hudson River at the time those studies were done.

Q And did that study concern itself with striped bass in the Hudson River?

A Among other fish, yes.

Q There's quite a lot of information in the report on striped bass, is there not?

A That's correct.

Q And did that report concern itself with the withdrawal of nonscreenable sizes of striped bass from the Hudson River?

A In a general generic sense, yes.

Q Let me read you a few lines from the conclusion of Carlson-McCann and see whether this refreshes your recollection as to a concern about entrainment. This is from page 45.

"Large numbers of eggs, larvae and young-of-the-year striped bass would be withdrawn from the Hudson River estuary like pumping a proposed hydraulic plant at Cornwall."

Now is the withdrawal that is being discussed there the same as the entrainment at Indian Point except for the fact that heat is added to the water at Indian Point that would not be Cornwall?

A No.

Q Could you describe the difference between withdrawing the eggs, larvae, and young-of-the-year striped bass at Cornwall and at Indian Point that indicates the difference between that withdrawal at Cornwall and entrainment at Indian Point?

MR. TROSTEN: Would you read the question back, please?

(The reporter read the pending question.)

MR. TROSTEN: Mr. Chairman, Dr. Lauer has

indicated that he was not -- did not participate in the Hudson River Fisheries investigation study. He did not prepare that report. I don't think it is appropriate for Mr. Macbeth to be cross-examining Dr. Lauer about the operation of the Cornwall pump storage project and how entrainment through a pump storage plant differs from entrainment through the Indian Point Nuclear Power Plant. Dr. Lauer has never professed expertise with regard to the operation of a pump storage plant. I think the question is improper.

MR. MACBETH: Mr. Chairman, Dr. Lauer offered us his opinion as to what the state of knowledge was about and the concern in the textbook community about entrainment through a plant such as Indian Point, and I just would like to clarify in what way Dr. Lauer thinks that entrainment is significantly different from that at Cornwall since he says he has read the Cornwall report and the Cornwall report, at least to my fair reading, indicates there is a great deal of concern about withdrawal at Cornwall. I would like to see whether Dr. Lauer thinks there is a significant difference between that kind of withdrawal and entrainment at Indian Point. He just indicated that he thought there was some significance.

CHAIRMAN JENSCH: That was what I had understood, he had interjected that thought showing that he understood there was a difference between those two.

MR. KARMAN: Mr. Trosten, maybe you better not use

the microphone.

MR. TROSTEN: Dr. Lauer did not inject a thought into this. He was asked a question and he answered the question. It is quite clear that Dr. Lauer is not professing expertise with regard to the Cornwall plant. He was addressing his earlier remarks, as the record will clearly indicate, to the level of concern with regard to entrainment through steam electric plants. He was not addressing his remarks to the -- to concern having to do with pump storage plants.

If we get into the Carlson-McCann-Cornwall report, as Mr. Macbeth is suggesting, there are many aspects of this that are going to lead us astray and waste a lot of time. The Cornwall report concluded there was no problem with entrainment. Are we going to start discussing whether Dr. Lauer agrees there was no problem with entrainment in the Cornwall pump storage plant? No. Obviously we shouldn't let this hearing degenerate into a discussion of whether Dr. Lauer agrees with the Carlson-McCann report. He was not participating in it, he never professed to have any expertise in the development of that particular report and so on. I think this is an improper line of questioning.

MR. MACBETH: Mr. Chairman --

CHAIRMAN JENSCH: Excuse me for a minute. I wonder if I understand what you are saying. I didn't understand that he was being asked if he had expertise in the operation

of any plant whether it is pump storage or nuclear or fossil fuel. I understood the question was, as the implied premise of the question was, that if you are pulling a lot of water by some pump through a screen, are you going to have some entrainment problems and haven't you ever heard of it having occurred before? Your recent discovery, as you stated, was in 1970 and '71, and I understood this question was no matter who pulls the water, how you pull it, if it is the same kind of pumping activity, whether for a pump storage, fossil fuel, or what-not, the problem really is, is this the first time you ever heard of entrainment on fish eggs and larvae along about 1970 and '71, with the Hudson River Fishermen's study outstanding?

Is this the first time you heard of it?

WITNESS LAUER: That's not what I said.

CHAIRMAN JENSCH: What did you say?

WITNESS LAUER: I said it was the first time I ever heard of it in 1970. I said that's when ~~term~~ the as it's now being used in this context really began to find its way into the literature in a significant way. That's when also at -- you got back to the real point, I think. The concern about entrainment is a volume-dependent concern, and, you know, outboard motors running up and down the river entrain water. To the extent that they do, they are going to be killing fish eggs and larvae and other organisms, probably.

CHAIRMAN JENSCH: You are a -- you have professional expertise in that?

WITNESS LAUER: I don't. I was talking about this being a volume-dependent nature. It was also about that time, 1965, '6, '7, on up through there that many of these much larger fossil fuel and nuclear plants began to come into existence and with their coming into the existence, and the obvious result being that they were requiring much, much larger proportions of the water of a cooling water source stream than previous size plants. I think that was one of the things that is expressed in the literature as having started to raise concerns amongst people about entrainment effects.

As long as the amount of water used was small, relative to the cooling water source body of water, it was generally written off as probably of no real significant concern. As that has changed and the volumes have gotten greater, relative to the larger sizes of the plant, the degree of concern has increased.

It was in that light that -- from reading the report that seemed to highlight the interest with the Cornwall report. It was going to represent a withdrawal of a very large volume of water and so it also appeared to be a volume-dependent concern.

CHAIRMAN JENSCH: Let me ask you, you used a term that may have been a qualification. You said it was along

about '70 and '71 that this entrainment or volume-dependent factor became a part of the responsible literature or some such term. Are you distinguishing that this report is not responsible literature and that it need not have been given concern back in '65 or '68 when it was issued?

WITNESS LAUER: No. Well, the report wasn't issued then. The report actually came out since I have been at NYU. I think it was written about '69 and came out shortly after I came to NYU, which was in '69. The report became available in late '69 or early '70, is my recollection.

CHAIRMAN JENSCH: There was no literature prior to '69 or '70 about fish impingement, entrainment,or volume-dependent activity on fish?

WITNESS LAUER: There was a little bit, but it was very minimal. I think what was in existence at that time was summarized very well in a literature review by Dr. Coutant at that time, which appeared, I believe, 1970 in Chemical Rubber Company Review Edition.

CHAIRMAN JENSCH: I just don't have that issue here.

WITNESS LAUER: I don't, either. I am telling you that's what it was in. At that particular time there were very few specific references that could be made to results emanating from studies designed specifically to determine the effects of passage through power plants on the organisms. There were a few, but to a large extent it had to do with

what one might infer from general literature information on temperature tolerance, turbulence and pressure, et cetera.

CHAIRMAN JENSCH: Probably the best data, then, was what happened at Indian Point No. 1 in 1965, when they had that fish kill?

WITNESS LAUER: That didn't have to do with entrainment, sir.

CHAIRMAN JENSCH: Impingement. Well, it indicated a problem that might be of concern. Were you familiar with that?

WITNESS LAUER: They are totally different kinds of problems. The entrainment is in no way the same kind of a physical, biological problem as is the impingement one. The impingement situation purely has to do with the fact that screens are there of a certain mesh size and they will collect anything in the water that comes against the screens. This is one kind of an interface of a power plant with aquatic life.

The entrainment interface with aquatic life is quite a different thing. It is affected by the fact there is a screen out there keeping larger organisms out and limiting the problem to the smaller organisms that can pass through the screen. But other than that, it has no -- it has no particular relevance to the kinds of experiences that an organism is exposed to while it is going through the plant, compared to the kinds of experiences an organism presumably

goes through when it is collected on a screen, an intake screen. There are really quite different kinds of generic problems we are talking about here.

CHAIRMAN JENSCH: Yes, but the only point, either one or both of those things is to see whether a problem of distribution of the fish or larvae wouldn't have been a concern where they were in the river. You say the best -- there wasn't much in the literature before '69 or '70, but you certainly started with something in '65 to wonder where the fish are that are coming into the Indian Point plant. And I just am puzzled that it's taken so long to be concerned about distribution of fish when you had actual evidence. Maybe it wasn't getting out from these different fossil fuel plants throughout the nation and getting into the literature, that this was happening, but you certainly had a pretty concrete example right at hand to kind of, it seems to me, suggest that it would be a good idea to know where the fish are in the river, and when they are going to be there. I take it you didn't feel the evidence justified that?

WITNESS LAUER: I wasn't here at that time.

CHAIRMAN JENSCH: I know that. I am asking for your opinion. There wasn't enough evidence to justify the concern?

WITNESS LAUER: There were some studies started in 1965 having to do with distribution of these larger sized

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fish, two and a half inches and above. They were started in '65 because NYU did some of them before I came to NYU. Most of them involve seining studies. Then as that problem developed and became more crystallized, there were some very substantial studies. Those studies grew over the years, but then there were very substantial studies undertaken with the Raytheon ~~Ratheon~~ Corporation to look further into this using various kinds of gear type.

Then it takes time to get those studies completed and out and into the literature. As indicated with this Cornwall report, it was started in '65 and the results came out in '69. It is unfortunate that these kinds of things take time, but from our point of view who are doing the work, probably, they are not taking enough time. From the point of view of the decision-makers who have to weigh these things and make decisions, I am sure you feel they are taking too long.

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CHAIRMAN JENSCH: It isn't that. It just seems to me it weighs on the recommendation of the Applicant that they want some more time. They say that this recommendation by the Staff can only be adequately tested if there are more data for which they need more time. So, in measuring the comparative confidence in the isolated incident of this recommendation for more time as contrasted with the recommendation for closed-cycle cooling system, you have to evaluate just how well they have gone to the problem earlier.

MR. MACBETH: Mr. Chairman, I would like to return for a moment if I can to the state of concern of the technical community and state of knowledge about withdrawal of organisms over the last several years.

BY MR. MACBETH:

Q Let me read --

MR. TROSTEN: Have we got a ruling on the validity of this line of questions? I have objected to it for the reasons I gave.

CHAIRMAN JENSCH: I think he has withdrawn the previous question and is going to state another one.

MR. TROSTEN: All right.

BY MR. MACBETH:

Q I will read to you the last paragraph of the Carlson-McCann Report and ask you whether that indicated in the technical community a level of concern about withdrawal of

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nonscreenable organisms by power plants.

"Operation of other plants requiring large volumes of water in combination with the Cornwall Plant could destroy sufficient numbers of the nonscreenable life stages to adversely affect subsequent populations. These effects could be particularly severe if plants were constructed in areas of high fish concentrations. Coordinated studies of fish distribution at future and existing plant sites should be made for an evaluation of their accumulative effects on the important fisheries of the estuary."

MR. TROSTEN: I object.

CHAIRMAN JENSCH: We haven't heard the question,yet.

MR. TROSTEN: Yes. There was a question, Mr. Chairman. He read a portion from the Cornwall Study and asked whether Dr. Lawler -- Dr. Lauer agreed this indicated a level of concern in the technical community with the withdrawal of water by power plants. That was the question essentially and I object to the question on the grounds he is asking Dr. Lauer to express an opinion about a report which he did not author. It is a highly speculative vague sort of inquiry which I feel is objectionable and I think that the Chair should rule it out of order.

MR. MACBETH: Dr. Lauer offered us his opinion as to the level of concern in the technical community on the entrainment of nonscreenable organisms in the Hudson River and

in the U.S. generally.

I was -- I have asked him whether -- what this particular paragraph, and the concluding paragraph in this report indicates about the technical concern at the time the report came out. It seems to me that if Dr. Lauer is competent to give us his opinion as to what the level of technical concern was, that he ought to be able to comment on what this indicates. I am not asking him what the underlying bases of all the data in this report are, but simply whether this kind of conclusion doesn't -- whether it indicates as to the level of -- the level of concern of the technical community on the withdrawal of nonscreenable organisms from the Hudson estuary.

MR. TROSTEN: May I speak to this, Mr. Chairman.

CHAIRMAN JENSCH: I would like first of all to get precisely what the question is. I didn't quite get the same impression Applicant's counsel did. Maybe it is there, but your ultimate objection --

MR. MACBETH: It is there. Could we ask the Reporter to read it back.

CHAIRMAN JENSCH: I had the impression Dr. Lauer had given us an assessment of what the technical literature was prior to 1969 or '70 and I understood the question was really, did he agree with this conclusion in comparison with his previously expressed opinion about the --

MR. MACBETH: That wasn't quite the phrasing. Could the reporter read it back?

(The reporter read the record as requested.)

