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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

PLACED IN REGULATORY CENTRAL FILES
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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)	

Ground Floor Hearing Room
8120 Woodmont Avenue
Bethesda, Maryland

Thursday, 26 April 1973

Hearing in the above-entitled matter was reconvened, pursuant to adjournment, at 9 a.m.

BEFORE:

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

DR. JOHN C. GEYER, Member.

MR. R. B. BRIGGS, Member.

APPEARANCES:

(As heretofore noted.)

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C O N T E N T S

WITNESS:	DIRECT	CROSS	REDIRECT	RECROSS
Dr. C. P. Goodyear (resumed)		11,265	11,277	11,313
Harry G. Woodbury (recalled)			11,315	11,317
Karl Kniel (recalled)	11,319			
James C. McFadden (recalled)	11,323	11,335		11,353
Dr. John P. Lawler (recalled)		11,385	11,392	

EXHIBITS

None.

("Set I, Staff Responses to Applicant's Interrogatories on Supplemental Staff Testimony on the Feasibility of a Fish Hatchery and Replacement by Stocking of Fish in the Hudson River," and "Request for Additional Information," omitted from hearing record for 25 April 1973, is included herein following page 11,451.)

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P R O C E E D I N G S

CHAIRMAN JENSCH: Please come to order.

Last evening, I believe, the Applicant desired to adduce some evidence through Mr. Woodbury. Is he here?

MR. TROSTEN: Mr. Chairman, he isn't here at the moment, but if we can continue with Mr. Macbeth's cross-examination of Dr. Goodyear --

CHAIRMAN JENSCH: All right. Proceed.

Whereupon,

C. PHILIP GOODYEAR

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

CROSS-EXAMINATION (Continued)

BY MR. MACBETH:

Q Dr. Goodyear, I turn to page 7 of your testimony of 23 April, the test analysis, and there in discussing material in Table 1, you say that, "In extrapolating these data, I predict the ultimate usefulness," and so forth.

What is the basis of your opinion here that those figures are in fact probably overestimates?

A The estimate of brood stock survival is biased by the situation where most of the data, or a good portion of the data included in the table consisted of efforts to spawn and produce smaller numbers of fish, and as a result,

1 the mortality which is associated with the exportation of
2 the stock is lower.
3 In other words, if you look at the table in the
4 column under "Brood Stock," it is labeled "Pounds," and if
5 you compare the survival values to the number of pounds of
6 fish which were collected, you will notice that the lower
7 weights of fish that were exploited are associated with
8 higher survival rates.
9 The reason for this is partly related to more
10 care in handling the fish, and as a result, they hit higher
11 survival, but if you are required by your program, the goal
12 of your program to exploit, or to utilize a larger stock,
13 then the result of handling and taking fish which are not
14 exactly ripe for taking are reduced. Both the survival
15 estimate, that is, and the fractional spawn.
16 In the case of the phase two survival, these
17 estimates are based on restocking fish without transporting
18 a great deal, and in most cases represent fish which were fed
19 in the pond. So it is a cultural technique. Values for
20 survival indicate the survival rates in that period, that
21 indicates they would be cared for in the hatchery.
22 In the field, you would have to add to that
23 mortality associated with transporting the fish and
24 mortality once they are introduced into the environment.
25 I think that the values that are presented here

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1 are high estimates of the actual result that would be
2 derived from stocking.
3 Q I turn now to the second set of interrogatories
4 answering questions from the Applicant, to page 17, and
5 interrogatory number 11.
6 MR. TROSTEN: Mr. Macbeth, this was the set of
7 interrogatories introduced into evidence by Staff counsel,
8 is that correct?
9 MR. KARMAN: Set number two.
10 MR. MACBETH: Yes, it is by Staff counsel.
11 BY MR. MACBETH:
12 Q This interrogatory was discussing the research
13 program and asking what factors would be unresolved at the
14 end of the research program, and you state there a number
15 of factors which in your opinion would be unresolved.
16 CHAIRMAN JENSCH: That is what page and number?
17 MR. MACBETH: Page 17, question 11.
18 CHAIRMAN JENSCH: Thank you.
19 BY MR. MACBETH:
20 Q And discussing there the factors which would
21 be unresolved in your opinion at the end of the research
22 program, you say that another will be the question of
23 compensation as a factor operating within larvae populations
24 for striped bass.
25 Why, in your opinion, would the question of

1 compensation as a factor operating in that population be
2 unresolved at the end of the research program?
3 A Principally, the simple fact that the very small
4 portion of the mortality which could be controlled through
5 compensatory effect as much as 5 percent mortality, as a
6 matter of fact, could produce a very substantial change,
7 or a very substantial compensatory change in the survival
8 rate.
9 In order to detect and demonstrate that such a
10 change is in fact present, one would have to describe the
11 various factors involved in survival with sufficient precision
12 to demonstrate the existence of a factor which could be a
13 very small component of the survival rate itself.
14 I might point out, also, that the compensation in
15 growth does not necessarily have any effect on the survival
16 rate. So that that sort of information is not really
17 very useful, per se. It provides various information for
18 other reasons.
19 Q You also say in the following sentence that
20 generally speaking, factors which will be unresolved will
21 be adequate quantification, processes which control the
22 magnitude of important biological parameters, such as
23 mortality rate within the population and growth rates within
24 the population, et cetera.
25 Why, in your opinion, would the mortality rate be

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1 unresolved without good quantification?

2 A The mortality rate itself may be fairly well

3 described from the superficial standpoint.

4 In other words, the total mortality and the rate

5 of change in concentration of fish at various life stages

6 may be described with some precision, but the factors which

7 control the magnitude of the mortality rate are much more

8 difficult to quantify, and presently, as far as I am aware,

9 there is not a specific effort designed to study the factors

10 which are causing the mortality rate.

11 This is related to the statement above on

12 compensation, because the factors which control magnitude of

13 mortality rate are the factors which control the, or the

14 presence or absence of the compensatory mechanism.

15 CHAIRMAN JENSCH: If you will use the microphone

16 as much as you can, Dr. Goodyear, and speak loudly. Some-

17 times your voice drops because you are so close to Mr.

18 Macbeth.

19 BY MR. MACBETH:

20 Q Doctor, let me see if I understand you fully.

21 In your first response, I was left with the impression that

22 it would be difficult to measure the mortality rate with

23 the fineness of precision that would be necessary, variations

24 in the change of the mortality rate, and your second answer

25 that it would not be probable with the present research

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1 design to identify the factors which would induce changes
2 in the mortality rate of that fineness.
3 Have I understood those answers correctly?
4 A Pretty much. There are really two different ways
5 of looking at a mortality rate. If you just simply want
6 to describe the rate of decay in a population, then all
7 one has to do is to obtain estimates of the size of the
8 population at specific intervals. If, on the other hand,
9 you want to know how a population would respond to the
10 stress, then you need to know what factors cause the
11 mortality that you are observing.
12 My feeling concerning the capability of demon-
13 strating compensation is related to the fact that one has
14 to describe the components of the mortality rate with
15 sufficient precision to demonstrate their contribution.
16 In other words, one would have to separate the various
17 components statistically.
18 Q What would those components be likely to be? I
19 want to know in a general sense, so I know the kinds of
20 components you are thinking of in the situation.
21 A One of the components would be a mortality which
22 is associated primarily with the stage of development of
23 the organism. For the first 15 days of its existence,
24 depending on temperature, striped bass larvae really don't
25 have to feed to survive, and any metabolic or physical

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1 deficiency in their makeup doesn't show up, necessarily,
2 until their food resources are completely exhausted. At
3 that point, there is a loss which is related to the state
4 of development, which is in effect a genetic component,
5 and would not be expected to change all that greatly in
6 degree. That is one component of the mortality rate, which
7 would be very difficult to determine simply because the
8 factors which control it are related to so many other features
9 in the environment -- for instance, temperature.
10 The predation would be considered another
11 component in the mortality rate, and many other features as
12 well. However, I think you can get an idea of the problem
13 of determining compensation by realizing that what you have
14 to do is to demonstrate that the change in mortality which
15 could be attributable to nondevelopmental stage losses is
16 changing with the density of the fish. That can be a very
17 difficult task, and I have not seen a specific discussion
18 anywhere which indicates a methodology to do so.
19 I could provide a more detailed discussion, but
20 not on the spur of the moment.
21 MR. MACBETH: I think that is sufficient. I
22 have no further questions at this time.
CHAIRMAN JENSCH: Does Applicant have any ques-

BY MR. TROSTEN:

Q Dr. Goodyear, you indicated in Table 1 of your April 23 testimony that lower survival is associated with the higher pounds of broodstock, is that correct?

A Generally speaking.

Q Is it correct that you have a .88 survival associated with 6500 pounds of brood stock from Moncks Corner?

A Yes.

Q And do you have a .5 survival associated -- .58 survival -- associated with 354 pounds from Richmond Hill?

A Yes.

Q And do you have a .09 survival associated with no clear number of pounds from Fayetteville? Is there no data on the pounds from Fayetteville?

A The pounds were not reported. Well, you can see the breakdown which is presented. It was 107 females, but the weight of those fish was not reported.

Q Well, what order of magnitude would be the weight?

A Probably around 1000 pounds, plus or minus.

Q You also have a .5 survival assoiated with 421 pounds from Richmond Hill. Is that correct?

A Yes.

Q In addition to that, you have .75 survival

1 associated with 2655 pounds from Moncks Corner, is that
2 correct?
3 A Yes.
4 MR. TROSTEN: Thank you, Dr. Goodyear. I have
5 no further questions.
6 CHAIRMAN JENSCH: I wonder if you could give us a
7 little further on that. The figures are shown, that you
8 addressed correctly from the document, I wondered what the
9 next sequence of questions would be as a net result of all
10 those recitals you read from the document.
11 MR. TROSTEN: The conclusion that I would draw
12 from this, Mr. Chairman, just hearing Dr. Goodyear for the
13 first time, is that it really isn't correct generally to say
14 that the lower pounds -- that the higher pounds -- of survival
15 are associated -- I am sorry.
16 That the higher pounds of fish are associated
17 with lower percents of survival. As I look down the table,
18 I see that there is indeed a 1.0 survival associated with
19 low numbers of pounds from Edenton, but generally speaking,
20 looking at the table, I would say it is a stand-off, if
21 anything. It looks to me in many cases the higher
22 percent survival is associated with the higher pounds. I
23 could simply refer the Board to those two columns, pounds
24 and survival.
25 CHAIRMAN JENSCH: Dr. Goodyear, could you give us a

1 comment on that conclusion?

2 THE WITNESS: The survival in Edenton which

3 demonstrates the highest survival indicated in the table,

4 all comes from fish handled with extra care. The elimina-

5 tion of those fish from the column would change the overall

6 estimate by some margin, I am not sure exactly how much,

7 but it would change it in a downward direction.

8 CHAIRMAN JENSCH: You mentioned Moncks Corner

9 there, too. The poundage there is much higher. How about

10 Richmond Hill? Is it necessarily related to the poundage

11 there? There is a lesser poundage from Richmond Hill.

12 THE WITNESS: The survival values that are

13 presented throughout are partly dependent upon the type of

14 collecting, the origin of the fish, and the Richmond Hill

15 efforts are based on collections, a lot of them, from

16 various fishing techniques. Normally, you would expect

17 to have even higher survivals are presented in the table

18 than in the data I have. Those survival rates are biased

19 partly because the mortality that is associated with the

20 ovulating technique, in other words, after the ejection of

21 the hormone, is included in the survival column. It is not

22 related to the transportation and selection mortality,

23 and if you will -- well, almost all of the fish in the

24 Richmond Hill survival column were dying after the injection

25 of the hormones.