MR. TROSTEN: Mr. Chairman, having --

(BOard Conference.)

CHAIRMAN JENSCH: Go ahead.

MR. TROSTEN: Mr. Chairman, I think this question is clearly objectionable. The Carlson-McCann Report is there for everyone to read. It says what it says. For Mr. Macbeth to ask Dr. Lauer whether he agrees that this particular paragraph taken in isolation means that the technical community had some concern or indicates a level of concern, is to ask a completely vague and ambiguous and conjectural question that is improper; and it should not be allowed.

I think as I say, the report speaks for itself. Dr. Lauer did not author the report. The report says what it says and to ask a witness whether he thinks that this report indicates a level of concern in the community is wrong. If there was a concern, it was this; if there was not a concern, it was not this. The report speaks for itself. To ask him whether he thinks this passage indicates a level of concern in the technical community, I think is all wrong.

MR. MACBETH: Well --

CHAIRMAN JENSCH: Excuse me. As I try to get the question on its reading, the witness is being asked does this portion

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of the McCann Report reflect a concern in the technical community; and I think the gentleman has already indicated that he has given us an appraisal of what he felt was the literature in this field for some years even though he wasn't at NYU; he wasn't on the Cornwall Report; but he is familiar with it. I think he is just being asked in view of that statement, as I understood his previous answer, that there really wasn't much going on and it hassuddenly been thrust upon them, low and behold, Calvert Cliffs and the rest of it. I think this is just kind of testing what he has told us previously that there really wasn't much going on or nobody was concerned.

MR. TROSTEN: Well --

CHAIRMAN JENSCH: Objection overruled. Witness may answer.

WITNESS LAUER: First of all, I think my response was characterized by Mr. Macbeth as saying that I indicated there was no previous technical concern either by people -- something to the effect in the Hudson River or around the Hudson River or nationally. I never said any such kind of statement. I referred to the level of technical concern that was evident in the available public literature in that time period in late 1969 to '70 and I think my response to the question is wholly compatible with what I said before. When I came to NYU, NYU sponsored a symposium on Hudson River ecology in

September of 1969. I was the person who was responsible for making all of the arrangements and getting the agenda put together and subsequently it resulted in a proceeding of the symposium.

That was where the conclusions of the investigators involved in the Cornwall Report first reported their conclusions. It later came out in this separate published form in 1970.

So, this is wholly compatible, the timings that I just referred to, are wholly compatible with my previous answer.

CHAIRMAN JENSCH: I don't think that is the question before you. They are not asking you to justify what you said before. They said does it reflect a concern in the technical community, yes or no.

WITNESS LAUER: Well, it does.

CHAIRMAN JENSCH: Thank you. Thank you.

Next question?

MR. MACBETH: Thank you.

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(Discussion off the record.)

CHAIRMAN JENSCH: We have had a moment of off-the-record discussion and perhaps it should be noted now that we are back on the record that some inquiry has been made as to how long further cross-examination would be, and the Hudson River counsel has indicated perhaps an hour and a half more on his planned interrogation.

I think as I said to the applicant's counsel at one of our last sessions, I think that all parties should undertake the length of cross-examination that they feel is necessary for their point. We are here to sit until that's done.

I am sure that's all the applicant's counsel wanted to know, was for scheduling.

MR. TROSTEN: Absolutely, Mr. Chairman.

CHAIRMAN JENSCH: Proceed, Mr. Macbeth.

BY MR. MACBETH:

Q Let me read you a sentence from the page 6 of the Carlson-McCann report where it says, "Distribution studies were limited to eggs, larvae and young-of-the-year because older striped bass could avoid the screens --"

CHAIRMAN JENSCH: Not quite so fast, please.

BY MR. MACBETH:

Q I will begin at the quotation again: "Distribution studies were limited to eggs, larvae, and young-of-the-year

because older striped bass could avoid the screens at the intake of the proposed plant."

Dr. Lauer, does that quotation indicate to you that in 1965 when the Carlson-McCann study began that there was some concern in the technical community about withdrawal of nonscreenable sizes of striped bass from the Hudson Estuary?

MR. TROSTEN: I object to the question for the reasons I have given before, Mr. Chairman. I think this is going to lead us into a highly speculative and improper inquiry here.

CHAIRMAN JENSCH: Objection overruled.

MR. TROSTEN: Mr. Chairman, would the -- would Mr. Macbeth tender to Dr. Lauer the documents so that Dr. Lauer could read the statement in context before he responds?

CHAIRMAN JENSCH: I think that should be offered.

MR. MACBETH: Certainly.

(Counsel handed documents to witness.)

MR. TROSTEN: Dr. Lauer - would the reporter read the question back?

(The reporter read the pending question.)

WITNESS LAUER: I am not really too sure whether I even want to answer the question in light of the fact that in the arguments earlier Mr. Macbeth was obviously indicating he was trying to bring out a contradiction in what I had said

previously compared to what this document says and you cut me off short saying that I should just answer the question.

I don't think that I am willing to answer the question unless I can answer it fully in the context in which it is being asked.

CHAIRMAN JENSCH: Oh, you may do that. Sure, give your explanation. I thought you were trying to justify what you felt was a contradiction and I didn't think the question asked for that. It just said do you think it reflects a concern in the technical community.

Do you think it does? Then you can explain it.

WITNESS LAUER: I think Mr. Macbeth paraphrased what I said and said I am asking this question. I think I ought to be allowed to ask this question in the interest of showing that whereas he says the concern was exhibited in the literature in '69 to '70, this indicates it was earlier than that.

CHAIRMAN JENSCH: If you will -- my point is answer the question directly and then explain it any way you desire.

WITNESS LAUER: Okay.

I think this is contained in the abstract of this paper and it does indicate the purposes for which the study was set out and it also summarizes the results of those studies and I would say in general that based on a statement

of objectives, one of the concerns that was desired to be answered had to do with the matter of what effect there would be on eggs and larvae and other organisms that would be withdrawn from the river.

This is -- this was a statement of concern at the time they proposed the studies and it was appropos the large volumes of water that I talked about. This was not in the public literature until '69 or '70. I think it absolutely does reflect the concern of these individuals who were framing up the study about the possible effects of withdrawal of organisms into that plant.

I would also like to say that they further indicate that their studies on distribution provided bases for drawing some conclusions which were to the effect that the plant would have negligible effects on fisheries of striped bass and other species occurring in the estuary, but the next point is really the important one, having to do with the stage at which we find ourselves in looking at this as a scientific community.

That is as follows: "If the plant were constructed, further studies would be required to determine the actual numbers of life stages entering the plant."

I only read this to indicate that this is a reflection of the fact that we do things by staging in time and you do what you can do to get a piece of information to

hopefully address itself to a set of hypotheses or concerns and then decisions are made based on that data and then subsequently they are saying there will have to be additional studies done to in a sense validate those conclusions.

I think this is a normal scientific research logical process that needs to be followed.

CHAIRMAN JENSCH: May I ask you this: You said there was not much in the public literature. I wonder if you were making a distinction there was other literature that was known to people? For instance, I say this without any criticism of any operator of a fossil fuel plant or any other operator of a plant that's pulling a lot of water out of a body of water, but were there other pieces of literature which reflected concerns, maybe those pieces of literature were not generally available to the public, but were you making a distinction between public and private literature in your answer?

WITNESS LAUER: If I did not say so, I was addressing myself to the scientific literature with which I am familiar. I wouldn't have any way of knowing for example in this specific interest the Cornwall proposal, whether or not articles appeared in The New York Times or other of the public literature.

I was referring to the scientific literature that

represents the state of the art as far as the scientific community is concerned.

CHAIRMAN JENSCH: You know of nothing of a substantial nature in the technical literature prior to '69, '70 about the withdrawal of larvae and eggs from large volumes of water?

WITNESS LAUER: There was very little. There were two reports of any substance. One was authored by Kerr back in the 1950s having to do with an operation of a power plant in California wherein for that particular stage of the game the conclusions raised no real source of concern. They concluded that everything looked pretty good; and so that in itself would not have aroused additional concern.

Then there was another paper which I referred to before -- or there was work going on which was generally known to exist within the scientific community prior to 1969 and '70 that had to do with the effects on larvae going through the Connecticut Yankee plant on the Connecticut River although there wasn't much coming into the literature subject to the date I mentioned.

Those were really the only two papers of any consequence having to do with the passage of -- through the plants of fish eggs and larvae that existed prior to the time, 1969 to '70 that I am talking about. In fact there is not much right now. There is quite a bit of work going on,

including what we are doing, at various power plants, but it hasn't reached the scientific literature yet but it should be within the next few months to the next year.

CHAIRMAN JENSCH: Is this a good place to interrupt?

MR. MACBETH: If I could have a couple of quick questions? Some of the answers are longer than I anticipate.

BY MR. MACBETH:

Q The concern expressed in this Carlson-McCann report obviously was not in the scientific literature but it was a concern expressed about striped bass in the Hudson River, was it not?

A (DR. LAUER) Well, it was a concern that these people apparently had when they set up the program to go about determining the distribution of eggs and larvae in the river. The expression of that concern didn't come out until the proceedings of the Hudson River's Ecology Symposium and this document, both of which came out to my recollection in 1970.

Q But they did concentrate their study on striped bass in the Hudson River, is that correct?

A That's correct.

Q And the report was made for a plant that is also owned by Consolidated -- if ever built, will be owned by the

Consolidated Edison Company, is that correct?

MR. TROSTON: I object to that.

MR. MACBETH: Would the applicant's counsel stipulate that the Cornwall Pump Storage Project, if built, will be owned and operated by Consolidated Edison?

CHAIRMAN JENSCH: What is the relevancy of that?

MR. MACBETH: Just to show while there may not have been concern in the scientific literature, there was concern about these fish in this river that was reported to this company. Whatever vague knowledge there may have been in the scientific community generally, this company, on this river was concerned about these fish and has been since 1965.

CHAIRMAN JENSCH: That's a different question than you have propounded.

MR. MACBETH: I was just pointing out that this study was done in connection with the Storm King plant and done for Consolidated Edison.

CHAIRMAN JENSCH: Was Consolidated Edison participating in this 1965 report?

MR. MACBETH: Yes.

CHAIRMAN JENSCH: That's a different question.

MR. MACBETH: I was moving to it a little more slowly.

CHAIRMAN JENSCH: Let's take this present question. Objection sustained.

MR. MACBETH: Well, I don't think it would be appropriate to ask Dr. Lauer about the participation of Consolidated Edison in this study unless he has particular knowledge of it.

BY MR. MACBETH:

Q Do you know what participation of Consolidated Edison in this study was?

A Which study?

Q The Carlson-McCann study.

MR. TROSTEN: I object to that too. The witness has said he doesn't know.

MR. MACBETH: All right. I'll save that for another witness at another time.

CHAIRMAN JENSCH: Is this a convenient place to interrupt your cross-examination?

MR. MACBETH: Yes.

CHAIRMAN JENSCH: This is a little later than we usually recess but perhaps gives us a chance to get to an eating spot without so much delay.

What time is suggested for the recess?

MR. MACBETH: Would 2:15 be good?

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

(Whereupon, at 12:45 p.m., the hearing was recessed, to reconvene at 2:15 p.m., this same day.)

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AFTERNOON SESSION

(2:15 p.m.)

CHAIRMAN JENSCH: Please come to order.

Is Dr. Lauer and Dr. Lawler here?

They are here.

Are you ready, Hudson River Fishermen's Association?

MR. MACBETH: I am.

CHAIRMAN JENSCH: Proceed, please.

Whereupon,

GERALD J. LAUER

and

JOHN P. LAWLER

resumed the stand as witnesses on behalf of the Applicant and, having been previously duly sworn, were examined and testified further as follows:

CROSS-EXAMINATION (Continued)

BY MR. MACBETH:

Q Dr. Lauer, I would like to turn to table 19 in Dr. Lawler's testimony to pursue a few other points.

How did you convert from the meter reading on the -- in Exhibit II to the concentration in a thousand cubic feet -- per thousand cubic feet?

A (Dr. Lauer) Each meter when purchased has a calibration curve which comes along with it indicating the number of revolutions turned relative to a given volume of

ar2

water passed through the meter. In addition, we go through a procedure to recalibrate these meters in order to be sure that the calibration stays the same.

Having this calibration in hand, we can then take the meter readings, convert those to water volumes passed through the meter itself, then this is extrapolated to the diameter of the net and volumes of water are thereby calculated for having passed through the net.

Knowing these volumes of water calculated to -- that are passed through the net and the numbers of specimens collected in that sample, it can then be -- the abundance per thousand cubic meters or any other given number or volume can be calculated and were calculated with this information in hand.