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MR. BRIGGS: Can you really conclude anything from these numbers on effects on pounds of fish and the survival other than you feel that if one takes greater care and one would be able to take greater care with a small number of fish, that you would expect greater survival?

THE WITNESS: Basically, that is correct.

MR. BRIGGS: I don't want to interrupt you, but remember, you are comparing the survival with large weights of fish, not the small weights at Edenton, but the .77, and then the fraction spawned with the .77, that you used to get your final number of .0081.

THE WITNESS: There is another factor that is involved in the extrapolations there. The survival is the survival of individuals, and the size of the individuals affects the pound estimate quite a bit. I will have to check to make sure that the comments that I was making were accurate, but I still feel that they are. But it is based more on the number of fish than on the pounds. It is not necessarily an accurate statement that I made earlier.

MR. BRIGGS: But depending how one selects the numbers here, he can reach a variety of conclusions based on the number in this table?

THE WITNESS: Yes. I quite agree.

CHAIRMAN JENSCH: Are there any further questions? Do you have redirect?

MR. KARMAN: I have a few questions, Mr. Chairman. Could we have a recess?

CHAIRMAN JENSCH: We will recess to reconvene in this room at 9:40 a.m.

(Recess.)

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CHAIRMAN JENSCH: Please come to order.

You had completed, have you not, Applicant, cross-examination of Dr. Goodyear?

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: Is there redirect?

MR. KARMAN: I have a few questions, Mr. Chairman.

REDIRECT EXAMINATION

BY MR. KARMAN:

Q Dr. Goodyear, I call your attention to the testimony submitted by Dr. Lawler dated April 20, 1973, on the striped bass hatchery stock replenishment estimates, and I specifically call your attention to page 11 of such testimony. In the last paragraph on page 11, Dr. Lawler indicates, and he is referring to Table 6 of his testimony which is the page following page 11, he states, "The numbers appearing in Table 6 were taken from the computer runs (given to the Applicant by the Staff) used to generate percentage reductions for multiplant effects in the Staff's February 8, 1973 testimony."

Do you have any comment on that sentence, Dr. Goodyear?

A Yes. The discussion which appears on page 11 and the table on page 12 includes discussions of the model which was used not for producing the percentage reductions for multiplant effects as indicated, but for verification of the distribution of fish in the estuary for the flow conditions which were used in the model as input.

The column in Table 1, or Table 6, rather, labeled "No Recruits to Year Class 1" were not a portion of the model which was used, or a portion of the output in the model used to generate the percentage reduction of figures.

Q Pardon me, Dr. Goodyear. What was that table entitled, or that column, rather, that you were talking about?

A "Recruits to Year Class 1." There are three columns.

Q Under which heading is the one that you are referring to?

A It is in reference to all three columns.

Q I see. Thank you.

A The reason for eliminating that portion of the model in the computer runs used to produce the calculations that were presented in previous testimony is that the carrying capacity is based on an arbitrary estimate and it is not consistent with the concept of a completely independent survival of young-of-the-year fish, and as a result it is not consistent with the information which I had evaluated in relationship to striped bass.

However, the distribution data which is on output of the model which produces relative distributions before the step in the model that this information came from, before the step that is used to crop the individual compartment standing crop to the carrying capacity, is a method of verification of the model which was used in previous testimony.

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That component was used and was submitted to the Applicant, as well as the runs which were used to produce the percentage reduction figures.

Now, my use of this model that Dr. Lawler is describing was in producing the distribution figures which show a longitudinal distribution of relative abundance and concentration at specified time periods. There is another factor related to these figures that was the subject of some misunderstanding on the computations that have been present ed on my part.

The 30 to 50 percent estimate that has been presented, or was presented in the final environmental statement was based on an assumption that the average year would have flow characteristics as indicated by mean flows for the months of May, June, and July, and that value was produced under those assumptions.

The range that is indicated is a range based on variation in the methods of producing the output in the model, in other words, the variations in the parameters which would be input, from the point of view of where the salt front is assumed to be and the magnitude of the density of flows, which were also assumptions.

The variation which is produced was produced in the table in my previous testimony that showed a ---

MR. TROSTEN: Which previous testimony?

THE WITNESS: This was on multiplant effects.

I believe it was 14 to 43 percent. It is based on a constant assumption concerning the variations in magnitude of density induced flows, et cetera, but where the flow was allowed to vary from one type of flow condition to another, and periods which are typically drought periods, the flows are below normal. They show the reduced impact of the plant, which is not associated with the assumptions concerning the position of the salt front as a function of flow.

Likewise, high flow conditions produce low impact by moving the population downstream from the plant. So the two types of variations are different in nature. This is another problem that is going to have to be resolved in determining population levels and fluctuations in the estuary which from a sampling point, because the distribution to a great extent is influenced by early flow conditions in the estuary.

Q Dr. Goodyear, I now call your attention to the testimony submitted and dated April 24, 1973, by Dr. McFadden, entitled, "A Response To Staff Comments on Applicant's Research Program."

On page 1 of such testimony, Dr. McFadden in drawing a distinction between an experimental approach which is "mostly descriptive and one which is more analytical in nature", states, "the implication is that the Applicants," and now he is referring to your testimony of April 10, 1973 on the Applicant's research program, Dr. McFadden states, "The implication is that the Applicant's research program is descriptive rather than

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analytical, and this characteristic dooms it to failure in its attempt to detect possible environmental impacts in sufficient time to mitigate them."

Do you have any comments on that statement, Dr. Goodyear?

A Yes. The implication as it is presented in his testimony was not the intent of my discussion of the importance of descriptive versus analytical approaches to research. I think that it is quite possible for the research program to detect changes in populations with sufficient precision to demonstrate the change which has occurred.

However, the problem that must be resolved in that relationship is that it is necessary to demonstrate the cause of such changes. This is a very difficult thing to do, and based on the stated "null hypothesis" has been presented, which is that the attempts -- let me back up. The "~~no~~ null hypothesis" is based on the presumption that it will be necessary to statistically demonstrate an effect of plant operation before it will be concluded that the plant has had an effect.

That has nothing to do with describing what changes are occurring in the population. I think Dr. McFadden stated that "~~no~~ null hypothesis" -- this is on transcript page 7505, lines 21 through 23 -- stated, and I quote him, "The ~~no~~ null hypothesis which we seek to disprove is there is no effect by the plant."

This means that all of the sampling variation, variability, and the degree of precision with which the controlling

1 parameters are measured, will influence their ability to reject
2 the hypothesis that they are propounding.
3 Consequently, the difficulty is liable to arise as a re-
4 sult of the studies which are being undertaken, that is, that
5 at the end of the studies the data will not be sufficiently
6 precise to allow rejection of that hypothesis. With a conse-
7 quence, the same sort of the situation will develop then as
8 is present now, because several factors can be influencing
9 the population at the same time.
10 Q Dr. Goodyear, I call your attention to additional
11 testimony of Dr. John P. Lawler dated April 20, 1973 related
12 to the contribution of Chesapeake Bay to the striped bass
13 fishery in the Middle Atlantic States.
14 On page 7 of such testimony, Dr. Lawler is discussing
15 corrections which you applied, and he quotes them, certainly
16 one with respect to a column B in a table. Dr. Lawler states
17 "We cannot agree with the inclusion of column B. We have already
18 tried to account for the movement out of the Chesapeake as
19 described on pages D-16 and D-17 of the April 9, 1973 testimony.
20 "Application of Column B is redundant, i.e., it does the
21 same thing twice."
22 Do you have a comment on that?
23 A Yes. The inclusion of column B in my comments was
24 based on the assumption that only 50 percent of the total
25 portion of the over age group can be shown to be exploited

1 outside of the Bay. The other 50 percent are exploited inside
2 of the Chesapeake system.
3 They are based on two different concepts.
4 Q On the same page, Dr. Goodyear, the last paragraph
5 on this page 7, Dr. Lawler states, "With respect to column C,
6 if Dr. Goodyear is suggesting that some of the Chesapeake Bay
7 migrants move through the Mid-Atlantic and into the North
8 Atlantic, then we agree, and so indicated on page D-19."
9 "We don't know what information he is using, however, to
10 obtain the estimate of 50 percent of the migrants being cap-
11 tured elsewhere, and doubt that such would apply over all age
12 groups."
13 Would you like to comment on this paragraph, Dr. Goodyear?
14 A Yes. I based my comment on the number of 50 percent
15 on the recapture data of fish which have been tagged inside
16 of the Chesapeake Bay and which were recaptured outside of the
17 Bay. Of the 115 such fish that I have been able to identify,
18 108 were found in three or four studies.
19 To provide the data base for the assumption, I have pro-
20 vided here a copy of the distribution of coastal recaptures
21 of striped bass which were tagged in Chesapeake Bay. Three
22 areas of recapture were indicated. The New England area is the
23 area north of the Mid-Atlantic States, as we have used them
24 in past testimony.
25 In describing the data which is present, I find that the

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number of point 5 was somewhat high, but I still believe that it is probably as accurate as any of the rest of the assumptions that were made.

The corresponding value is point 44.

Q Dr. Goodyear, on pages 12 through 13 of this same testimony of Dr. Lawler, Dr. Lawler quotes from the recent publication by Schaeffer, much of which was discussed at yesterday's session of the hearing. After having read this extract from Dr. Schaeffer, unless it is the whole one -- I don't know -- which is quoted on pages 12 and 13 of Dr. Lawler's testimony, is this Schaeffer analysis on regression consistent with your concept of the origin of the Mid-Atlantic striped bass fishery?

A Yes, it is. I might point out that it does provide a bit of information which is useful that I was not aware of before, and that is that the relationships are still evident, the regression relationships, are still evident in the young-of-the-year fish, which is known for West Coast populations, but had not been demonstrated in East Coast populations.

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MR. KARMAN: I have no further questions, Mr. Chairman.

I would like to offer into evidence now documents which have been distributed to the Board and to the parties, and which were used by Dr. Lawler -- Dr. Goodyear in his redirect testimony at this time.

It is titled "Distribution of Coastal Recapture Chesapeake Tagged Data."

CHAIRMAN JENSCH: What does the first sheet show?

MR. KARMAN: It is titled "Distribution of Coastal Recapture of Chesapeake Tagged Striped Bass."

CHAIRMAN JENSCH: The document consists of how many pages?

MR. KARMAN: One page; subsequent to that, there are attached 7 pages of computer plots related to relative abundance for the years 1949, 1955, 1964, 1967, 1968, 1969, and 1970.

In addition thereto, there are 7 pages of plots on relative distribution for the years -- I believe they are the same years.

MR. TROSTEN: Relative abundance?

MR. KARMAN: Concentration. I believe they are the same years, 1949, 1955, 1964, 1967, 1968, 1969, and 1970.

I have a one-sheet document attached, also, which is a revised regression analysis related to the distribution

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1 between the Hudson and the MidAtlantic, and then two pages of
2 literature references.
3 CHAIRMAN JENSCH: In this collection which you
4 gave us this morning and which you have identified at this
5 point, we do not have the references.
6 MR. KARMAN: This may have been attached inadvertently,
7 Mr. Chairman, this literature cited. It may have been stapled
8 to the other.
9 CHAIRMAN JENSCH: You do not intend to offer
10 those last two pages, then?
11 MR. KARMAN: No.
12 CHAIRMAN JENSCH: You gave to us a stapled
13 collection which included all you have described, which
14 started with "Distribution of Coastal Recapture of
15 Chesapeake Tagged Striped Bass," and ending with the single
16 sheet which you have identified as the revised regression
17 analysis, which is not so labeled, and has "28 November
18 1972" at the top, and then there are --
19 MR. KARMAN: That is correct, sir.
20 CHAIRMAN JENSCH: I wonder if you would consider
21 your proposal, that you propose there be incorporated within
22 the transcript that collection of papers that you stapled
23 together, and which I have now identified?
24 MR. KARMAN: I so make that offer, Mr. Chairman.
25 CHAIRMAN JENSCH: Very well.

1 MR. BRIGGS: I seem to have missed something.

2 To what do these charts apply, to what testimony?

3 MR. KARMAN: Could Dr. Goodyear respond to that?

4 MR. BRIGGS: Yes.

5 THE WITNESS: Those charts indicate the distri-

6 bution of eggs, yolk sack larvae, post larval fishes, and

7 juvenile fishes, which would be produced by the flow

8 conditions which were modeled for each of the years.

9 MR. BRIGGS: These are calculated numbers?

10 THE WITNESS: Those are calculated numbers.

11 MR. BRIGGS: To what testimony should they be

12 attached, or related to? There must be some testimony in

13 here where these were used.

14 THE WITNESS: They could be used either in con-

15 nection with the multiplant effects testimony that I

16 presented earlier, but in reference to the importance of the

17 location of the fish in relationship to flow conditions,

18 which influences the estimates that were made for the years

19 indicated, that we discussed a minute ago.

20 MR. BRIGGS: Are these provided in reply to any

21 requests for information, or substituted for previous charts

22 that were used in calculations?

23 THE WITNESS: Just a moment.

24 These plots are presented to help elucidate the point

25 that I was making concerning the importance of the distribution

1 the fish in connection with the explanation which I have
2 just given.
3 MR. BRIGGS: All right. Thank you.
4 DR. GEYER: In connection with the table which is
5 the first sheet of the offer just made, do these sources have
6 the information on the number tagged and those caught inside
7 the bay, for example, as compared to those caught outside?
8 THE WITNESS: Yes. That data is summarized in my
9 previous testimony concerning the origen of the midAtlantic
10 fisheries.
11 DR. GEYER: Could you give author reference to
12 that?
13 THE WITNESS: Just a moment.
14 DR. GEYER: If you would rather wait until we
15 have a recess and find it then, that would be quite all right.
16 MR.KARMAN: We should be able to get that for you.
17 CHAIRMAN JENSCH: Perhaps you could supply it
18 during the course of the proceedings, at this session.
19 MR. KARMAN: Yes, Mr. Chairman.
20 CHAIRMAN JENSCH: Have you concluded your
21 redirect?
22 MR. KARMAN: Yes, Mr. Chairman.
23 CHAIRMAN JENSCH: Mr. Briggs has some questions.
24 MR. KARMAN: That was accepted, my offer of
25 testimony, was it not?

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1 CHAIRMAN JENSCH: I don't know whether we would wait
2 for further identification -- if there are objections to
3 the collection of sheets which have been identified by
4 staff counsel being physically incorporated in the
5 transcript --
6 MR. TROSTEN: I have several questions I would
7 like to ask about this.
8 First of all, there are two sets of charts, one
9 is labeled "Concentration," and the other "Relative
10 Abundance."
11 Is that correct?
12 You said "Distribution," at one point, and I want
13 to be sure we are talking about the same thing.
14 MR. KARMAN: One is relative abundance, and one
15 says "Concentration."
16 MR. TROSTEN: All right.
17 Is it correct that these two sets of charts,
18 7 charts each, are being offered in support of Dr. Goodyear's
19 conclusion that 14 to 43 percent of the striped bass would
20 be taken by Indian Point Unites 1 and 2? Is that the purpose
21 of this offer?
22 I am not entirely clear why this is being offered.
23 THE WITNESS: That the variation from 14 to 43 per-
24 cent is related to the distribution of the fish.
25 MR. KARMAN: I think possibly, Mr. Trosten, that the

1 testimony as given by Dr. Goodyear this morning and with
2 this as a backup would speak for itself.
3 He explained rather lengthily this morning as
4 to the 14 to 43 and the 30 to 50 percent figures which have
5 been discussed, and where any differences would lie, and this
6 is his backup material, copies of which, of course, were
7 sent to the applicant many, many months ago.
8 MR. TROSTEN: Copies of which?
9 MR. KARMAN: I believe copies of this were sent
10 to the applicant many, many months ago.
11 MR. TROSTEN: I don't believe we have seen this
12 until yesterday, when you gave us this information, or
13 something like it.
14 MR. KARMAN: November 10, I believe, is the date
15 this was sent to you.
16 It was our understanding that it was.
17 MR. TROSTEN: Can you tell us what is the purpose
18 of the document entitled "November 29, 1972," dated November
19 29, 1972, which appears to be a revised regression analysis?
20 THE WITNESS: The original regression analysis was
21 based -- the one that was presented in the Final Environmental
22 Statement was based on the 3-point moving averages for the
23 midAtlantic landings and the Hudson landings.
24 The utilization of the 3-point moving average for
25 the midAtlantic landings is, in effect, making a comparison

1 much broader, and would serve to increase the apparent
2 accuracy, the apparent precision of the regression analysis,
3 and this particular regression was based on the same data
4 as the original regression, and does not use the 3-point
5 moving average for the Atlantic landing.
6 It produces the same basic description with
7 somewhat less precision, but still greater precision than
8 the data which was cited by Dr. Lawler in his last testimony,
9 which shows a relationship between the Chesapeake young-of-the-
10 year and the midAtlantic-New York ones, which is being used to
11 indicate the potential importance of the Chesapeake.
12 MR. TROSTEN: Let me be sure I understand this.
13 Is this regression analysis to be substituted
14 for a regression analysis that appears in the Final
15 Environmental Statement and, if so, would you tell me which
16 page it appears on?
17 THE WITNESS: It is supplemental.
18 MR. TROSTEN: To the regression analysis that
19 appears on what page?
20 THE WITNESS: The analysis presented on B-58,
21 Figure 514.
22 MR. TROSTEN: I have one other question I would like
23 to ask you.
24 Are these your products, Dr. Goodyear?
25 They are not signed. We have this problem with

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things not being signed.

THE WITNESS: Yes, they are.

MR. TROSTEN: I may have a question or two about it, but I have no objection to having it received in evidence.

CHAIRMAN JENSCH: Mr. Macbeth?

MR. MACBETH: No objection.

CHAIRMAN JENSCH: The Staff's offer, the first sheet of which is entitled "Distribution of Coastal Recapture of Chesapeake Tagged Striped Bass," and the last page of which bears the date November 29, 1972, will be physically incorporated in the transcript and shall constitute evidence on the part of the regulatory st aff.

ed 3 (Documents follow:)

DISTRIBUTION OF COASTAL RECAPTURE OF CHESAPEAKE TAGGED STRIPED BASS

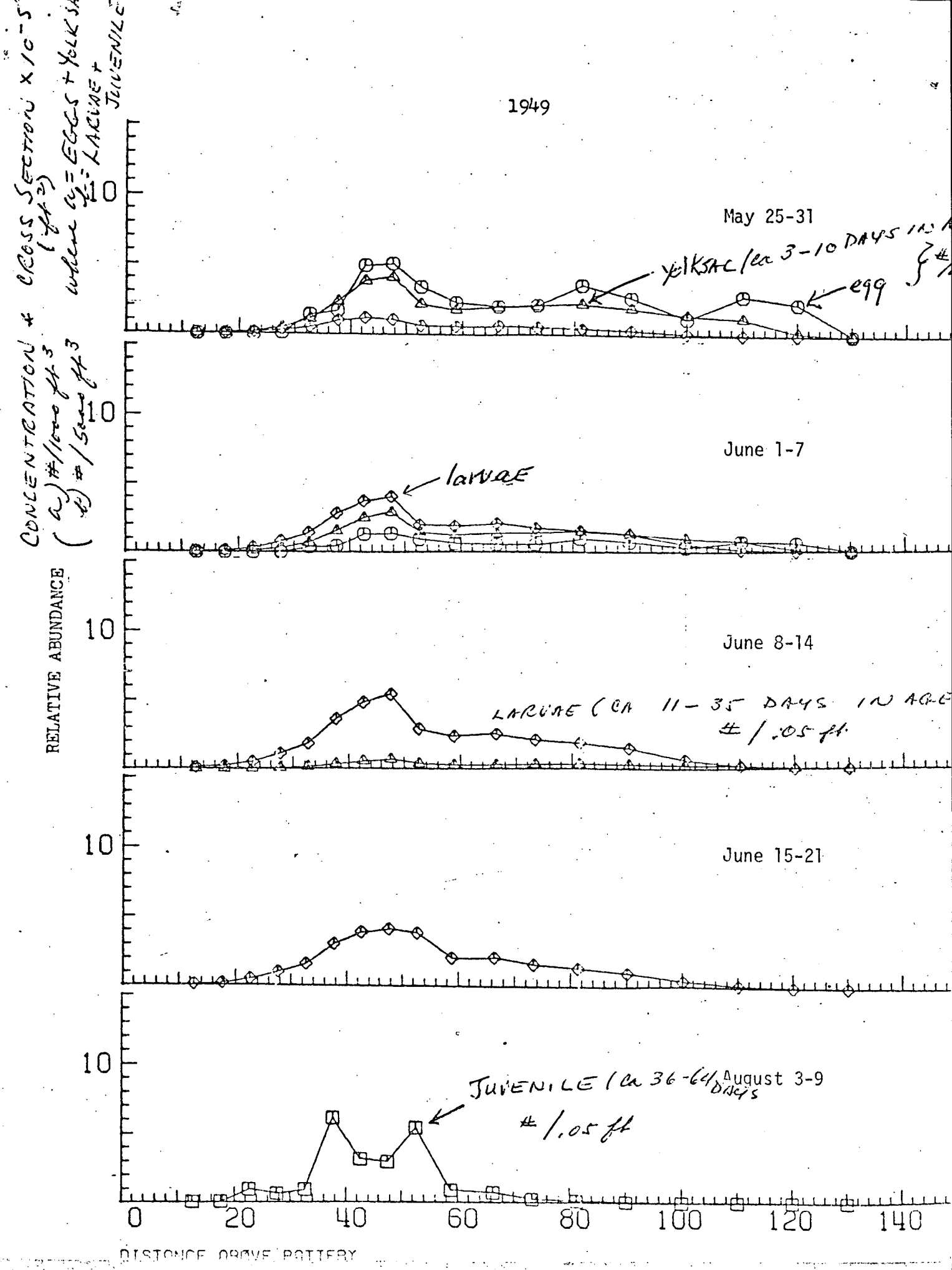
Area of Recapture	Number of fish recaptured by study				Total	Percent
	A	B	C	D		
New England	29	16	4	8	57	53
Mid Atlantic	23	13	6	6	48	44
South Atlantic		1	2		3	3
					108	

A. Nichols, P. R., and R. V. Miller. 1967. Seasonal movement of striped bass, *Roccus saxatilis* (Walbaum), tagged and released in the Potomac River, Maryland, 1959-1961. Ches. Sci. 8(2): 102-124.