Q Could you give us the function by which the meter readings multiplies to produce the figure per thousand cubic feet? Obviously the thrust of my question is we ought to have them provided with the meter reading numbers and we would be interested in having them make a few calculations ourselves on volume, and we just need that function to be able to give a few conversions.

A I can't give you that offhand. It does vary with each meter. You have to take each meter into consideration to calculate this for each sample so that there is no absolutely correct magic number. But I think I could give

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you an approximation as to what a revolution or a unit number of revolutions would generally calculate out into in terms of volume of water filtered. I can't do that off the top of my head, but that information could be drawn.

Q I'd appreciate it if you would supply it just so we could make calculations of that sort.

I would just like at this time to put a few questions about table 19 to Dr. Lawler so we could get clear a few of the basic terms here. I don't want to leave Dr. Lauer, but I think while we are on this, it might be useful to do it.

Could you perhaps draw on the easel, Dr. Lawler, where the tows marked east and east channel were taken in relation to the intake to Indian Point 1?

A (Dr. Lawler) This represents the Hudson River. This is flow downstream. This is the Indian Point intake (indicating).

The transect marked east channel was in this general vicinity. I forget how many yards offshore it was, but I can get that estimate for you.

Q I'd appreciate that.

A And the transect marked east was taken right in front of the plant.

Q That would be within literally a couple of feet of the dock?

A Well --

Q Within 10 yards?

A I'll say for the moment within 100 feet, and check that distance also.

Q Thank you.

Now you have terms on this table, east shore and east quadrant. What is meant by east shore?

A Oh, the expression east shore corresponds to the east transect.

Q And east quadrant?

A That would correspond -- I'll have to check this point also, but I am pretty sure that corresponds to the average of the east and east channel samples.

Q Again I would appreciate it if you would check that.

A I am almost certain of that, but I will check both points.

Q On what days were samples done of -- for the data which are reflected under the headings east and east channel in table 19?

A As I indicated before, the data here refer to the day of July 25th.

Q And what time were the samples for east and east channel taken?

A These samples were run around the clock, 24 hours.

Q Let me understand that. Going up and down these

transects, continuously all day long?

A That's correct. The samples were taken approximately every two hours.

Q And at what point did you divide day from night?

A Generally I divided day and night from 9:00 o'clock at night to 6:00 in the morning was nighttime; and from 6:00 o'clock in the morning until 9:00 o'clock at night was daytime. It is conceivable for the later -- since this was toward the end of July, I may have used 8:00 o'clock to 6:00 in the morning and that may explain the computational procedure that was used here. This is what I indicated to Dr. Geyer before that I would check. I may have used the 8:00 o'clock sample as well as the midnight sample in computing the intake concentrations that are shown here.

Q Again I would appreciate it if you would --

A Yes. I will check all of those points. I don't have them in front of me.

Q Dr. Lawler, on the previous page you say that -- page 60 of the testimony, the data reported in table 19 in terms of total serranoid, white perch, striped bass, because the number of striped bass caught was too small to perform any valid analysis, how many striped bass were caught?

MR. TROSTEN: Striped perch?

BY MR. MACBETH:

Q Excuse me, striped bass.

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A (Dr. Lawler) I don't know the exact number, Mr. Macbeth, but I know it was quite small. Many of the samples did not contain striped bass.

Q Perhaps if you could provide that data as well. I am just a little curious as to what the actual numbers were.

Did you take any tows below 20 feet?

A Generally we towed below 20 feet, but in this particular run I don't think tows were made below 20 feet. Again I could check that further.

Q Do you know the efficiency of your gear, what it was?

A No, sir.

MR. TROSTEN: May I ask a question of Mr. Macbeth? When you use the -- and it is for the purpose of clarifying this and succeeding questions -- when you use the term "what was the efficiency of your gear," efficiency compared to what? Are you using it in some absolute sense? I am not sure whether we are all thinking of this in the same sense, and I think it would be helpful for the record if you could clarify that.

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MR. MACBETH: Well, it is my understanding that when you sample for organisms of this size you can do comparative tests with different gear at different speeds. You have some indication of how efficient the particular speed is that you have chosen to use.

Now I am not sure technically and exactly what -- in what terms that kind of result is expressed but that is what I am aiming at: Is there any kind of control or measure to see whether tows were made at a faster speed or with a different size of mesh, different results would be produced?

As Dr. Lauer pointed out this morning, the low number of yolk sac for -- this is probably due to the fact they were simply passing through the neck.

Yolk sac may be very difficult to collect in any case, but it might in that situation of that sort be informative if there were a variety of tow speeds and gear being used so one could see whether the most efficient gear were being used to catch the organisms the experimenter was seeking.

WITNESS LAWLER: Mr. Macbeth, I might add that in answer to your prior question to me that sampling was done at greater depths than 20 feet during that period.

BY MR. MACBETH:

Q Could you provide the data for July 25 at those other

dates?

A (Dr. Lawler) I am certain I can.

Q Thank you.

I assume you don't have it right in front of you now?

A No, I do not.

Q Well, until I have the further information, I would like to turn back to table 16 in Dr -- excuse me, figure 16 in Dr. Lauer's testimony.

Dr. Lauer, how much of the -- what depth -- excuse me -- what depth from the surface do you think is represented by the surface tow?

A (Dr. Lauer) What depth at the surface is representative of the surface, is that what you are aking? I am sorry.

Q No. What depth down from the surface does the surface tow represent? In other words, when figures are presented here surface tow, does that indicate the concentration of fish only at the surface or do you think that that represents the number -- concentration of fish, say, down to 3 feet or 5 feet or what sort of part of the water column does that two represent?

A When the surface nets are being towed, they are towed such that the upper portion of the net stays just below the water surface and the net is 22 inches in diameter so the

net is sampling water from within an inch or two below the surface to a depth of 22 inches below that. That is the thickness of the water layer that the surface nets are sampling.

Q And would the bottom tows, what part of the water column in relation to the bottom of the river is represented by the bottom tows?

A The nets are towed in such a way and the geometry of the line is let out in such a manner that the bottom nets are towed at a depth approximately 2 feet off the bottom. This is accomplished by letting out a depth of line approximately three times the length of the depth of the water at that site and the net itself is fastened at a length 6 feet above the end of the cable, the depresser at the end of the line, so that the end result is that the bottom net is approximately 2 feet off the bottom.

Q Now in figure 16, in part of the six cases represented there, there is a greater abundance of organisms at the bottom than at the surface and the mid-depth point falls somewhere between those two extremes.

Is it your opinion that if one had an analysis of the entire water column, one would see an even gradient from surface to bottom?

A I don't think the gradient would be a smooth as that line because of the variability of the abundance and distribution of larvae within and among samples so that the

points would not fall -- I wouldn't expect them to fall precisely on thatline to be that smooth.

I would expect that the general relationship of abundance increased with depth would apply. It may not be exactly in conformance with that line.

Q Well, let me see how general that statement is.

Do you think that in -- that it may be the case that there is a clustering of organisms, a greater concentration of organisms in the bottom area than in the mid-depth? And perhaps to put it a different way, that the gradient would not be smooth but would show a rapid increase in the, say, the last 3 or 4 feet from the bottom?

A I think the information of the data that we have indicates that that is very probably true. It is probably true. It is probably that the abundances are considerably greater at a depth between the 2 feet off the bottom and the actual bottom than they are above. Part of the reason for saying this is that when one calculates the total mean abundance over the water column for most of these organisms that show this diurnal distribution difference vertically, you come up with quite a lot higher mean abundance values for the nighttime compared to the daytime samples, and since this is even true for samples during a nighttime immediately following the previous day, and it is improbable to suppose that these organisms are generating that fast on a diurnal

basis to cause those differences in numbers, it seems probable that there is quite a lot higher concentration of those organisms right down near the bottom, closer to the bottom than we can sample with this kind of gear.

Q Would it also be true that during the daytime there are considerably fewer organisms right at the surface in the first foot or two of water at the surface than there would be in the water immediately -- again that the gradient would show a real change in the last 2 feet down from the surface?

A Well, I think the data indicates that there is -- one could expect an increase in abundance with depth below the surface samples generally increasing toward the mid-depth and on down toward the bottom.

Exactly what the rate of increase would be with increase in depth would show up if one had a sample -- had a series of samples at each finite depth I don't know but as I indicated earlier, I would expect in general it would follow this kind of a progression if looked at on a seasonal abundance basis which is what is represented in this figure 16.

Q Do you think you could use the surface net catch to estimate the density of larvae or young juveniles more than 2 feet below the surface?

A Not unless you knew what the relationship was of the surface abundances to depths greater than surface.

For example, the mid-depths and the bottom depths we have. If you only had surface data, you would come out with a much lower estimate of the population abundance in the vertical water column than if you have data relative to depths. It was because of these kinds of general findings by others and the results from our own sampling that we instituted a program sampling at more than just the surface so we could better chracterize the distribution of the larvae in the river during the daytime and also at night.

Q Earlier this morning you were descirbing the phototactic effect onthe river. That led me to think that perhaps in that first literature -- where the light would penetrate -- it was -- less abundance of organisms than a foot or two further, say, at 4 or 5 feet.

Would that be true?

A Well, I don't think there is any point in theorizing any further about this than the data indicated on figure 17. I think that is as firm an indication of vertical distribution difference relative to day and night as we can come up with based upon the data we have. I think that indicates a very obvious difference in vertical distribution between the day and the nighttime.

I responded to your question having to do with phototactic response. It is a theory really as to whether or not this is a phototactic response. They are showing a

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vertical distribution difference. I think there is -- it is not known with certainty whether this is due to light intensity or some other factor. It may well be due to light intensity.

Q The change in distribution of larvae from day to night at the surface in figure 16 would be evidence supporting the theory of a phototactic response, would it not?

A It may indicate that it is phototactic response. It doesn't necessarily indicate that thatis a cause and effect relationship, but it may indicate that it is.

End #16

Q Thank you.

MR. TROSTEN: Mr. Macbeth, would you define what you mean by phototactic response. I am not familiar with the term and I think it would be helpful for the record if you clarified what you meant by that.

MR. MACBETH: Well, I have got that from Dr. Lawler on page 60. Perhaps we could just ask him.

He said there the river sampling data -- throughout the 2400 period. This is not unusual since the collections consisted primarily of white perch which do not exhibit the phototactic behavior of the striped bass.

Perhaps -- I just think Dr. Lawler could give us a better description than I could.

MR. TROSTEN: I agree.

WITNESS LAWLER: I am sure that I got the description that you refer to from the literature. My understanding is that the phototactic response which I also understand can be either negative or positive refers to the tendency of some organisms including the striped bass to seek light or to move away from light.

BY MR. MACBETH:

Q Certainly I should have referred to this as a negative phototactic response generally. If we can have the record reflect that I would appreciate it.

DR. GEYER: While we are clarifying terminology in figure 16, it would help if we could put the night dots or

kar 2

triangles or squares in solid black. There is no distinction between the two.

WITNESS LAUER: That is a problem that came out on reproduction and it is also present in another figure which maybe I should point out at this point since it is the same kind of problem.

The dots which were black on the original representing the nighttime sample seasonal abundance information should be on that line beginning at the point of 50 per thousand cubic meters of water at the surface and descending there down to the mid-depth which is approximately 30 per cubic meters and increasing again toward the bottom to approximately 45.

Those should be solid circles rather than having open spaces in there. The other figure where that problem also came up --

DR. GEYER: Let's fix the other two lines on this diagram just so there are no problems.

WITNESS LAUER: Okay. The one as far as the eggs are concerned, the nighttime abundances for the eggs should be that line representing the higher abundances.

DR. GEYER: Right.

WITNESS LAUER: Those triangles should be filled in as solid triangles.

DR. GEYER: The same for the squares?

WITNESS LAUER: I think probably for the squares,

kar 3

but I would want to check that back against the original figure because it is not apparent from looking at them which was the case. I think it would be the higher abundance of the two, but to be sure I would like to check that.

It is just not clear enough that I think I will look back at the original figure and I will do that and report back.

DR. GEYER: Thank you.

WITNESS LAUER: The other figure where that problem showed up is figure 15, page 43, in the bottom panel of that figure where it shows direction or occurrence rather of striped bass seasonally and other fish species, the key -- in the key portion of its peak abundance should have been a solid bar. It was in the original graph and just didn't come through as a solid on reproduction.