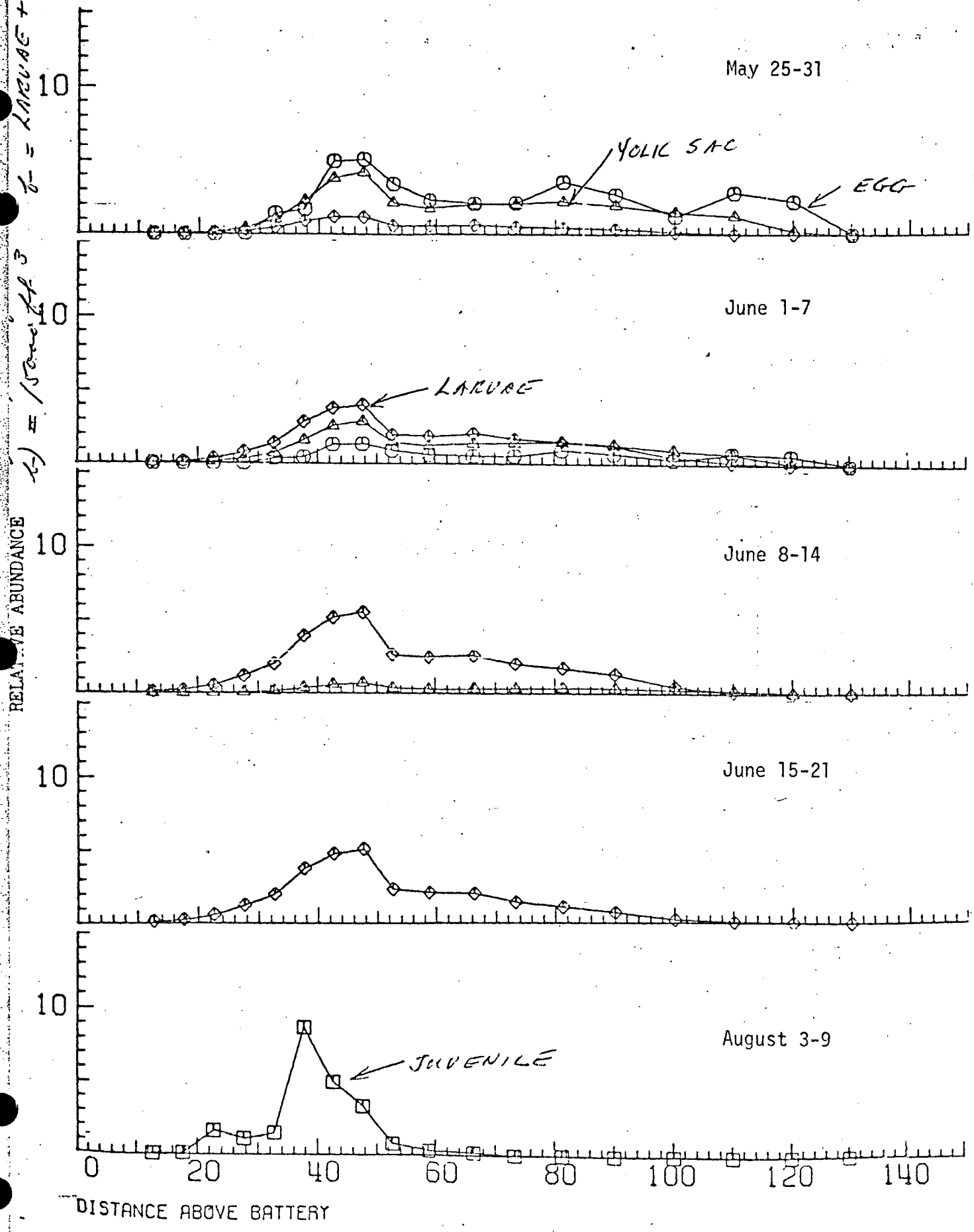
B. Vladykov, V. D., and Wallace, D. H. 1952. Studies of the striped bass, *Roccus saxatilis* (Walbaum), with special reference to the Chesapeake Bay region during 1936-1938. Bull. Bingham Oceanogr. Coll. 14(1): 132-177.

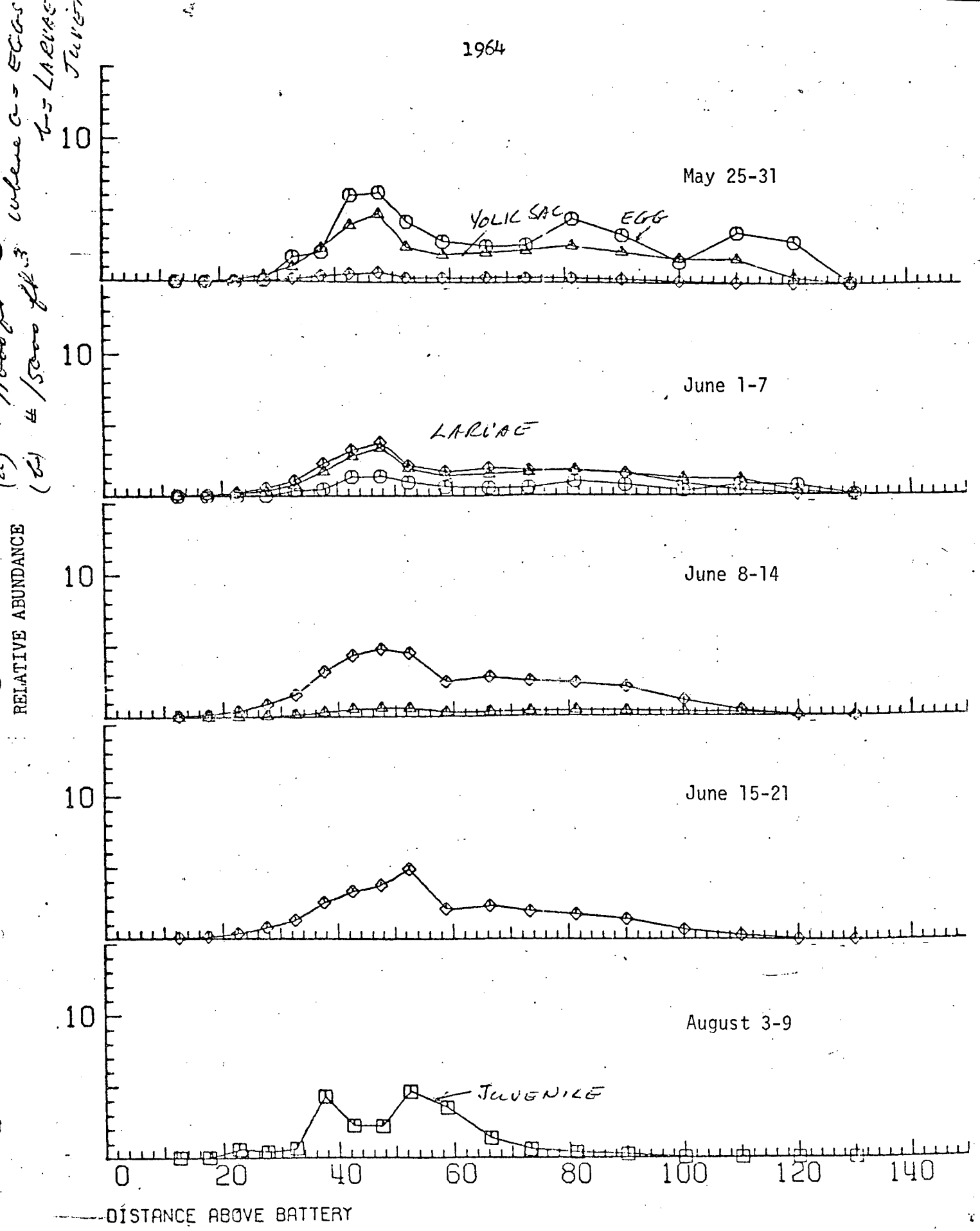
C. Massmann, W. H., and A. L. Pacheco. 1961. Movements of striped bass in Virginia waters of Chesapeake Bay. Ches. Sci. 2(1-2): 37-44.

D. Chapoton, R. B., and J. E. Sykes. 1961. Atlantic coast migration of large striped bass as evidenced by fisheries and tagging. Trans. Amer. Fish. Soc. 90(1): 13-20.

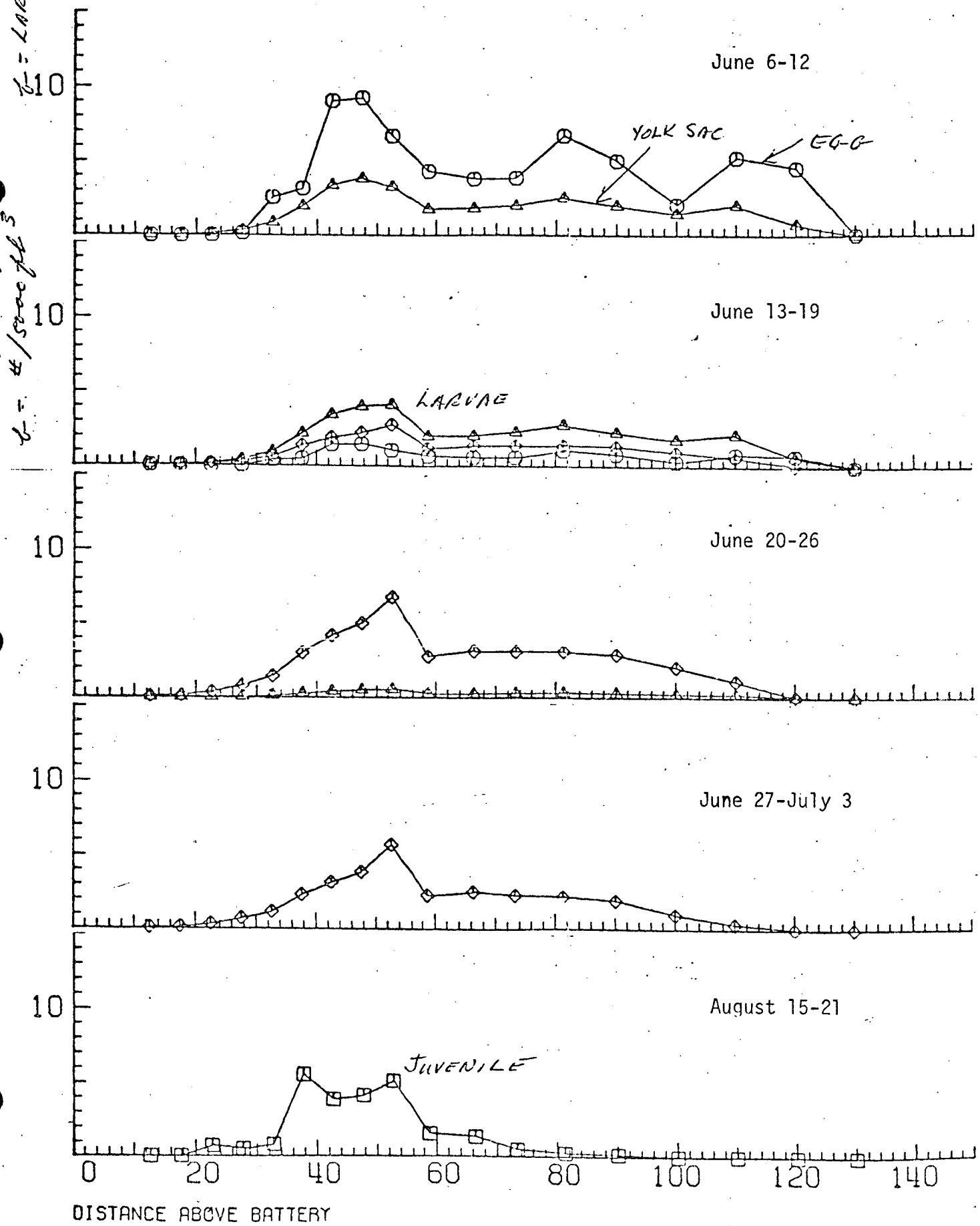

1949
CONCENTRATION * CROSS SECTION x 10-5 (ft²)
(a) #/1000 ft³
(b) #/5000 ft³
where a = EGGS + YOLK SAC
b = LARVAE + JUVENILE
RELATIVE ABUNDANCE
10
May 25-31
yolksac (ca 3-10 DAYS IN A
egg
June 1-7
larvae
June 8-14
LARVAE (CA 11-35 DAYS IN AGE
#/.05 ft
June 15-21
August 3-9
JUVENILE (ca 36-64 DAYS
#/.05 ft
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