Simply up -- start with striped bass at the bottom, as far as the eggs are concerned, the solid portion of that would have been that portion -- these are true all the way up the line, they are the least distinct portions of each of those bars. For the striped bass eggs, this would have been for approximately the middle portion of that graph extending from a little bit later than mid-May to about -- a little bit short of the end of May. That would be the solid bar.

With the striped bass yoke sac larvae, the peak abundance occurred just a little bit later beginning about

kar 4

midway through the solid bar for the eggs and extending through what would amount to approximately the first week of June.

I think the other, the solid bar for the post-yoke sac larvae is apparent. It is the center section that is not filled in but should be. It is delimited by vertical lines across the bar.

DR. GEYER: Right.

WITNESS LAUER: And simply with the white perch there is a little block right in the middle for the ~~white~~ white perch eggs where there should have been blotted in. And for the white perch larvae that is apparent, that is in the middle, the general portion, the middle portion of that bar, that should be filled in and subsequently on up the line.

For the ~~Clupedit~~ Clupeid larvae there is a center portion delimited by vertical lines that should be filled in solid. For the anchovy again that is a broken line along there for the anchovy larvae. The blocked in portion should be approximately in the center of the longevity of those lines for the anchovy.

Proceeding up to the next line, the blocked in portion should be at the left-hand end of that bar starting about a quarter of an inch in from the end of the bar and extending over about one inch in length. That is a vacant or opposite and it is obvious that that was the spot that was filled in and should be so.

kar 5

For the smelt, again the season of peak abundance was toward the left-hand end of that line, between the two vertical lines that bisect the bar, the longitudinal bar, about a half inch in length.

For the silversides, it is approximately in the center about one inch long, blacked in portion.

And for the eel, the peak abundance should have begun, should be blocked in beginning at the beginning of the first bar, beginning in early May and extending approximately half way along the length of that first section or bar for the eel. That is also a broken bar.

DR. GEYER: Thank you.

WITNESS LAUER: You are welcome.

CHAIRMAN JENSCH: Would you continue, please, Mr. Macbeth?

BY MR. MACBETH:

Q Yes, I would like to move on now to the table presented at the end of the testimony where you say that samples of number of live larvae in the intake samples yield 54 percent. Is it first approximation of survival for striped bass, white perch larvae which pass through unit one.

Dr. Lauer, if Indian Point Two were operating at full power with the present cooling system, what would be the approximate delta T across the condenser tubes?

A (Dr. Lauer) Fifteen degress Fahrenheit.

kar 6

Q And could the plant be operated to produce power with no increase o heat across the condenser tubes on the present cooling system?

A I don't feel qualified to answer that question. I am not a power plant engineer.

Q Well, I don't think there is any need to press on that.

Let me show you a chart. This is entitled, "Condition of NESP Striped Bass and Striped Perch Collected Through the Intake and Discharge of Indian Point."

MR. TROSTEN: Mr. Macbeth, are these data provided to you by the Applicant?

MR. MACBETH: Yes, they were.

CHAIRMAN JENSCH: Would you read that title again?

MR. MACBETH: The title is, "Condition of Morone SP (Striped Bass and White Perch) Collected from the Intake and Discharge of Indian Point," chlorine effects data not included.

E # 17

CHAIRMAN JENSCH: Thank you.

BY MR. MACBETH:

Q Is that a tabulation of data which you collected in the summer of the intakes and discharge of Indian Point 2? When I say you, I mean you or people under your control?

A It is.

MR. MACBETH: Mr. Chairman, I would like to offer this chart in evidence as Hudson River Fishermen's Exhibit III.

CHAIRMAN JENSCH: The document which Hudson River Rishermen's Association counsel has just referred may be marked for identification.

(The document referred to was marked Hudson River Fishermen's Exhibit III, for identification.)

CHAIRMAN JENSCH: Having thus been identified and having been previously offered, any objection?

Applicant?

MR. TROSTEN: I would like to see it, Mr. Chairman.

Mr. Chairman, this data having been -- being a summation of data collected by New York University, we have no objection to it being received inevidence. However, we would like to make the same request of having an opportunity to review it to ascertain its accuracy.

CHAIRMAN JENSCH: Well, I think as we indicated before,

anytime any party feels that any exhibit or evidence is subject to a motion to strike, the motion may be made.

Hudson River Fishermen's Exhibit III is received in evidence.

(The document referred to, marked Hudson River Fishermen's Exhibit III, for identification, was received in evidence.)

BY MR. MACBETH:

Q Dr. Lauer, looking at that chart, how many days did you do sampling in the intake and discharge of Indian Point 1 in which the Delta T across the condenser tubes was 15 degrees?

A (Dr. Lauer) One day.

Q What was that day?

A August 1st.

Q What kind of -- what size of fish, what state of development of fish did you expect to find in the vicinity of Indian Point on the first of August, striped bass and white perch?

A In general, this is near the tail end of the striped bass and white perch egg and larvae season. The occurrence of larvae in that portion of the river, I would judge from our field sampling data, and in general the -- these larvae would be those that are probably on the order of half an inch

long. I wouldn't have any comments based upon the juvenile and adult fish. We are sampling the eggs and larvae.

Q Do the larvae become somewhat tough, more resistent to external stresses as they become older?

A It depends on what the external stress is. Some stresses appear to be more effective on the older adult fish and other stresses appear to be more damaging for the younger stages of the fish. So, you would have to specify as to what the stresses were that you are talking about.

Q Let me ask you to draw on the easel the places where the nets were in the discharge channel from which you made collections during the summer. Would you make a diagram of that sort for us?

A There really is a figure already in my testimony which indicates this but I will be happy to draw it for you.

(Indicating.)

CHAIRMAN JENSCH: Well, excuse me just a minute. If you already have it, Doctor, let's see it. Is the chart in the testimony adequate?

MR. MACBETH: I would just like some indication -- it is a rather general chart.

CHAIRMAN JENSCH: I see,all right.

MR. MACBETH: If there could be an indication of how far it is from the end of the discharge channel.

BY MR. MACBETH:

Q Perhaps, if you could tell us the page that chart appears in on the testimony, we could be a little clearer about it.

A (Dr. Lauer) Okay. It is page 5. Figure 1 on page 5.

CHAIRMAN JENSCH: To show the particulars to which counsel just referred, I think that is what he is seeking. Thank you.

WITNESS LAUER: Okay. We are going to have a scaling problem here. I will start over.

This is a schematic obviously. As I indicated before there are fixed fine screens out here and there are the vertical traveling screens back here on each of the intakes.

(Indicating.)

Water from one of these pumps, approximately half the water comes over the surface condenser bank over here and the other half services this condenser bank and vice versa.

MR. MACBETH: Mr. Chairman,I was really only interested in the position of the stations in the discharge channel. I don't mind the witness describing the rest of the system to us if the Board thinks that wouldbe helpful, but it isn't really required as an answer to my question.

CHAIRMAN JENSCH: Maybe he feels he needs it to get

the background.

WITNESS LAUER: I wasn't clear that you were just interested inthe discharge stations.

BY MR. MACBETH:

Q Fine.

A (Dr. Lauer) I can't tell you exactly to the foot what this would be, but in any case, the water goes through these condensers and into so-called water boxes going out of the condensers. It then enters the discharge canal that is under the floor, under the building and the end of the building is approximately in a position like that and the discharge canal then comes open. You can see it from the outside, look down into it. There are concrete girders from one side to the other of this intake canal and looking at Figure 1, at the E-1 designation, we are sampling -- we have our sampling set up located off of one of these girders. We go down a girder and we have one of those rigid frame sampling devices I described in the intake this morning attached to this girder and going down into the bottom of the discharge canal. This is the sampling station designated as D-1.

I don't know exactly what the length of distance is from the building to that girder or from the condenser water box to that girder.

Q Could you give us just a rough approximation?

A hundred feet, two hundred feet? Just so we have a general picture of these distances.

A I would say it is probably approximately about 100 feet from the point where the water comes out from underneath a covered area.

Q Thank you.

A Then --

MR. TROSTEN: Excuse me, Dr. Lauer. There is, by the way, a figure showing all of this, Figure 3-2, Indian Point Plant site lay-out on page 3-4 of the Final Environmental Statement. I don't think it is to scale. Maybe it is.

MR. MACBETH: Well, Figure 3-2?

MR. TROSTEN: That is right.

MR. MACBETH: That fails to locate the discharge sampling stations which is really what I was trying to get located here, at least I don't locate the discharge sampling stations on that figure.

WITNESS LAUER: Okay. Using Figure 1 in my testimony again, D-2 is designated as being located just short of the bend which then goes out into a -- take this and back it back over here. The discharge canal broadens out out here in this area in front of the submerged discharge ports and there are steel girder structures which go from one side to the other across the discharge canal through here.

(Indicating.)

We had built a platform that extends across the full width of the discharge canal here and again a -- one of those rigid steel pipe sampling structures that I described for the intake this morning is attached to the steel girder and extends from above water level down into the -- to the bottom of the discharge canal at this point.

One of the reasons for locating this here is that this area broadens out and velocities drop in there.

(Indicating.)

We located it here where we still have higher velocities for sampling of the nets.

(Indicating.)

BY MR. MACBETH:

Q So discharge station No. 2 isn't all the way to the discharge canal? It is how far from the end of the discharge canal?

A (Dr. Lauer) I don't really know what to define as the discharge canal.

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Q The point at which the water goes through the ports and into the river, how far is it --

A Relative to each port so there is no one answer that can be given.

Q Let's take the first port.

A I don't know. I would say probably -- this is purely a guess -- probably on the order of 150 feet.

Q Again roughly how far is it from discharge station 1 to discharge station 2?

A I am not sure. It is my recollection that the overall discharge canal structure is on the order of 1500 feet long so based on that, a rough approximation may be that the distance between those two may be on the order of perhaps a thousand feet, something like that.

Q All right. Thank you.

Would you now look at pages 10 and 11 of HRFA's Exhibit No. 2 and tell me for the first week of August what the results of sampling at station No. 2 were, how many fish were taken alive, how many dead, and how many taken stunned?

A I am sorry from what date?

Q August 1, 1972, the day on which there was 15 degrees across the condenser tubes.

A For the entirety of the day?

Q For the entire day at discharge station No. 2.

A Well, I think this is going to take some time. If

ty 2

you want to take a rest.

I am going to have to pick these numbers out from a pretty long list of numbers.

(Witness conferring.)

A Okay, based upon this quick look at the situation, it appears to me that there were amongst the samples taken in D-2, one live larva, 13 dead larvae, and one stunned larva on August 1, 1972.

Q Thank you.

Would you now refer again to Hudson River Fishermen's Exhibit No. 3 and tell me how many days you took samples on which there was no increase of temperature across the condenser tubes?

A Well, these would be days for which this data is representative. I am not sure if these are the total days as under the condition you describe.

Q This is all the data you have collated at this point?

A That is right. For that condition where there was no delta T, there were 8 days sampling represented.

Q And what was the total number of organisms taken alive, dead, or stunned at the intake under those conditions with no increase of temperature across the condenser tubes?

A For that period of time which represented the period of peak abundance of larvae which extended from June 18 to June 27, the number of live larvae on the intake was 151,

stunned 35, dead 140.

Q Does that add to 326?

A It does.

Q And how many larvae alive, dead or stunned were taken at the discharge stations at times when there was no increase in temperature across the condenser tubes?

A This doesn't designate that they are from both discharge stations and I think it is correct to say that this represents sampling from discharge 1 station and not from the discharge 2 station. That could stand to be corrected.

In any case, the number of the live larvae in the discharge samples were 118 alive, 31 stunned, and 84 dead.

Q Does that add to 243?

A Is this your exhibit? I am writing on it.

Q Well, I think the Applicant can probably provide us with a clean one.

A That adds up to 233, yes.

Q I have 243. Maybe your arithmetic is better than mine.

A I come up with 233.

Q Okay.

And what was the total number of organisms taken throughout the summer as reflected in Exhibit 3 at the intakes to Indian Point 1?

A I don't know the answer to that other than to say

that up to the point this data was accumulated, that number is indicated on table 9 indicating that there were a total number at the intake of 657.

Q And so that in the course of this series of -- or this one experiment, during the period in which there was no increase of heat across the condenser tubes, 326 of the total 657 organisms taken at the intake stations were collected?

A I am sorry, I didn't follow that.

Q Let me rephrase it.

In the course of the experiments this last summer as reflected in Exhibit 3, a total of 657 organisms were collected at the intake stations. 326 of those were collected at times when there was no increase of temperature across the condenser tubes; is that correct?

A Yes, of the organisms identified on this exhibit, that is true.

Q Yes. And that -- those are the same numbers that apply of course to table 9 on page 51 of your testimony of October 30?

A Yes.

Q And in the course of the summer a total of 399 organisms were taken at the two discharge stations and of those 243 were taken at times when there was no increase of temperature -- 233, excuse me -- were taken at times when there was no increase in temperature across the condenser tubes;

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is that correct?

A That is correct.

Q And that again are the same numbers that are reflected in your table 9 on page 51 of the October 30 testimony, right?

A Yes.

Q Do you think it is fair to say, Dr. Lauer, that we might be -- we will probably expect more severe effects on eggs and larvae in young juvenile striped bass passing through the condenser tubes at a time when there is a temperature increase of 15 degrees across the condenser tubes than at a time when there is no increase across the condenser tubes?

A Depends on what the ambient temperature is relative to the fish's tolerance to discharge temperatures. One would have to consider this on the basis of the combination of laboratory temperature tolerance data, the ambient temperature that existed at that particular time, and the temperature tolerance of the organisms relative to those temperatures in order to decide whether that was probably the case.

Q Well, I realize that it is a complicated problem and would need a good deal of further analysis and as you said in the October 30 testimony, you haven't had time to complete that. I only received these documents yesterday so I am afraid we haven't had time to do a very thorough

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analysis of them either. Perhaps we will be able to put something in later testimony that would help in this kind of analysis.

I don't think there is too much point in pursuing any individual questions of that sort.

Let me just ask you about a few of these collections.

Would you take a look at Hudson River Fishermen's Exhibit No. 2 on sheet 3 at the two collections at intake station No. 1?

Take a look first at the one that began at 1510 and ended at 1540 on the 15 of June.

A Okay.

Q How many fish at that intake station were taken alive?

A 35.

Q How many were taken dead?

A 18.

Q And how many were taken stunned?

A 49.

Q How long was that net in the water?

A In that case assuming that this -- there hasn't been a mistake in transcribing this data, 30 minutes.

Q Would you take a look at the next test at intake 1, the one that began at 1545 and ended at 1547 the same day?

A Okay.

Q How many fish were taken alive at that time?

A 19.

Q How many were taken dead?

A Zero.

Q Would that indicate to you that many of the fish taken stunned were dead in the first experiment, the first line of data I referred to there, were killed or stunned by being held in the net for a considerable period of time while water was passing through the net?

A They may have been.

Q Did you include the data from that half hour net test in the figures that you put together to produce table 9 in your -- on page 51 of the testimony of October 30?

A I can't be certain about any particular piece of data, but I presume so, without double checking it.

Q Now the total number of fish that were collected in the course of the summer, weren't a very large number collected in that half hour tow on the 25 of June?

A Well, I don't know what you mean by very large.

Q Well, something on the order of, say, the total number of fish stunned at the intake -- collected from the intakes that were stunned, say, something on the order of 5 to 10 percent? Weren't 105 taken in the course of the summer at the intake stunned?

A Yes, there were.

Q And how many again were taken in that one half hour tow?

A. 49.

Q Well, I would have to increase my estimate. Isn't it something moreon the order of 50 percent that were taken in that one tow, the total number taken in the intake stunned?

A I don't know what percent it would be but it would be the ratio of 49 out of 105.

Q Right.

So that if that figure is used in the -- in any kind of estimate of the meaning of these numbers, some by areas might come in through the fact that that one tow the net was in the water for 30 minutes and large numbers of fish were taken, large numbers dead and stunned, and normally the net was only in the water for five minutes; is that correct?

A That would be possibly correct assuming that that time of net set is accurate and assuming that the condition of those organisms were affected by the time of the sampling. There is no way of knowing that for sure.

Q No. There is no way of knowing it for sure, but what do you think the probability is that if you had that net in the water for 30 minutes, the condition of the fish would change, fish that came in alive would be likely to be taken

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out as stunned or dead?

A I really don't know because there are other data points you could pick out where -- for even a five or even a one-minute tow you could either find all of them alive or all of them stunned or all of them dead. So there is no way of really determining for any particular data point just what the controlling factors may have been and what the results of those were.

End #19

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Q When you told us this morning that you used the short tows in order to check the condition and the longer tows to sometimes check the abundance, weren't you indicating that -- at least there was more probability with a longer tow that the condition of the fish would change?

A This appears to be true.

Q Thank you. Let me take a look at Exhibit III.

Dr. Lauer, I turn now to sheet five of Hudson River Fishermen's Exhibit No. II and show you the data collected at intake one on June 20 from 02:30 to -- excuse me, strike that.

From 0200 to 0215.

A June 20?

Q Yes. What number of fish do you find there taken alive, dead or stunned?

A 0200 to 0215, there were three alive, 16 dead, and two stunned.

Q And that was in the water for 15 minutes?

A Appears so.

Q Look at the next line, the test beginning at 0230 and ending at 0245. What do you find there for fish taken dead, alive or stunned?

A Five alive, 12 dead, zero stunned.

Q And again the net was in the water for 15 minutes?

A That's correct.

Q The next line where the data shows the test began at 0300 and went to 0330, what do you find there for fish dead, alive or stunned?

A Two alive, 12 dead, and zero stunned.

Q And --

A Wait a minute. I switched lines, I think. Two alive, 12 dead and two stunned.

Q And finally the next line where the chart shows that the test began at 0335 and went to 0400, what do you find there for the fish dead, alive and stunned?

A There were 10 alive, 11 dead, two stunned.

Q And that test lasted 25 minutes?

A It appears so.

Q Now in that group of tests on that day, where the net was in the water each time considerably longer than five minutes, do you find a higher ratio of dead and stunned fish to alive fish than is normally the case in the other tests that you ran in the course of the summer?

A I don't know. I'd have to analyze the complete sets of data to be able to make any statement on that.

Q All right. But again the same statement you made earlier would hold, that there is at least some probability that holding the fish in the net for a longer period would change the condition, namely change them from alive to either dead or stunned? This would hold for those tests, 25 and 30

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minutes each?

A This could be the case. However, it is also true in these instances that the volume, the total volume of water filtered, the amount of time, is considerably less than sampled out in the river in a net being towed because velocities are different. So we don't really face the kinds of clogging problems in here that you would have for the same length of period of tow out in the river. I really basically think that considering the variability that exists amongst the data at any given data point, one can't just look at a particular data point or a few data points and draw any significance from these. You have to look at a considerable number of samples and under a given set of conditions to begin to draw conclusions as to abundance, condition and factors that might affect condition having to do with the sampling.

Q Wouldn't it be important to have the various tests that you did look at in that series all uniform? In other words, have the net in the water for five minutes in each one?

A It may turn out to be desirable to know. If it would be desirable, one would have to have the kind of data I just described. However, one of the reasons why the timing is different is that we are carrying on a monumental amount of sampling in this type of situation and we are frequently

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trying to get several pieces of information out of the same tow, and that's one of the reasons for the length of the tow varying somewhat from time to time.

Q You said a longer tow would be useful for abundance. Did you use those tows of 25 minutes to a half an hour in constructing the chart on table -- in table 9, page 51, which describes the condition of the fish?

A Without checking every particular point, I would presume so.

Q Dr. Lauer, do you know --

CHAIRMAN JENSCH: Excuse me for interrupting. Would it be too much of a job for you to check that tonight, whether you did or didn't?

WITNESS LAUER: I am not sure if we can do it tonight or not. We'd have to have the foundation data available to us here, and I'll have to look into that to see. I am not sure I have that with me. I'll find out and let you know as soon as I have a chance to check it.

CHAIRMAN JENSCH: Thank you.

I think it is important sometimes to establish whether you used the data or not. If you disregarded it, it might lead to a different result.

WITNESS LAUER: The reason I am giving a qualified answer is I am under oath, and I couldn't swear to the completeness of every particular point.

CHAIRMAN JENSCH: That's perfectly all right.

WITNESS LAUER: I think they probably were.

CHAIRMAN JENSCH: We just want to be sure one way or the other, though.

BY MR. MACBETH:

Q Dr. Lauer, do you know whether any tows of a half an hour were made at any time in the discharge stations in the course of the summer?

A (Dr. Lauer) I would expect there probably were some, but I would have to examine all the data individually to see if there were and identify what it was.

Q I'd appreciate it if you would look and be able to tell us how many tows of, say, 15 minutes to a half hour were made at the intakes and how many tows of 15 minutes to a half hour were made in the discharge, at the discharge stations in the course of the summer. Again obviously that would be more easily done overnight.

MR. MACBETH: Mr. Chairman, I think that virtually concludes my cross-examination. If this would be a convenient time to take a break, I think I could review my notes and see whether there are another handful of questions I should put to Dr. Lauer. We have been going an hour and 15 minutes. Perhaps this would be a good time to take a brief recess.

CHAIRMAN JENSCH: When you do that, would you also indicate your view to the Staff? I have had a request from

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the Staff that they would like intermission between your conclusion of the examination and their starting so that they can pick up their notes. If you will do that.

How long would you like to have for recess?

MR. MACBETH: Could we have 15 minutes?

CHAIRMAN JENSCH: All right. At this time let's recess and reconvene in this room at 3:45.

(Recess.)

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CHAIRMAN JENSCH: Please come to order.

Have you concluded your examination?

MR. MACBETH: No, Mr. Chairman, I have a few questions I would like to take up.

CHAIRMAN JENSCH: Proceed.

BY MR. MACBETH:

Q Dr. Lauer, just so we are clear on the record about one or two items, I am right in assuming, am I not, that there is no breakdown available between striped bass and white perch for those fish taken alive, dead or stunned which are recorded in exhibit 2?

A Yes, that's correct.

Q And is it also true that exhibit 2 represents all or virtually all of the data collected at the intakes and discharges between May and the first of August of 1972?

A I think it represents most of the data as you stated it. At least that we use that is relevant to fish eggs and larvae. There may have been other samples taken that were used.

Well, there were other samples taken used for other purposes like the microzoaplankton, phytoplankton.

Q Restricting ourselves to the white perch and striped bass, is this virtually or --

A I think it is just about all.

Q I just wanted to make sure there isn't some other

data somewhere that I should pursue like a hound hot on the trace of something or other. It doesn't seem like the right metaphor for fish.

(Laughter.)

Q Could you tell me the speed of the boat during the 1971 tows?

A No, I couldn't tell you what it was. It is several knots but it varies. The speed of the boat -- the actual speed of the boat varies depending upon wind conditions and the strength of the tide flows among other things.

In order to try to compensate for that as much as possible, what we do is to try angulator use or rigging on the back of the boat to try to angulate so as to try to get the cable at the same angle relative to the vertical axis all the time so that we are about as confident as we can be that the speed of the net movement relative to the water going into it is approximately the same, which is probably more important than the absolute speed of the boat.

Q It is. Could you give me an approximate number of what that relative velocity is?

A Relative velocity into the nets?

Q Yes, of the water into the nets.

A No, I couldn't.

Q Is that something you could find out by checking your data or something you simply don't know?

A I think that would be something that would have to be determined by direct measurements, putting flow meters down with the nets under a series of different tidal conditions to see precisely what those velocities are. Going into the net as well as past the net.

Q That was not done in 1971?

A We have not done that.

Q Thank you.

I show you now a document consisting of four pages and the first page has a table headed, White Perch, Mean Abundance, Number per Thousand Cubic Meters at Seven Sample Stations.

Is that document a compilation of the number of white perch and striped bass eggs, yolk sac larvae, and larvae collected at the surface, mid-depth, and bottom at the seven sampling stations which NYU maintained in the course of the summer of 1971?

A Yes, it is.

MR. MACBETH: Mr. Chairman, I would like to offer this document in evidence as Hudson River Fishermen's Association exhibit number Roman numeral four.

CHAIRMAN JENSCH: Would you show that to counsel, please.

(The document referred to was marked Hudson River Fishermen's

Association Exhibit No. IV, for identification.)

CHAIRMAN JENSCH: Is there any objection?

MR. TROSTEN: Subject to the same qualifications, Mr. Chairman.

CHAIRMAN JENSCH: The document identified by Hudson River Fishermen's Association counsel may be marked for identification as Hudson River Fishermen's Association exhibit number IV, having been previously offered no objection from the regulatory staff?

MR. KARMAN: No objection, Mr. Chairman.

CHAIRMAN JENSCH: Very well. That exhibit, number IV, is received in evidence subject to the motion that may be made.

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(The document heretofore marked Hudson River Fishermen's Association Exhibit No. IV, for identification, was received in evidence.)

BY MR. MACBETH:

Q This exhibit number IV has night sampling data for white perch on the first page and night sampling data for striped bass on the third page. In both cases, the last samples taken are on the 21st of July. Is that the last date on which samples were taken at night?