1955

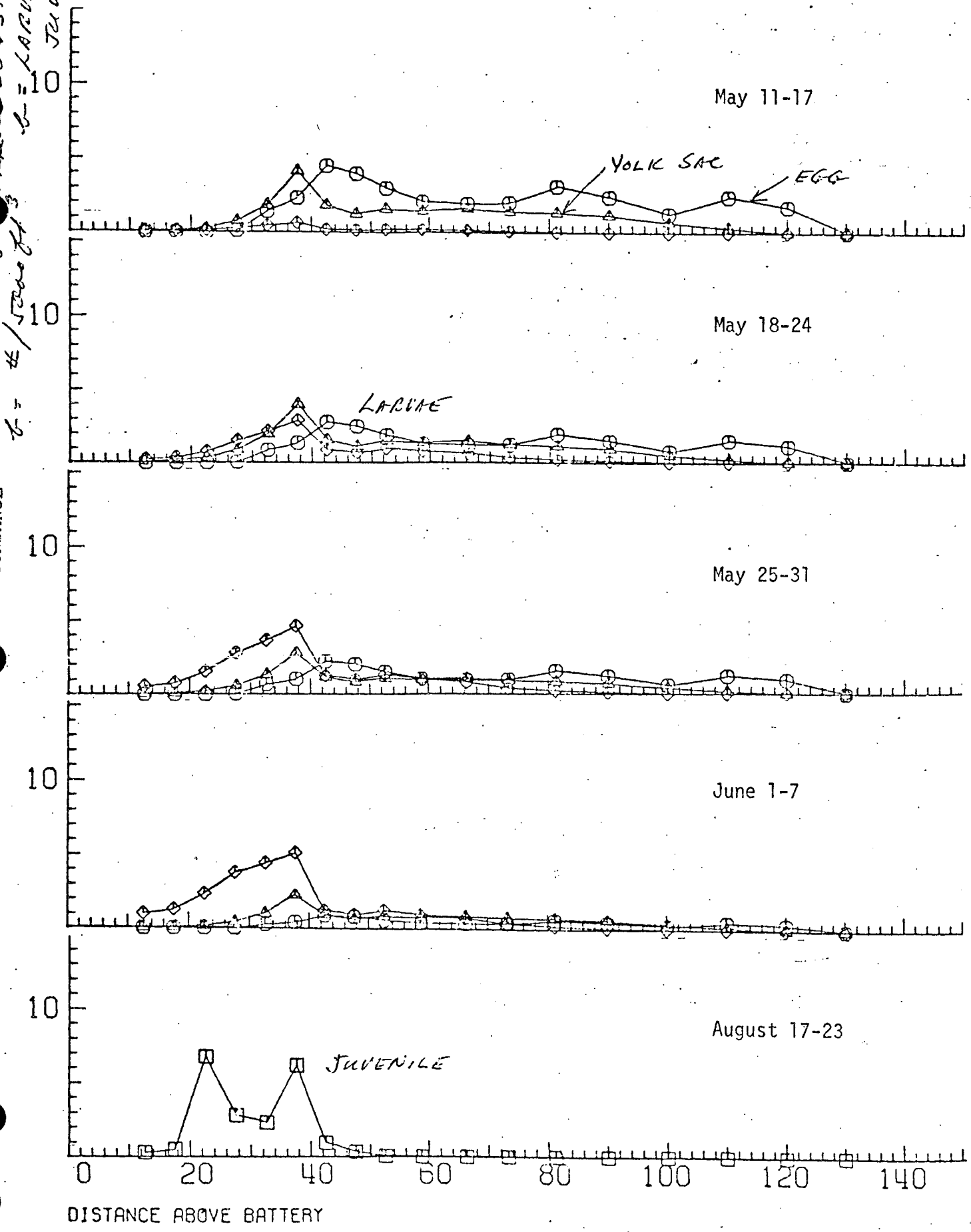

May 25-31
YOLK SAC
EGG
June 1-7
LARVAE
June 8-14
June 15-21
August 3-9
JUVENILE
10
0
20
40
60
80
100
120
140
RELATIVE ABUNDANCE
DISTANCE ABOVE BATTERY


1964
(a) #/1000 ft.3 where a = EGGS
(b) #/1000 ft.3 b = LARVAE
Juvenile
RELATIVE ABUNDANCE
10
May 25-31
YOLK SAC
EGG
June 1-7
LARVAE
June 8-14
June 15-21
August 3-9
JUVENILE
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

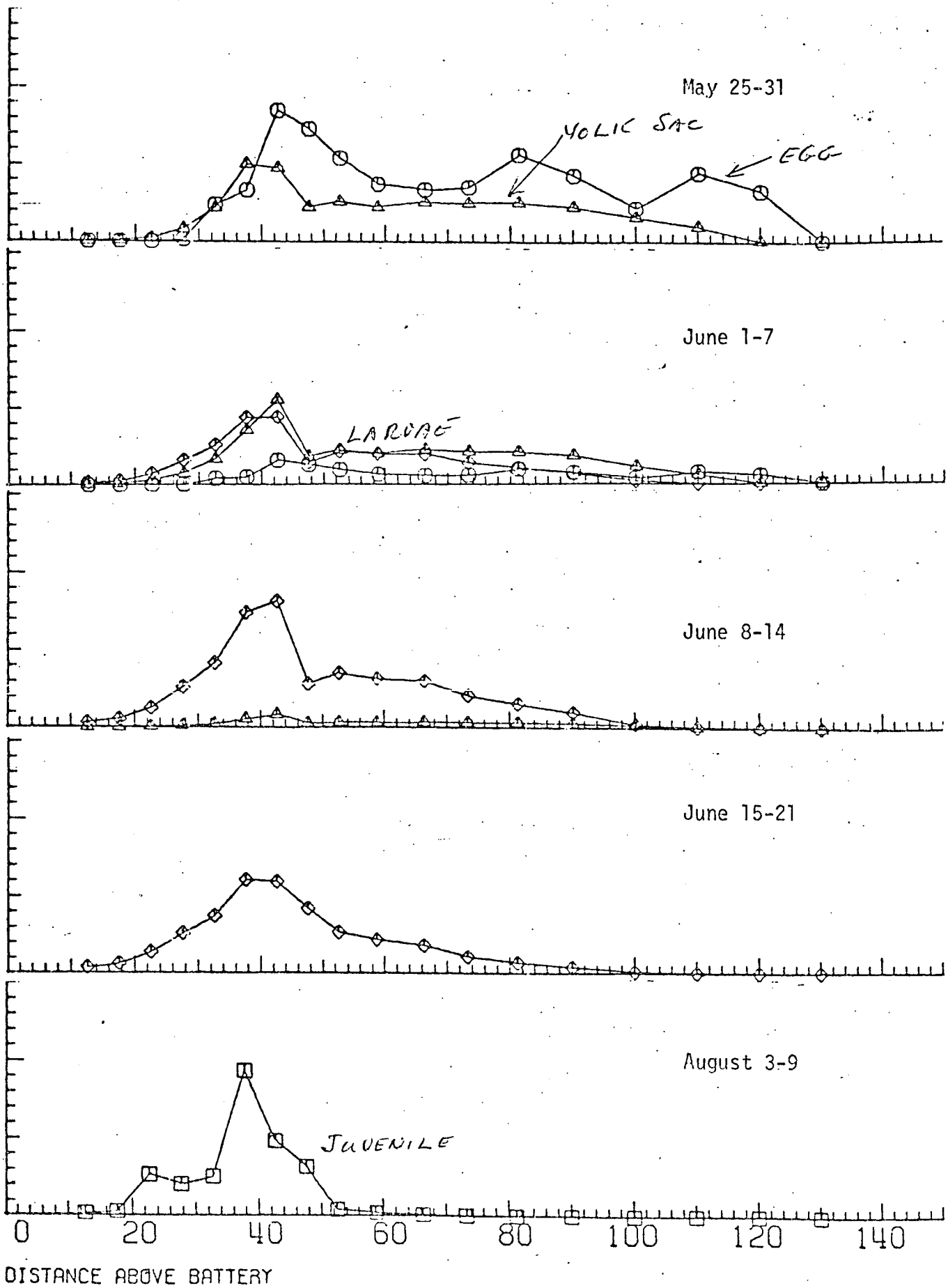
1967


June 6-12
YOLK SAC
EG-G
June 13-19
LARVAE
June 20-26
June 27-July 3
August 15-21
JUVENILE
10
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