A No, it is not. The sample dates entered on these tables, and that's true of each of the tables, are sample dates wherein organisms of this description, yolk sac larvae or larvae or eggs as the case might be, were found at some one of the stations on that particular date. In other words, we had samples that were taken prior to this and subsequent to this, but they were all zeros.

Q And they would be of the same regular intervals I believe that these are, every other week for the night samples and twice a week for the day samples?

A That was true for 1971, up until about the month of November. Then it went on a reduced sampling intensity.

Q All right. Thank you.

So that after July 21st of 1971, no white perch yolk sac larvae or larvae were taken in the tows at night, is that correct?

A That would be correct. If there had been some taken, there would still be another date entered on the table.

Q And the same is true for striped bass eggs, yolk sac larvae, and larvae at night, none taken after July 21st?

A That's correct. The next sampling date would have been approximately two weeks later which would have put it into the 4th or 5th of August, thereabouts, and there would have been none taken at that time or else it would have been

on there.

Q Turning now to the day samples for white perch and striped bass which appear on pages 2 and 4 of exhibit IV, here the last dates given are July 30th in both cases and is it true that tows continued after that, the numbers would be zero?

A That's correct.

Q I show you page 2 which covers white perch taken during the day and draw your attention to the tows taken on the 27th of July and the 30th of July. Is it true that the only column here among the six which reflects surface, yolk sac surface larvae, mid-depth, mid-depth larvae, bottom yolk sac and bottom larvae which has any number in it for those two weeks is figure .1 for bottom larvae on July 30th?

A That's correct.

Q I now show you the same chart for striped bass, page 4 of the exhibit.

CHAIRMAN JENSCH: Would you show it to him as you read the numbers?

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BY MR. MACBETH:

Q And draw your attention again to those two weeks. Is it true no striped bass, eggs, yolk-sac larvae, or larvae were taken at any of the sampling stations on the last two dates included in that chart which are July 27 and July 30th?

A (Dr. Lauer) That's correct.

Q Dr. Lauer, you know of any reason why there would have been a greater abundance of white perch and striped bass eggs and larvae in the vicinity of Indian Point in the last weeks of July and the 1st day of August of 1972 than there were in the last days of July and the 1st day of August of 1971?

A I don't know of any particularly, offhand. We may have a better feel for why that appeared to be the case after having done a lot more data analysis of abundance relative to temperature and other factors. As a general observation, though, it did appear that we had a later spring in 1972 and cooler water temperatures persisted for a longer period of time in the spring than in 1971. This may or may not have been a factor involved in seeing abundance for a later period of time in 1972 compared to 1971.

Q I show you pages 9 and 10 and 11 of HRFA's Exhibit No. II and draw your attention to the number of morone eggs and larvae taken in the intakes and discharges of Indian Point 1 for the dates of July 25 and following, and is it true there

were a number of striped bass and white perch eggs and larvae taken at the -- in the intake and discharge of the Indian Point 1 from July 25 through August 1st of 1972?

A Yes, there were, although I would like to qualify what you just said a little bit in that there were representatives of the morone group occurring then. I couldn't say at this point as to whether or not they represented both white perch and striped bass or either white perch and striped bass. It was one or the other or both.

Q Yes. And these would not be the only eggs or yolk-sac larvae or larvae, is that correct?

A It would be extremely doubtful if they would be eggs. That season had long since passed. They were, according to my characterization of them, they would be larvae.

Q Is it -- is at least one possible explanation of the fact that no striped bass or white perch were taken in the tows in the last weeks of July of 1971, but were found in the intake and discharge of Indian Point 1 in 1972, the -- let me rephrase that.

Can that situation be explained perhaps by the theory that the tows are not as efficient as they might be, and in fact the organisms are in the area and susceptible to the plant for a longer period of time than is shown by the towing data?

MR. TROSTEN: Mr. Chairman, I am afraid I will

have to object to that question simply on the grounds of lack of foundation.

MR. MACBETH: I thought the foundation was rather secure. We demonstrated that there were no white perch or striped bass, with one exception, taken in the last weeks of July of 1971, that white perch and striped bass were taken in the intake and discharge of Indian Point 1 in the summer of 1972 at the intake and discharge stations. There is some discrepancy between those two sets of data which may be explained by the fact that there is a later spring this year. I am asking Dr. Lauer whether it is not also possible, that it could be explained by the fact that the tows are not totally efficient and eggs, yolk-sac larvae and larvae may be susceptible to the plant intakes for a longer period than the Chambo tow data?

MR. TROSTEN: I continue to object to that for the reason I have stated, Mr. Chairman, and also for the fact it is additionally vague. When Mr. Macbeth says the tows are not totally efficient, I don't know what that means. It strikes me as being a vague question, lacking in foundation, which is inordinately difficult for a witness to be asked to respond to, Mr. Chairman.

MR. MACBETH: I disagree with the Applicant's counsel.

CHAIRMAN JENSCH: Didn't we define efficiency this

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morning based upon gear and what is supposed to be the factors that go into efficiency? I thought it had been. I think this raises a question of possibility for an explanation. Accept it or not. He is an expert in the field. We can't accept your thought that he is unable to handle the question of this kind.

Objection overruled.

WITNESS LAUER: Well, obviously if we don't get the organisms, we have no data foundation to support that supposition. It is within the theoretical realm of possibilities that there could be some larvae of that size some place in the river that our sampling would not have detected. However, if they were present in any amounts at all, considering the kinds of sampling program we are carrying on and the number of samples we are taking, we ought to be able to -- we would have determined certainly if there were any abundance of these things around. There could be very sporadic or sparse numbers of these organisms some place in the system that either may or may not be in front of the intake screens and may or may not be coming through the plant. If we don't get them in the nets, we can't say whether they are coming through or not. We don't have any data foundation for support of that supposition.

BY MR. MACBETH:

Q And it is true as you said earlier that you do

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do know the efficiency of your towing gear?

A (Dr. Lauer) No, we don't.

MR. MACBETH: That concludes my cross-examination, Mr. Chairman.

CHAIRMAN JENSCH: Does the Staff desire a recess before proceeding?

MR. KARMAN: Mine shouldn't take too long, Mr. Chairman. I think we might as well start now.

MR. TROSTEN: Would you like to come over here, Mr. Karman?

MR. KARMAN: I could swing over here so you could see me.

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CHAIRMAN JENSCH: Well, come on over on this side of the table of Hudson River. I think the reporter is going to have a problem.

MR. KARMAN: He will be able to hear me. I will make my voice as loud as I can, Mr. Chairman. The amplification is not too good.

MR. BRIGGS: Possibly if you just used the microphone but left it on the table there and didn't speak into it.

BY MR.KARMAN:

Q Dr. Lauer, on page 10 of your October 30th testimony, this is the continuation of Table 1. There seems to be a word omitted in Footnote No. 1 wherein it says, chlorine residual data above this line were determined by the --

A (Dr. Lauer) That is correct. The word was not in there. It should be the orthotolidine method.

Q Of course.

(Laughter.)

On page 12, Dr. Lauer, you discuss inhibition of bacteria. My question is, if bacteria were inhibited in their metabolism, would you not expect some inhibition in the zooplankton and phytoplankton or anything else living along with the bacteria?

A Is this one page 12?

Q On page 12 you discuss inhibition. My question is

pursuant to that, if you discuss the inhibition of bacteria, my question is do you also expect some inhibition of zooplankton and phytoplankton ?

A Well, page 12 is apropos phytoplankton, not bacteria. That is why I was raising the question. Page 12 of my testimony has to do with inhibition of phytoplankton as measured by the C-14 uptake method and not bacteria. So that is why I was having a problem with the question.

Q How about zooplankton?

A A generalization of that kind really can't be made. It really can't be made across a whole category of organisms like this except as measured by an assay procedure of this type. In other words, we are using a mixed population of phytoplankton to measure assay conditions of them. We do see some inhibition taking place. That could represent inhibition of all species within the population or some particular species within the population. It can't be applied carte blanche to zooplankton as a community necessarily. It is possible that there could be some zooplankton species that the metabolism of which might be inhibited at these same temperatures.

Based upon our temperature tolerance information that we have obtained, it is more helpful than specific probably. For example, we apropos the zooplankton and the neomysis in particular. We have determined their maximum

temperature tolerance relative to survival is on the order of 90 degrees and certainly in that specific instance, those organisms would be said to have been inhibited sublethally at temperatures below 90 which would be temperatures below those which caused the phytoplankton inhibition. I can't answer this for the total zooplankton community but there are specific components ~~componects~~ of the zooplankton community for which it might be accurate to say that inhibition, physiological or metabolic inhibition, will have occurred by the time of the temperatures -- or by exposure to the temperatures that are discussed on this page.

Q Could this increase, could this inhibition be indicative of some later mortality which was not measured?

A The inhibition of phytoplankton?

Q Yes.

A It could be. It is extremely difficult to determine whether phytoplnakton are dead or alive by any other than these kinds of metabolic activity assays, so it is uncertain as to whether inhibition in itself represents a lethal effect or not. It may or may not and that is also true of the question you ask. It may or may not indicate some subsequent lethal effects of the organisms that were involved in demonstrating this physiological inhibition.

Q On page 20, Dr. Lauer, you indicated and mentioned

that dead eggs appeared opaque. What other observations help determine whether or not the species are living or dead?

A Could you point that out, where that is on page 20?

Q I believe that it was mentioned in your testimony this morning about the observation of the dead eggs appearing opaque.

A Now, are you asking about other criteria?

Q Yes, what other criteria, what other observations can be made to determine whether or not the eggs are alive or dead?

A That is really the only clear-cut observation that I know of short of rearing them on through to the hatching stage to then determine their relative hatching success which may represent a sublethal effect rather than a lethal effect. It probably has if they haven't turned opaque. That is the only way really to tell unless they are macerated or disintegrated. That is the only way of telling the condition of the eggs as far as a lethal condition is concerned. And any kind of sublethal stress effect has to be judged on the condition of the larva that hatch out of the eggs.

That was the kind of criteria that we used in any case. It may be theoretically possible to utilize other kinds of a physiological technique like measuring respiration rates or other such parameters to determine some kind of stress, but these are the parameters that we used. It

appeared to be the proof of the pudding whether or not the eggs could hatch successfully into the larval stage.

Q With respect to the items on page 40, leading on to page 41 of your testimony, Dr. Lauer, you have given us some data here and my question is in the data that you have submitted, can it be used to measure biological compensatory limits for any of the species mentioned?

A No, it cannot.

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MR. TROSTEN: May I ask Mr. Karman to define what he means by biological compensatory measures? I would like to have the record clear on this point in light of the question and the answer.

MR. KARMAN: You mind if I consult?

CHAIRMAN JENSCH: Maybe to save time, how did you understand it, Mr. Witness? Maybe that would be a start. What did you understand biological compensatory measures to mean?

WITNESS LAUER: Biological compensation, I would include a response or a reaction or ability for a population of organisms in a natural ecosystem to withstand various levels of predation from whatever cause without experiencing a decrease in sustainable yield over a long period of time, that is organisms, in many instances, to exhibit the capability of carrying on live processes and successful reproduction in the face of various levels of predation and this is generally termed compensatory capability or compensatory research.

This compensatory research can be exercised or exist through a considerable number of different mechanisms; and my response to the question was that the particular studies that we have conducted so far are not of the type that would define either the mechanisms or the particular level of predation without considering the mechanisms involved that would determine the compensatory research of any of the particular species

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that we deal with in our studies.

A general kind of assessment of the compensatory research of an organism can be drawn in part based upon the direction, the lateral and vertical and horizontal direction of these organisms, the extent of the range of their species, of this species within the habitat, and some consideration of the understanding of the fact that they do naturally experience predation from a number of different directions.

As I indicated on page 41, since Neomysis is the only organism so far that we have found to experience effects going through the plant in the way of these invertebrate organisms, I indicated on page 41 that in view of this, future studies would need to include a monitoring of Neomysis population dynamics in the river, determination of rates of reproduction generation times, et cetera.

These are kinds of parameters that would begin to address themselves to determining compensatory research. Another direct way of determining compensatory research is to look at the response of a population of organisms over a period of time which is exposed to a level of predation in the system, whatever that source of predation may be; and as I indicated previously, these organisms are in the food web of the system and they are subject to natural predatory levels so it is assured that they do have a compensatory research. The real question is how much.

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CHAIRMAN JENSCH: Did you want to add anything to that statement or definition?

MR. KARMAN: No. I would just ask this question.