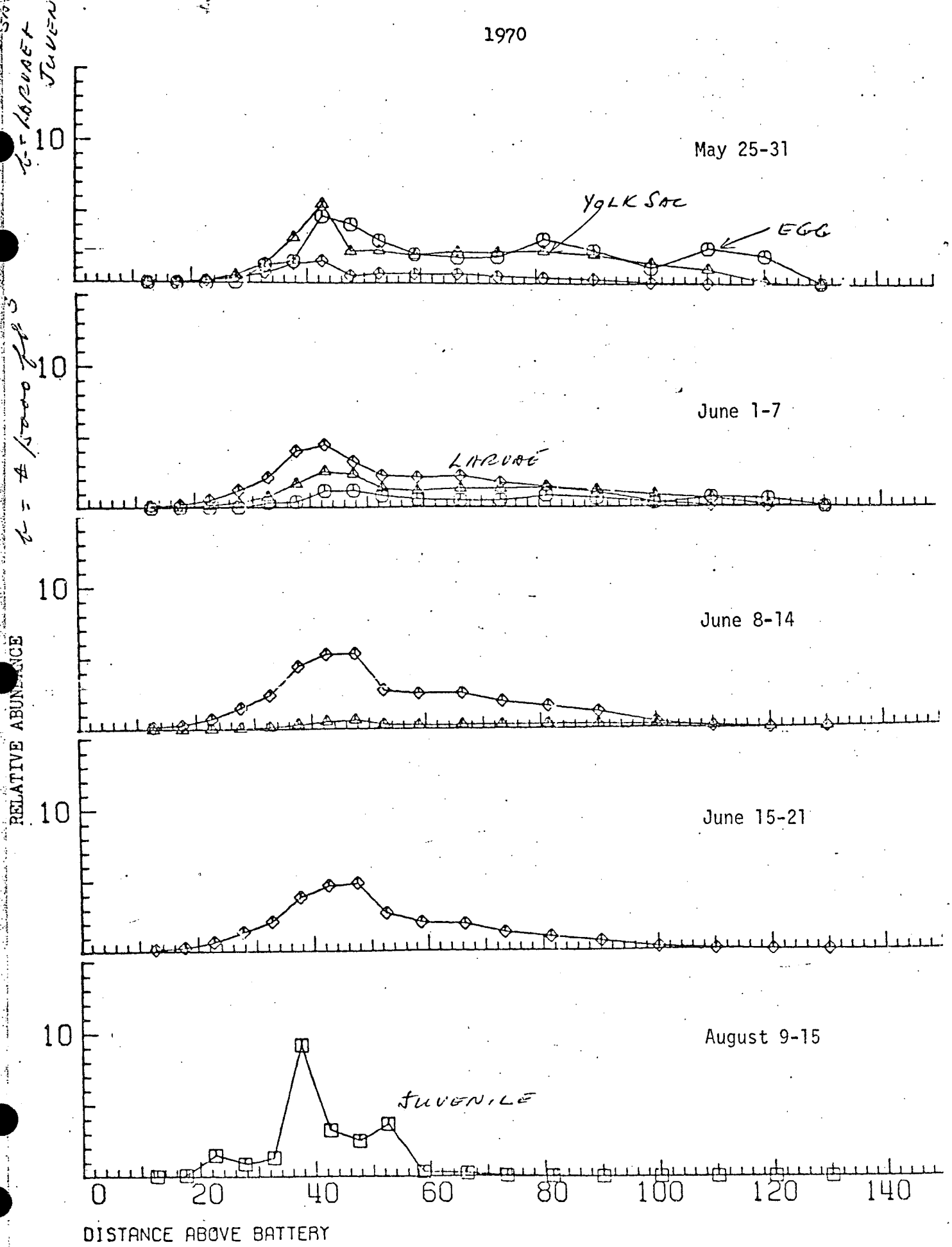
1968

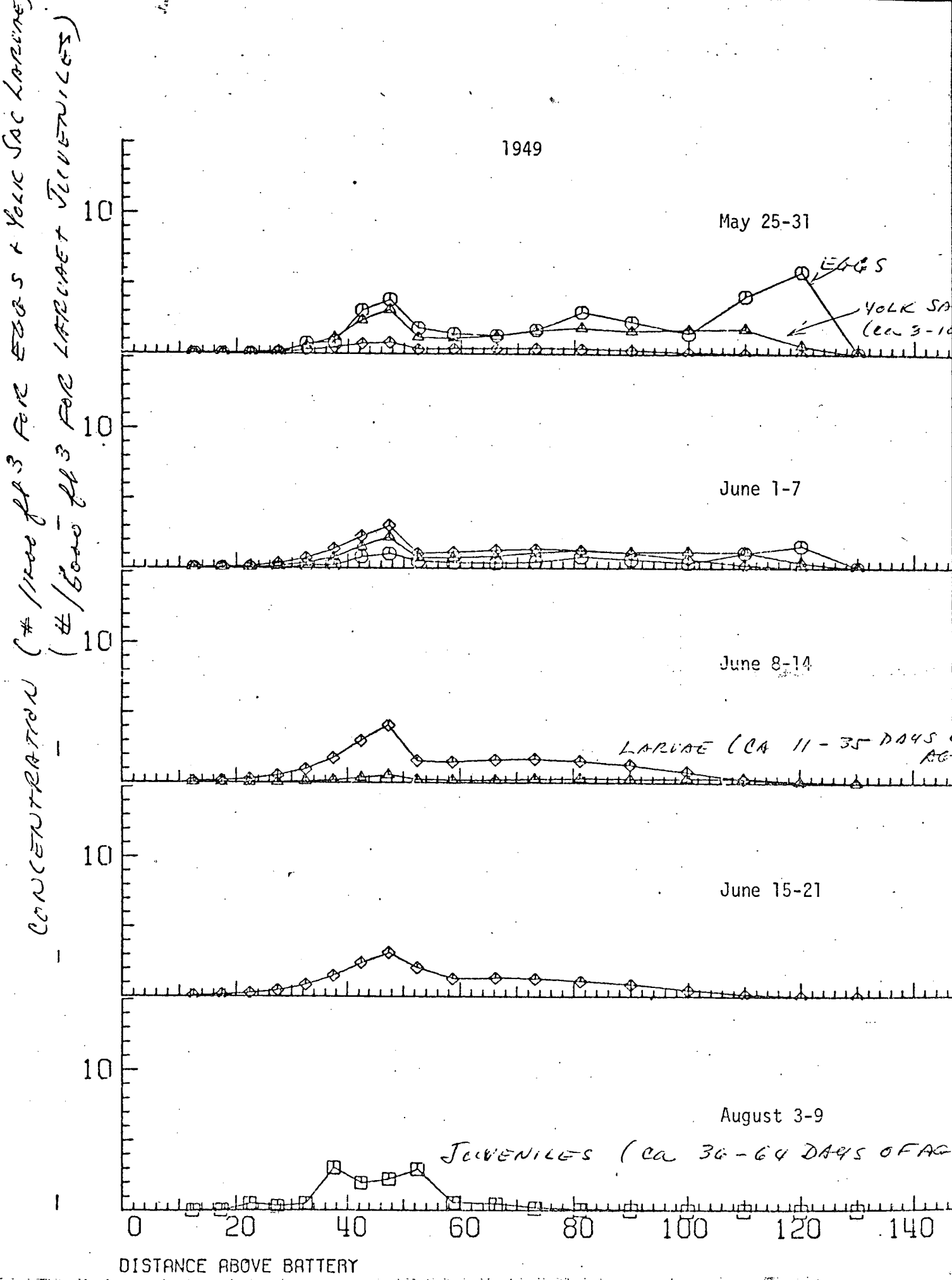

10
May 11-17
YOLK SAC
EGG
10
May 18-24
LARVAE
10
May 25-31
10
June 1-7
10
August 17-23
JUVENILE
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

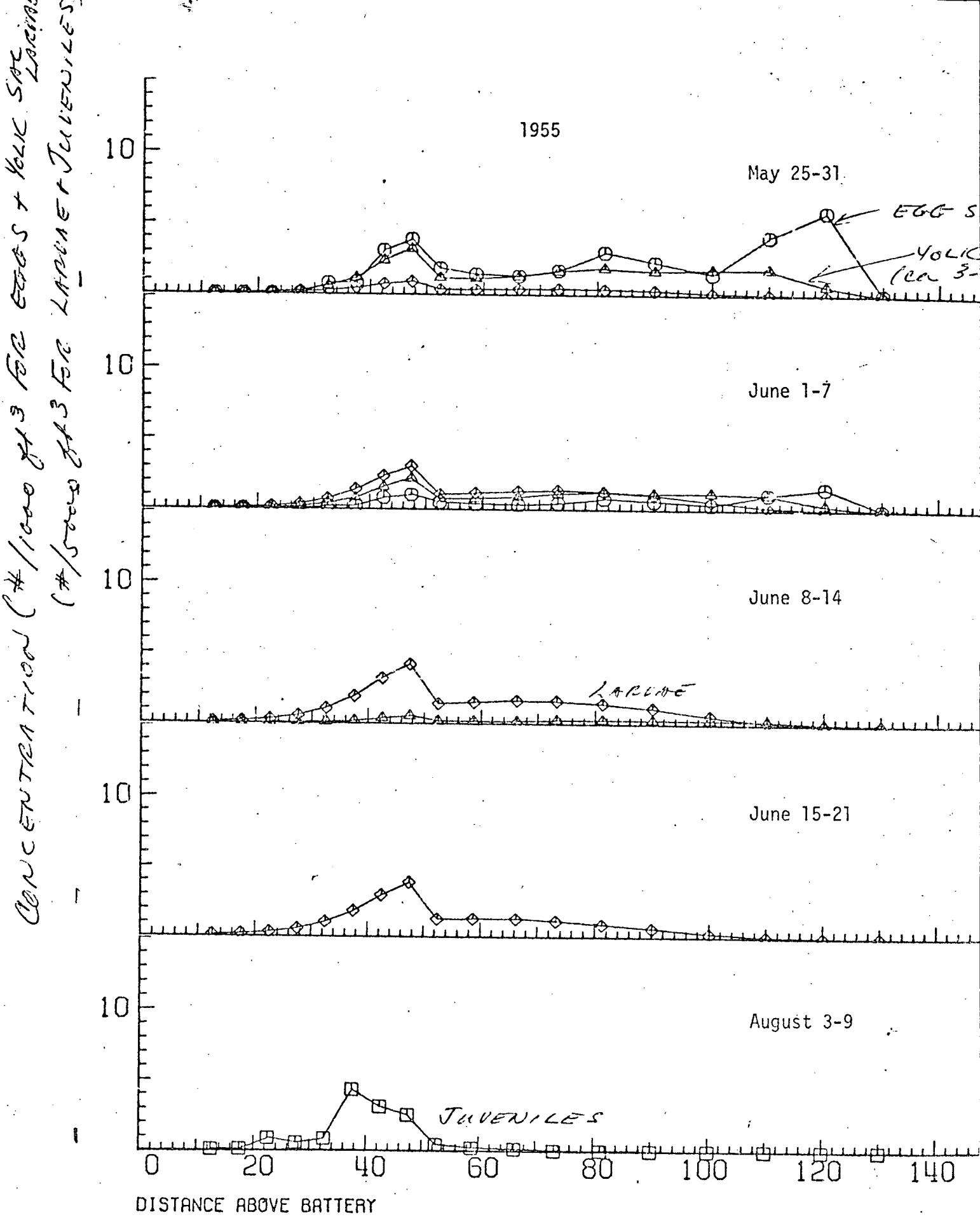
1969

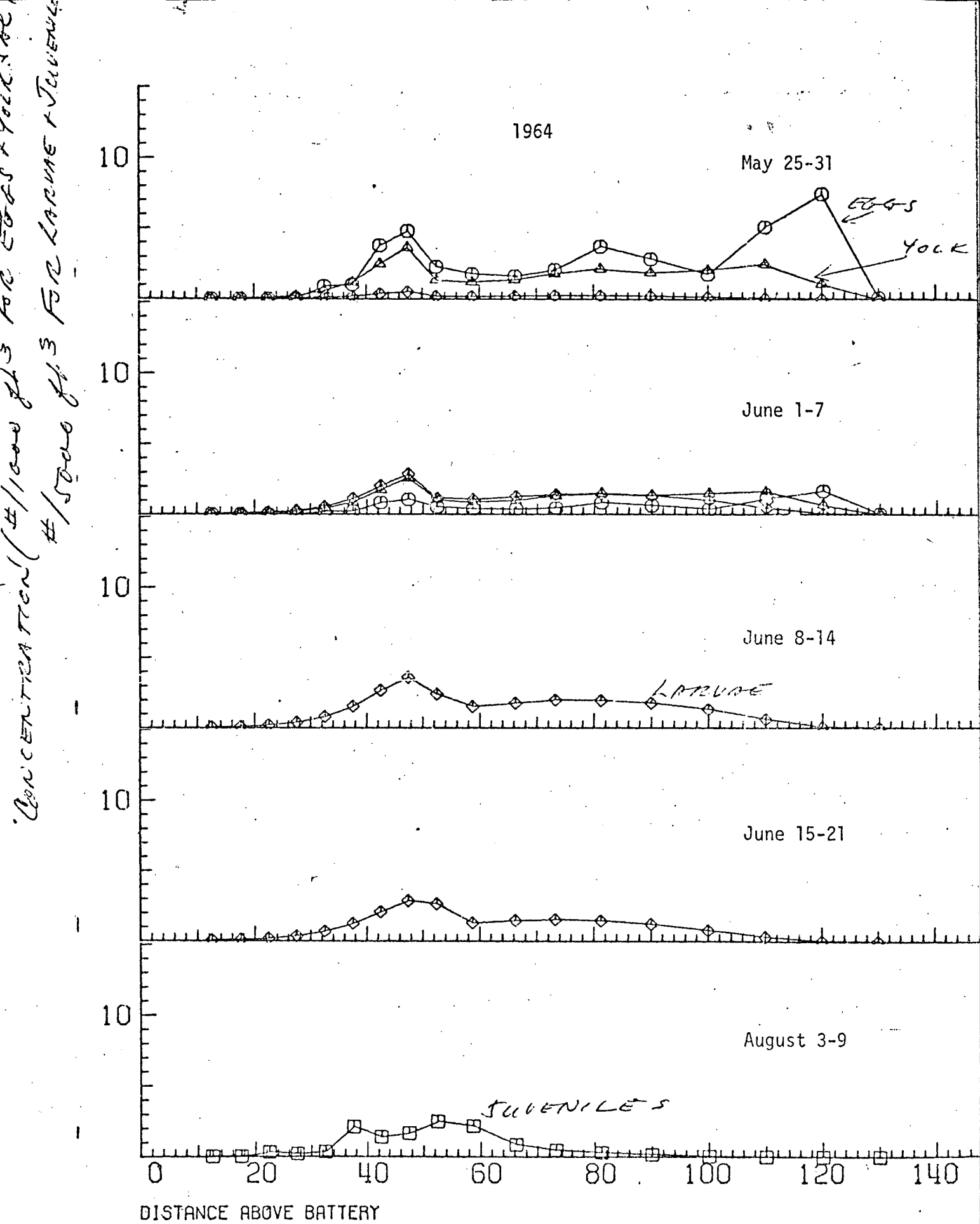

10
May 25-31
YOLK SAC
EGG
10
June 1-7
LARVAE
10
June 8-14
10
June 15-21
10
August 3-9
JUVENILE
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

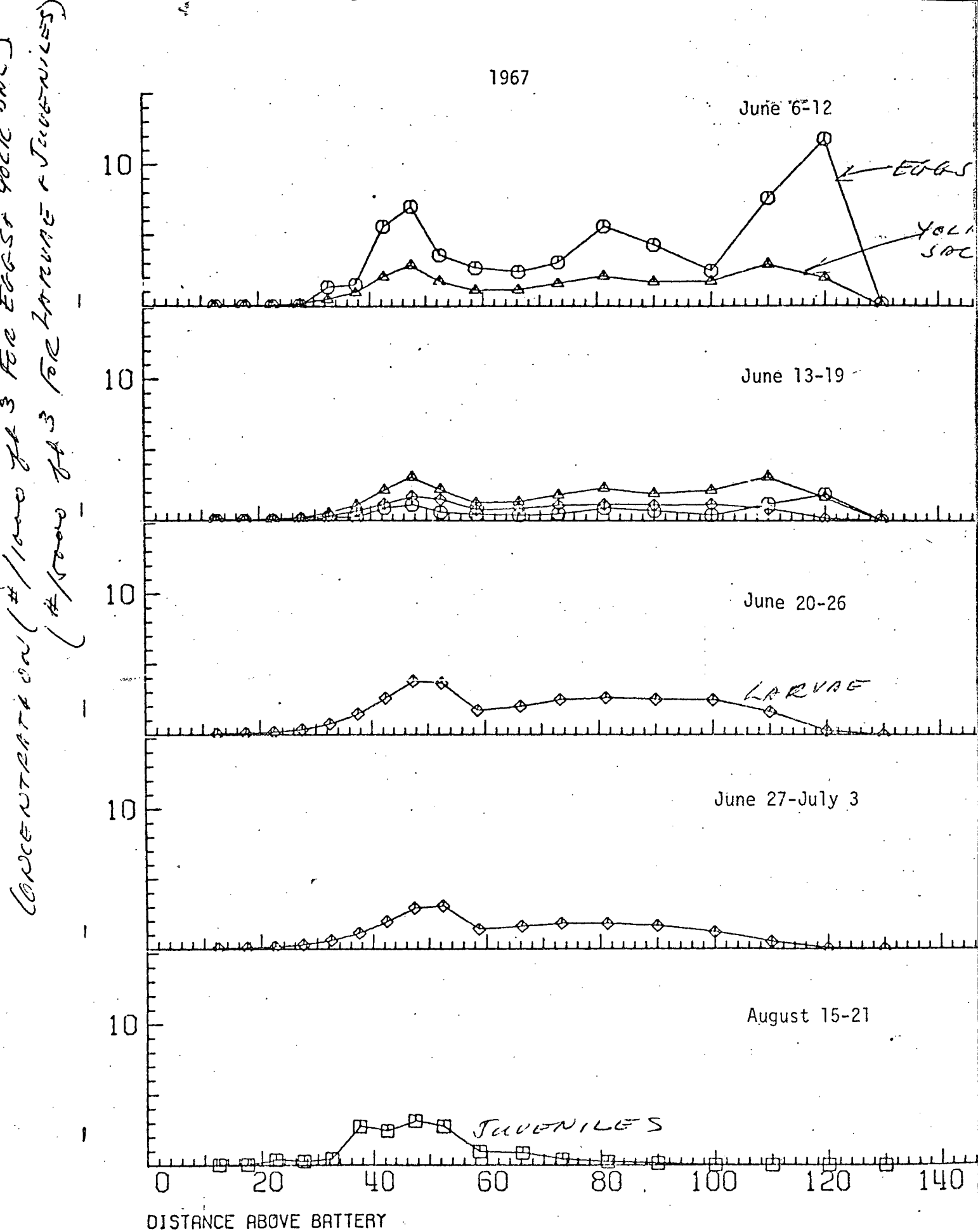
1970

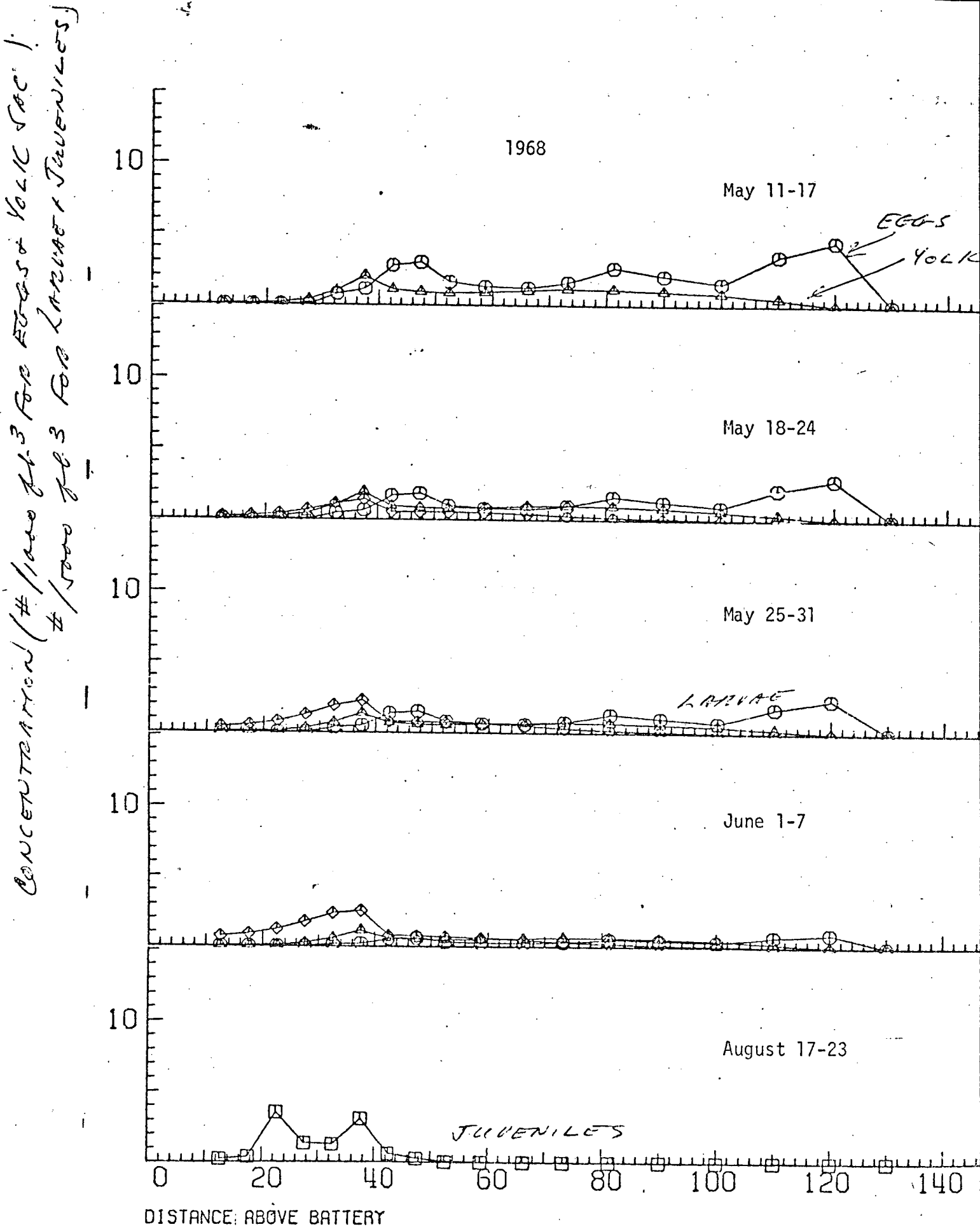

RELATIVE ABUNDANCE
10
May 25-31
YOLK SAC
EGG
June 1-7
LARVAE
June 8-14
June 15-21
August 9-15
JUVENILE
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

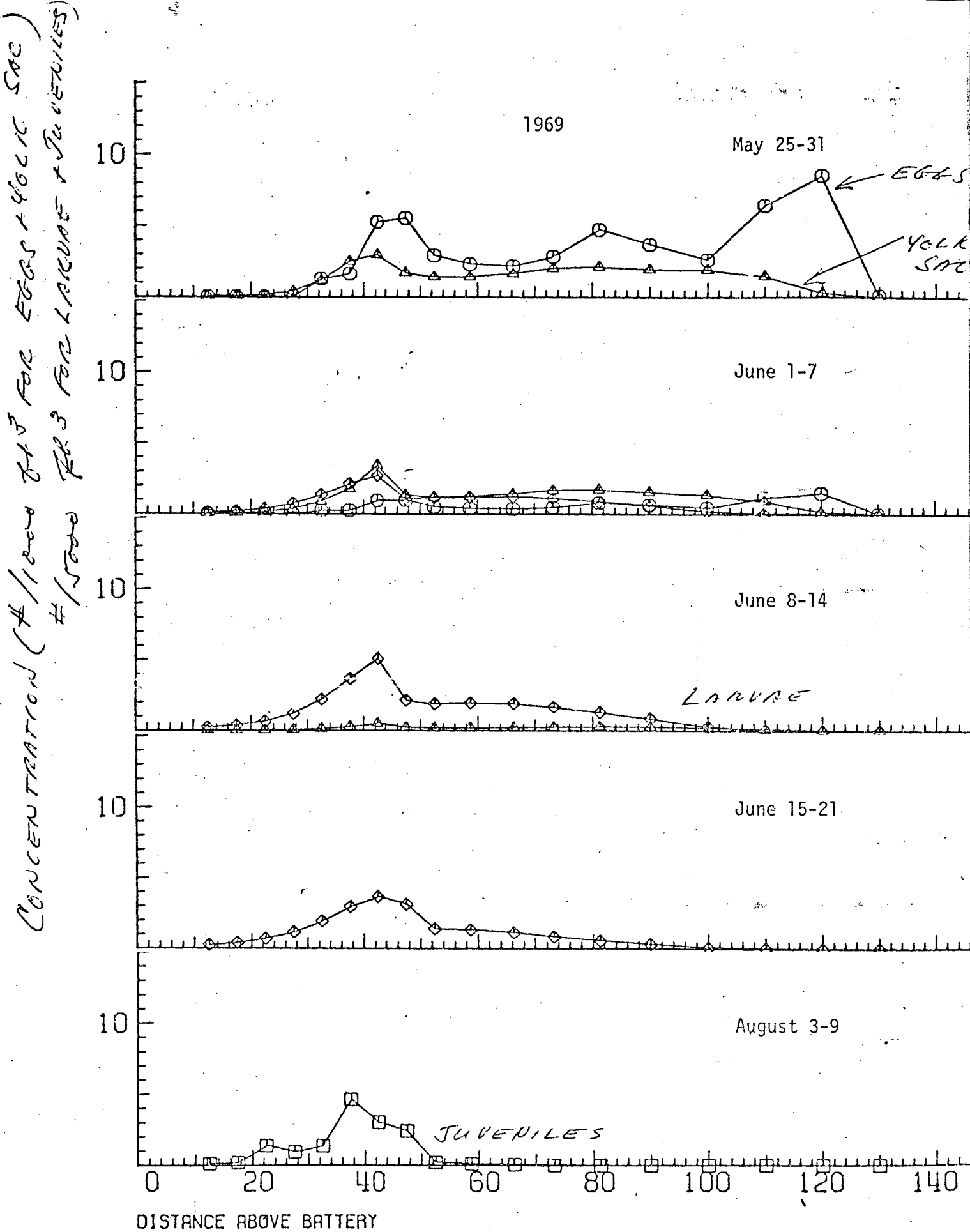

1949
May 25-31
EGGS
YOLK SA
June 1-7
June 8-14
LARVAE (CA 11 - 35 DAYS
June 15-21
August 3-9
JUVENILES (CA 36 - 64 DAYS OF AG
10
10
10
10
10
1
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY
CONCENTRATION (# / 1000 ft³ FOR EGGS & YOLK SAC LARVAE
(# / 1000 ft³ FOR LARVAE & JUVENILES)