BY MR. KARMAN:

Q Do you agree that biological compensability of a population or a biomass is the ability to adjust its number in response to changing mortality? Would this basically be in agreement with your concept?

A (Dr. Lauer) I think my concept of it would include that. I think it can be a broader concept than that, too, to take into account sublethal effects on a portion of the total population which may not be a lethal effect, but nevertheless may effect a ~~fine night~~ finite portion of the total population so it doesn't necessarily have to be a capability to respond to lethal effects.

CHAIRMAN JENSCH: Did you have any further inquiry about that definition, Applicant's counsel?

MR. TROSTEN: No.

CHAIRMAN JENSCH: Very well.

BY MR. KARMAN:

Q During your testimony, Dr. Lauer, the collections that you took, did they take into account the consideration salinity in your calculations?

A (Dr. Lauer) I don't really know what testimony you are referring to.

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Q In any of the samples you were discussing with Mr. Macbeth this morning.

A We recorded the salinity along with temperature and dissolved oxygen, along with the collection of each sample.

Q You believe the salinity would have some effect?

A We know that it does have some effect in determining, for example, whether or not, a particular species occur in the vicinity of Indian Point or not in a given time.

Q Do certain organisms array themselves in any way along this salinity gradient?

A This is known to happen.

Q Do striped bass, I don't think striped bass, for example, move into higher salinity in the shoals?

A I don't think that I know of or that we have from our own information any information to say yes or no to that question.

Q You don't know whether they move in that order or into the fresh water?

A No, I don't think our information would be enlightening in that regard.

Q Dr. Lauer, would you turn to figure 7 on page 16 of your October 30th testimony?

Do you have that in front of you, Dr. Lauer?

A I do.

Q Would you say that on May 24th, would you agree with

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me that on May 24th, the species composition is about 88 percent diatoms and 12 percent green algae?

A Yes, that is approximately correct.

Q Does this composition change change significantly between that date and June 21?

A Yes, it does.

Q Would it be correct to say that near or about May 24th there appeared some combination of environmental factors that became especially suitable for green algae but less suitable for diatoms thus causing a relative expansion of the green algae?

A I would say taken in a general sense, yes, that would be indicated.

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Q Could you let me know one or about what data your data suggest that the green algae are numerically dominant in the population?

MR. TROSTEN: May I ask for a clarification, Mr. Karman?

MR. KARMAN: From this chart.

MR. TROSTEN: What do you mean by dominant?

MR. KARMAN: Moving ahead. At one point we said the diatoms were dominant. I led up to that by indicating that the -- having the witness testify that the composition changed significantly between that date. I want to know at what date would Dr. Lauer from his own data indicate that the green algae became the majority.

MR. TROSTEN: Became the majority?

Okay. Thank you.

WITNESS LAUER: I couldn't pick out a particular date.

BY MR. KARMAN:

Q Approximately.

A (Dr. Lauer) I would say between the dates of June 7 and June 21.

Q Would such -- would the date of -- assuming that numerical dominance--majority, or the date of rapid increase in population growth be more indicative of environmental changes favoring the green algae?

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A That is an awfully general question. These kinds of shifts in dominance of phytoplankton, and I guess in a general sense they would be considered environmental -- these kinds of shifts in dominance of phytoplanton communities can occur as a result of a considerable number of different kinds of processes. One can be -- one such process can be factors such as nutrients, physiochemical conditions which would favor the greater rate of growth of the green algae over the diatoms.

This figure is a figure of relative abundance. It doesn't say anything about what the absolute abundances were. So this figure does not necessarily say that the diatom population decreased. It simply says that in terms of the numbers of organisms that existed in those samples, the green algae constituted a higher percent than the diatoms did. You can't necessarily construe changes in absolute abundances from information of this kind

Another kind of process that can cause this same kind of shift which I guess in general would be descirbed as an environmental condition would be by selective grazing of one componentn of the phytoplankton population rather than another so that if the herbivores in the system were grazing on the diatoms preferentially to the green algae, this could cause this kind of a change in the pattern of percent composition. In that case, it may be due

then to an absolute reduction in numbers of diatoms. But in general all I am saying is this kind of information cannot be taken as an index of absolute abundance of either any of the components in the system. It is a relative abundance kind of thing and it can be caused by considerable number of different kinds of mechanisms in the system.

Q One of the factors could be temperature?

A Temperature is known to have some influence on the species composition of phytoplankton communities including their species composition, so it is a possibility.

However, I think in a case like this we ought to be dealing with probabilities rather than possibilities and in years past, going back through the 1966 period, we have been doing studies upstream and downstream at this particular site and the primary sources of pollution into the Hudson River estuary occur up at the Troy - Albany area, upstream or northward of this Indian Point plant, and down in the New York City area, south of the plant.

What we have observed is, in past years, we weren't doing studies in this particular year at locations upstream or downstream, but what we have observed in past years is that these kinds of shifts in relative abundance within the phytoplankton community generally occur earlier upstream in the area of Hyde Park, Newburgh, and above, and progress downstream toward the Indian Point area.

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We have seen other instances where apparently, under extraordinarily high tide conditions, or drought conditions, where there is a high influx of the more saline water from the New York City area, that there are also influxes of plankton communities into the Indian Point area representing different kinds of species, relative species composition, than occur further on upstream. So it is an extremely complex pattern appearing to react probably more to the nutrient and organic input loads than to temperatures since they occur far upstream of any possible influence of the Indian Point plant.

It is also of interest to note that these kinds of changes in population relative abundance take place at very significantly different times of the year when you look at different years, and this again appears to be considerably related to fresh water flows through the system indicating that its conditions upstream are having some effect.

Our further point is that the shift that you described during the time period that we have already described here are taking place at temperatures considerably below what the literature indicates to have this kind of an effect for -- under controlled experiements where everything else is held pretty much constant except temperature.

Generally these shifts that have been related to temperature alone tend to take place at temperatures in the

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range between 86 and 89 degrees Fahrenheit and this is considerably higher than the ambient temperature conditions that exist in the river during the periods of middle June that we are discussing.

It is an extremely complex thing to try to determine the cause and effect relationship of something like this.

Q Chemical nutrients are part of the environment, are they not?

A Yes. The reason I was talking about -- the reason I was qualifying or hedging around environment was that -- for a moment was that obviously the presence of herbivores has an effect too and I guess in a sense you would describe that as part of the phytoplanktons' environment although environment I think generally is more used to describe physiochemical conditions that exist rather than the presence of other biological components in the system.

Q Dr. Lauer, could we compare the figure 5 statistics on page 13, phytoplankton abundance in the Hudson River in the vicinity of Indian Point, 1971, with the percent composition on figure 7? It appears that the figures stay -- remain somewhat constant in figure 5.

Is there any discrepancy between the various -- these two charts that I have indicated to you?

A No, I don't see any discrepancy between them. They

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are representations of two different kinds of data, one having to do with numbers of cells per liter, and the other having to do with the relative abundance of organisms within that -- within those numbers per liter. So they are really two different kinds of pieces of information. There is no discrepancy between them.

DR. GEYER: Is abundance measured on the basis of actual numbers or on volume?

WITNESS LAUER: Cell counts.

DR. GEYER: Cell counts?

WITNESS LAUER: Yes.

BY MR. KARMAN:

Q Is it possible than, Dr. Lauer, to take the proportion from figure 7 and apply it to the dat in figure 5?

A (Dr. Lauer) Yes. I think that is -- in general it would be, yes. I assume your meaning applying it to figure 5 by way of saying given a given number indicated on figure 5, we could then go back to figure 7 and look at the percent compositions for that particular date?

Q Yes.

A Yes. As a general thing, you could do that.

MR. KARMAN: May I have just a moment, Mr. Chairman.

CHAIRMAN JENSCH: Yes.

BY MR. KARMAN:

Q Dr. Lauer, how long do you indicate the striped bass would be in the vulnerable condition in the discharge or intake canal?

MR. TROSTEN: Would the --

BY MR. KARMAN:

Q The various phases of it?

MR. TROSTEN: Would the reporter read that question?

(The reporter read the record as requested.)

MR. TROSTEN: Mr. Karman, would you define what you mean by the term vulnerable?

MR. KARMAN: Susceptible to the effect by the intake canal.

MR. TROSTEN: Physically? Excuse me, I am just trying to understand what you are saying. Do you mean susceptible in the sense that they are small enough to go through the system?

MR. KARMAN: Susceptible to entrainment.

MR. TROSTEN: In the sense they are small enough to go through the system?

MR. KARMAN: Yes.

CHAIRMAN JENSCH: Would you like to have the question

reread?

WITNESS LAUER: Yes. It seems we have pieces of two questions at this point.

(The reporter read the record as requested.)

MR. TROSTEN: At this point, this witness can decide this for himself. It is questionable to me whether Dr. Lauer is the witness to respond to this as opposed to Dr. Lawler or perhaps another witness. I will let him decide that for himself.

CHAIRMAN JENSCH: I think that should be kind of a condition precedent to any answer that is beyond the scope of his work. If it is, he should so indicate it.

WITNESS LAUER: The question still seems to be a bit jumbled in that in further exploring the question, it was then indicated we were concerned about the size of the organisms relative to whether they can pass through the screens. Are we talking about size of fish relative to whether they can come through the plant in one instance? The questionas reread of -- ~~appeard~~ appeared to have to do possibly with passage time through the cooling water system. I think we still have a sort of jumbled question here involving two different things.

BY MR. KARMAN:

Q Maybe I can clarify it. On Hudson River Fisherman'e exhibit Roman numeral three -- do you have a copy of that before you?

A No, I don't.

(Document handed to witness by Mr. Macbeth.)

BY MR. KARMAN:

Q It would appear to me that certain samples of striped bass and white perch were collected from the intake and discharge at Indian Point around August 1st, is that not so?

A That's correct, in 1972.

Q Hudson River Fishermen's Exhibit number IV, those fish collected August 1st, do you have any idea how old they were?

A Not specifically except that they would have been spawned from the egg production in the spring and so we could come up with some kind of a probable mean estimate from that by looking at the zone of egg occurrence.

Q Do you have exhibit IV in front of you?

A I do.

Q On the page which states, "Striped bass, mean abundance, seven sampling stations," day samples, is there any significance between the figure that you just read to me from the intervenor's exhibit three and the figure of striped

bass at the stations in June 3rd of 2.3 eggs and 18.9 eggs at the surface, 18.9 at mid-depth and 17.4 at the bottom.

Is there any relationship between those figures and the figures that you just indicated to me with respect to the August 1st date?

MR. TROSTEN: Excuse me, Mr. Chairman. I would have to ask Mr. Karman if he would clarify his questions. When you say relationship, what kind of a relationship?

BY MR. KARMAN:

Q Is there any relationship between the spawning period of those eggs, which were samples at June 3rd and those that were taken on August 1st; is there a possibility that the striped bass would remain susceptible for the period from -- susceptible as I indicated before with respect to the intake canal from this June 3rd to the August 1st period?

MR. TROSTEN: Mr. Chairman, excuse me. I simply think that is a very vague question.

MR. KARMAN: Maybe the witness understands it, Mr. Trosten. He's the one answering the question, not you.

MR. TROSTEN: That's true but I feel the question is very vague, Mr. Karman.

MR. KARMAN: To you it might be; it would be vague to me, too, Mr. Trosten.

(Laughter.)

MR. TROSTEN: The problem is sometimes a witness

will try to -- well, okay.

CHAIRMAN JENSCH: I don't think that you should throw up your hands on this thing. I think the inquiry on what the relationship is is very pertinent and I am having difficulty with the question myself.

Are you saying that those which were collected are likely to be damaged through -- through going through the intake?

MR. KARMAN: No. What I am trying to say is, is there a possibility that the eggs that were collected would indicate that others would remain and be susceptible from that period of June 3rd until August 1st? Those that are collected, obviously they are finished.

CHAIRMAN JENSCH: Susceptible to being damaged?

MR. KARMAN: Susceptible to the entrainment, yes.

CHAIRMAN JENSCH: Does that assist?

MR. TROSTEN: I am afraid I cannot understand the question. I don't understand the question in relation to the data presented, Mr. Chairman. Maybe if Mr. Karman would rephrase the question, I would understand it.

MR. KARMAN: I am going to find out now whether I was vague.

BY MR. KARMAN:

Q I have a simple question. How old was the striped bass collected on August 1st?

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Q Do you happen to know whether or not this data will help to determine stage lengths?

A I think there might be some value there, but possibly pretty limited in that the -- both the hatching time and the growth of these larvae up to the point of feeding in this circumstance, in the first place took place -- well, it can be used to determine it for these specimens, from this stock, but I think it has relatively limited value for doing so when applied to the Hudson River striped bass, eggs and larvae because these ones representative of figure 17, as I indicated earlier, were cultured at higher ambient temperatures for one thing than occur in the Hudson River when the eggs are present there for the most part and that temperature was held constant throughout the rest of their exposure time which is not the kind of temperature, ambient temperature experience that similar larvae would have experienced in the Hudson River.

And simply, once the larvae reached feeding stage, the older larvae, they were fed in the laboratory and their feeding rate, availability of food to the, could ~~conservably~~ concurrably be quite different than what would be available to them in the Hudson River. This was a controlled experiment to -- that was necessary to get out these kinds of temperature tolerance information, and pressure tolerance information.

I think they would only have the most general, if any, application to defining specifically what this -- the

(Laughter.)

A I guess my answer is simple, too. I don't know for sure. All I can say is that they were larval stage fishes that based on our general experience probably did not exceed the length of a half an inch to three-quarters of an inch and they would have resulted from the egg crop produced in the spring of 1972. So, I can't tell you which larvae, for the individual larvae, whether they would have been ones emanating from the first egg spawn or the last egg spawn. There just isn't any way to know that with certainty.

An approximation of age could be gotten from looking at the relationships of their size back to the time in which eggs were spawned in the river.

Q Would you know when the last eggs were spawned?

A I don't know that -- we don't have that data worked up for 1972 yet. That's another problem with this. You are referring to 1972 data in exhibit three and 1971 data in exhibit four. I don't think there is more than a week's difference in the peak abundance of eggs in 1972 from what I know of the data that's coming up.

So, if that would be helpful, I think the peak egg abundance was generally within a week -- in '72 was within a week of when it occurred in 1971. I think it was approximately a week later, but we don't have this 1972 data at all digested to this stage at this point.

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BY MR. KARMAN:

Q Dr. Lauer, I ask you now to turn to figure 17 on page 45 of your testimony.

A (Dr. Lauer) Okay.

Q Did you have any control data which was not plotted in this figure?

A Any control data? There are no control data plotted in the figure at all. These are the experimental results for maximum tolerable temperatures derived by comparing the experimental results to control samples of these same organisms. We don't show any of the controls.

Q I see. But did you have it? Did you have it in your possession to enable you to plot this curve?

A Yes, indeed.

Q I see. Is there any chance of our seeing that data?

MR. TROSTEN: Certainly. I mean, the data are avaiable.

WITNESS LAUER: They are not here. The data, the data on the striped bass and white perch and tomcod, temperature tolerance, are being written up. They will form the basis for a PHD thesis and they are being written up by the candidate for that degree at the moment.

The bulk of that data is in Pittsburg, Pennsylvania at the moment. I couldn't make it available to you right now.

BY MR. KARMAN:

If it would be helpful, I would just judge that the approximate age of those fish might have been on the order of two months from the mean period of egg production, roughly.

CHAIRMAN JENSCH: Coming back to your question, staff counsel, is it your inquiry then that -- is there a possibility of those fishes in that larval stage, that they would be susceptible to entrainment on August 1, 1972; is that your question?

MR. KARMAN: That's correct.

CHAIRMAN JENSCH: Can you answer that?

WITNESS LAUER: Well, we collected them in the intake and discharge canal. So that is implicit indication that they are susceptible to entrainment.

CHAIRMAN JENSCH: Does that answer your question?

MR. KARMAN: Yes, Mr. Chairman.

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length that a given stage or age would be for the Hudson River stock.

Hopefully we will be getting some information of that kind if we are successful in getting Hudson River striped bass, males and females together, to give us some fertilized eggs in this coming season.

Q Are there distinct races of striped bass?

A Please?

Q Are there distinct races of striped bass?

A There appear to be, yes. This might be another complicating factor.

Q Would the striped bass from the Puppa River in South Carolina be of a different race than those from the Hudson?

A They could be and that is the reason for indicating that while we went there to get the materials, that is the eggs and larvae necessary and available to us at known periods of development to do these studies, that we then had to come back and try to do some number of experiments in the Hudson River using Hudson River stock to see how those relate; and we had -- we were able to do a minimal amount of this.

We had a minimal amount of success doing this and those data from the Hudson River stock are indicated on figure 17 by way of the open circles. We only had stock available from one spawn from the Hudson River for those particular

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developmental stages that are represented in figure 17.

In those cases they do appear to agree reasonably close, closely considering the fact that they are from a different stock and from a different pair of parents and were collected at different ambient temperature conditions than the laboratory stock down there.

I expect that we may get additional data of that kind for further verification of application of the South Carolina data to the Hudson River stock.

Q Do you happen to know, Dr. Lauer, whether the thermal tolerances are the same for the South Carolina striped bass as for the Hudson River striped bass?

A We can't be sure about that except to the extent that these circles which I indicated from the Hudson River stock appear to agree reasonably well with the values obtained for that same developmental stage for the South Carolina stock.

MR. KARMAN: Those are all the questions I have, Mr. Chairman.

CHAIRMAN JENSCH: Any redirect?

MR. TROSTEN: Not at this time, Mr. Chairman.

MR. MACBETH: Mr. Chairman, I thought of one more question if I could.

CHAIRMAN JENSCH: All right. Fine.

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BY MR. MACBETH:

Q Dr. Lauer, how long were the specimens taken from the intake and discharge sampling during the course of the past summer held to determine any abnormality of behavior or other effects after passage through the plant?

A Did you say how would the length --

Q How long did you hold them for observation after you removed them from the intake or discharge sampling station?

A Oh. For variable periods. Some of them we held -- we didn't hold at all for any delayed period of time. We held some for planned periods of 24 hours.

There were a considerable number of the live ones that we took out of the samples and placed in aquaria for subsequent use, for pressure studies and other kinds of laboratory experiments.

We held those there for, I think, probably the maximum time was on the order of two months, by which time they had grown to a considerably larger size because we were feeding them along the way. They were -- I guess they had increased probably 100 percent in length over that period of time.

So it was variable.

Q Do you have a compilation of that data indicating which station they were taken from, how long they were held there, eventual fate?

A I don't think -- in fact, I am quite sure that we are not at the stage right now where we have that compiled together into a compilation which would summarize those kinds of delayed observations. I am speaking from my recollection at this point. We are not at that stage of the data processing yet where we have accomplished the developing of a table or chart of that kind.

Q Again would you check that, and if compilations of that sort are produced before the end of this proceeding, would you produce them for me?

A Yes.

BY MR. KARMAN:

Q Dr. Lauer, I wasn't quite sure what your response was when I asked about the data base for figure 17, whether you would be able to provide that. You mentioned something about a PhD thesis. Is there any data you could provide to us?

A Well, all of this data can ultimately be provided to you. You are talking about the control data now?

Q Yes.

A And all of this data can be provided. I was just pointing out that I don't have it here and it is in Pittsburgh.

Q I understand.

A It does exist.

Q Mr. Trosten indicated his willingness to see to it

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that we do get it.

MR. TROSTEN: Or have access to it.

MR. KARMAN: Fine. Thank you.

MR. BRIGGS: There are four questions that I would like to ask concerning the numbers in table 19 of, I believe Dr. Lawler's testimony. These questions are related to how well the concentrations that are reported here are thought to represent the actual concentrations in the water from which the samples were taken and whether there is significant differences between the concentrations along the east shore and the concentrations in the intake. I guess the simplest, most straightforward way to ask the question is the following:

There is shown here for the day sampling an average value of 2.99 for the east concentration, and 1.41 for the intake concentration. Is there reason to believe that these concentrations actually were significantly different? Were the sampling devices used so nearly the same or susceptible to the same accuracy? Were the conditions so nearly the same that one can consider that these do actually represent different concentrations?

You may answer together since some samples, I believe, were taken by one group and some by another.

WITNESS LAWLER: At this point, Mr. Briggs, there have not been any analyses of significance or confidence limits put.

CHAIRMAN JENSCH: Speak louder, please.

WITNESS LAWLER: At this point there have not been -- there has not been any statistical analysis applied to these data. These data are simply the means of whatever data were available for these particular transects in the river and intake samples in the intake.

It is conceivable that there are not significant differences between these numbers.

MR. BRIGGS: Well, it is stated here that this information is used to, I believe, demonstrate that these larvae avoid the intake, so is what you are saying that there's no statistical analysis that shows that the larvae which are present along the east shore tend to avoid the intake?

WITNESS LAWLER: No, there is not.

MR. BRIGGS: All these data, I believe, were taken on one day, is that right?

WITNESS LAWLER: That's correct. Right.

MR. BRIGGS: Do you have data which show that the concentration -- other than these data -- that show that the concentration of larvae in the channel is consistently higher than the concentration along the shore?

WITNESS LAWLER: Well, I think that the analysis presented in the series of previous tables, where we define an F one which addressed itself to the distribution of larvae across the river cross-section would suggest differences,

perhaps not on the shore itself because those samples in the earlier tables were not taken on the shore, but they do suggest differences.

MR. BRIGGS: So the differences we see here may be attributed to differences between the concentration in the channels as opposed to the concentration along the shore rather than the fish tending just to avoid the inlet?

WITNESS LAWLER: That is possible, and I think there is some discussion of that in the text. If you recall, this follows after the discussion of the presentation of the distribution factor, F factor, if you will, refers to the -- what I call the quadrant average. So rather than simply use the east channel -- the east -- or east shore sample, that is the sample taken in the immediate vicinity of the plant, I generally use the average of that quadrant which involved taking the east channel transect as well as the east transect.

I think that's discussed in the text.

MR. BRIGGS: I believe there is some mention of it, yes.

There was some discussion previously about efficiency of collection and some mention during the day about the wide variability of the numbers that have been obtained. Although you haven't put confidence limits on the numbers here, I'd like to ask a question or two similar to some that were asked the other day.

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It is shown here that the average for the day sampling, east is 2.99. Is it possible that that number is -- could just as well be two as 2.99?

WITNESS LAWLER: I'd really have to look at the data on the river samples to answer that question, data on the river samples was relatively complete and it is conceivable that a statistical analysis could be applied to that. I would rather not at this moment say yes, the average could equally well be two as it could be three.

This is a -- what you are referring -- this is the mean of all samples observed and you are simply referring to the fact that the true mean of the population along the east shore,you are asking a question, could it be as low as two, or for that matter could it be as high as four. I'll put it another way: Certainly it could, depending on what confidence limit you associate with those, you know, with that range, within which you made your report.

MR. BRIGGS: Thank you. No further questions.

CHAIRMAN JENSCH: Is it the thought that all interrogation of Messrs. Lawler and Lauer have been completed, or just for Dr. Lauer?

MR. MACBETH: Just Dr. Lauer.

CHAIRMAN JENSCH: I see. Will Dr. Lauer be here tomorrow?

MR. TROSTEN: May I confer?

CHAIRMAN JENSCH: There is no request that he be here. We would just want to inquire.

MR. TROSTEN: Yes.

(Conference between counsel and Witness Lauer.)

MR. TROSTEN: Dr. Lauer can be here tomorrow, Mr. Chairman. He can be here.

CHAIRMAN JENSCH: Well, as far as I know, there is no request, but I understood your response to my inquiry about redirect, you said not at this time. I wondered if you planned to have any redirect of him or any further interrogation reasonably related in time to that which we have had today?

MR. TROSTEN: No. I think it would be helpful if Dr. Lauer were here during the cross-examination of Dr. Lawler since some of the questions have a tendency to go back and forth.

CHAIRMAN JENSCH: Is 9:00 o'clock a convenient time to reconvene in the morning?

MR. KARMAN: Mr. Chairman, I don't have access to a car and I do take the shuttle bus from Bethesda down here, and it doesn't get to 17th Street until about 9:00 o'clock, and it usually takes me about 10 or 15 minutes to get here. 9:00 would be difficult.

CHAIRMAN JENSCH: 9:15 would be all right?

MR. KARMAN: If all goes well.

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CHAIRMAN JENSCH: Well, if there is reasonable assurance that that's as far as it would go.

MR. MACBETH: Mr. Chairman, could I put a couple of questions about what I think are typographical errors to Dr. Lawler? I think it would make it easier in the morning. We could think about it over the night. It seems to me there are two or three words --

CHAIRMAN JENSCH: Why don't you talk to him off the record and if that doesn't help it, I'll straighten it out in the morning.

At this time let's recess to reconvene in this room tomorrow morning at 9:15.

(Whereupon, at 5:10 p.m., the hearing was adjourned, to reconvene at 9:15 a.m., Wednesday, 13 December 1972.)

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