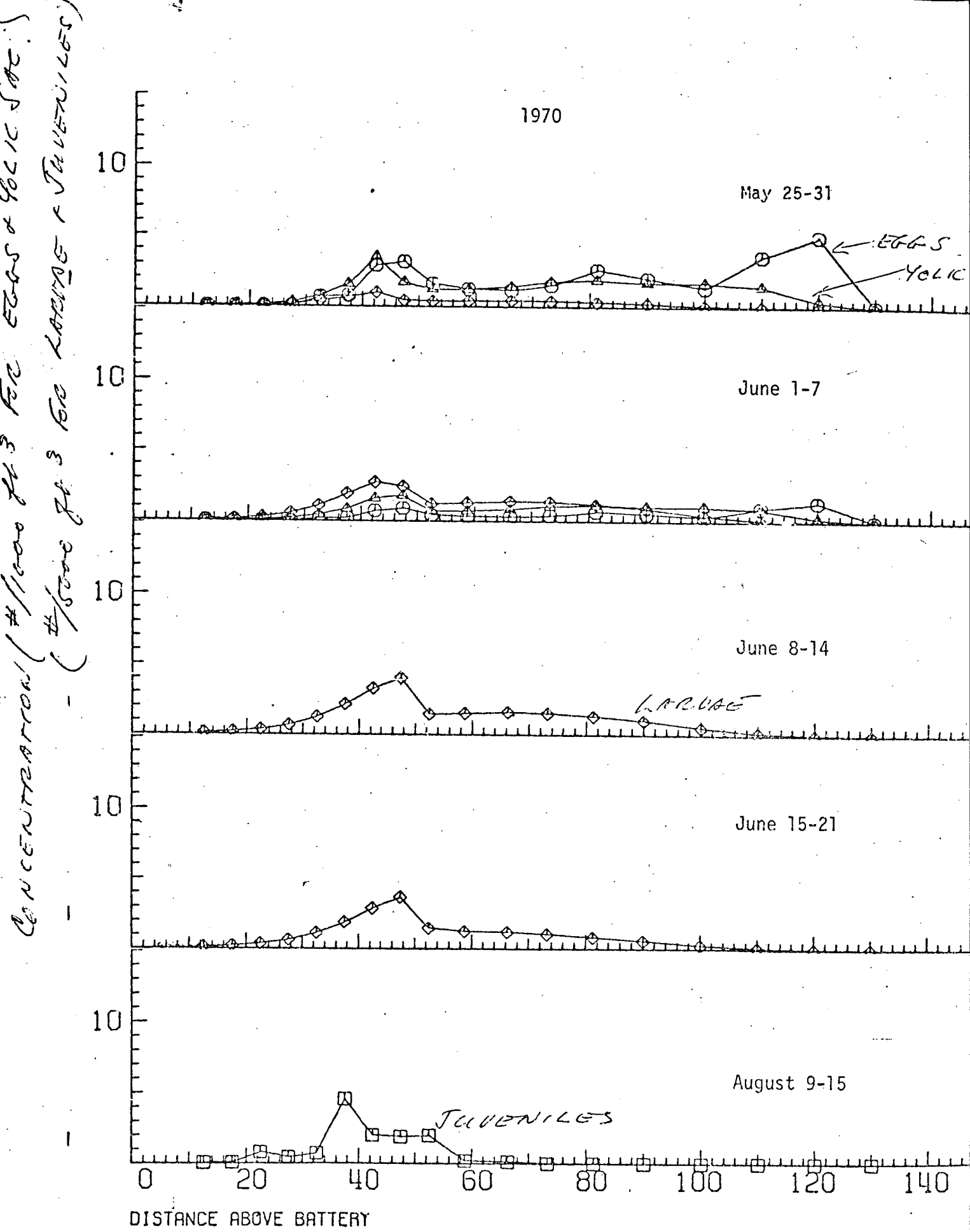

CONCENTRATION (#/1000 ft³ FOR EGGS + YOLK SAC LARVAE
(#/1000 ft³ FOR LARVAE + JUVENILES)
1955
May 25-31
EGG S
YOLK
June 1-7
June 8-14
LARVAE
June 15-21
August 3-9
JUVENILES
10
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY


CONCENTRATION (#/1000 ft³ FOR EGGS + YOLK SAC
#/1000 ft³ FOR LARVAE + JUVENILES
1964
May 25-31
EGGS
YOLK
June 1-7
June 8-14
LARVAE
June 15-21
August 3-9
JUVENILES
10
10
10
10
10
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

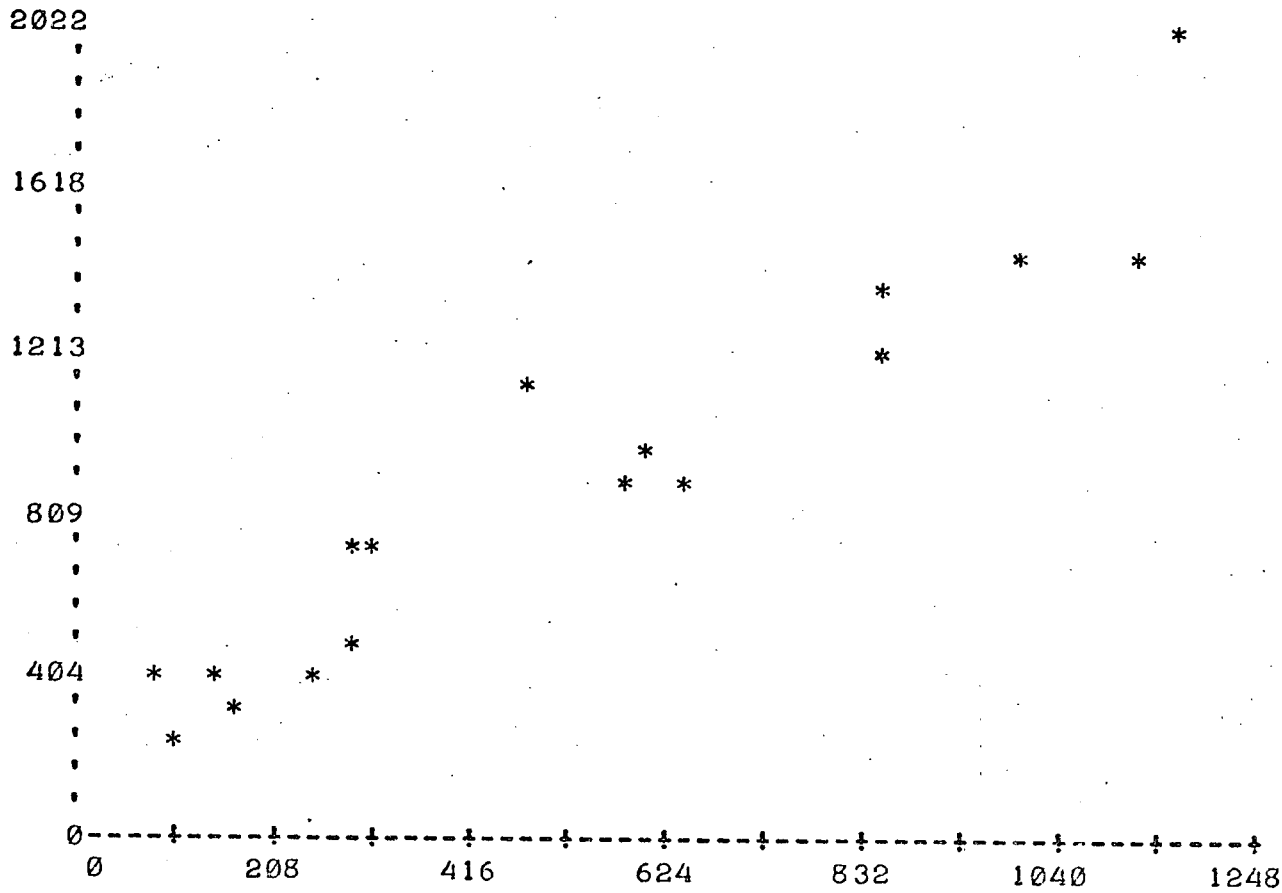

1967
June 6-12
EGGS
YOLK SAC
June 13-19
June 20-26
LARVAE
June 27-July 3
August 15-21
JUVENILES
10
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY
CONCENTRATION (#/1000 ft³ FOR EGGS & YOLK SAC)
(#/1000 ft³ FOR LARVAE & JUVENILES)


CONCENTRATION (#/1000 ft.3 FOR EGGS & YOLK SAC; #/1000 ft.3 FOR LARVAE & JUVENILES)
1968
May 11-17
EGGS
YOLK
May 18-24
May 25-31
LARVAE
June 1-7
August 17-23
JUVENILES
10
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY


CONCENTRATION (#/1000 m³ FOR EGGS & YOLK SAC)
#/500 m³ FOR LARVAE & JUVENILES)
1969
May 25-31
EGGS
YOLK SAC
June 1-7
June 8-14
LARVAE
June 15-21
August 3-9
JUVENILES
10
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY


CONCENTRATION (#/1000 ft³ for EGGS & YOLK SAC.)
(#/1000 ft³ for LARVAE & JUVENILES)
1970
May 25-31
EGGS
YOLK
June 1-7
June 8-14
LARVAE
June 15-21
August 9-15
JUVENILES
10
0
20
40
60
80
100
120
140
DISTANCE ABOVE BATTERY

DATE: 29 NOVEMBER 1972
SCALE
X MAX (114) =1: 10
Y MAX (2022) =1: 1



X IS HUDSON-3-POINT

Y IS ATLANTIC-5-YEARS-LATER

Y = 214.704+ 13.409X

0.98247 = COEFFICIENT OF SIMPLE CORRELATION

0.95656 = CORRELATION COEFFICIENT FOR REGRESSION
0.91501 = COEFFICIENT OF DETERMINATION = R SQUARE
0.08499 = PROPORTION OF VARIATION NOT EXPLAINED BY REGRESSION

NUMBER OF DATA SETS = 19
*

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a14
89

MR. KARMAN: Dr. Geyer had a question, and Dr. Goodyear has the testimony before him, and we could probably go along with that.

CHAIRMAN HEAD: Proceed.

THE WITNESS: That information on the recaptures is presented in the March 1 testimony entitled "Origin of Striped Bass Stock, Middle Atlantic Coast," and it is Table 11, and presented on page 8.

DR. GEYER: Thank you.

CHAIRMAN JENSCH: Mr. Briggs?

MR. BRIGGS: Dr. Goodyear, some months ago there was discussion of your model for calculating the effect of the plants on the removal of eggs and larvae from the Hudson River, and the continuous belt that was generated by the model.

It was my understanding that you indicated in reply to my questions that this continuous belt caused the concentration of the various species to be maintained in the section of the river where there were withdrawals and that this could result in as much as a factor of 2 difference between -- well, let's say it could result in a factor of 2 larger withdrawal from the river than one might expect without this continuous aspect.

You can correct me if that was the wrong understanding, and if it wasn't, is the model which you

1 are using at the present time subject to similar problems
2 with regard to the drawdown in the region of the river where
3 these eggs and larvae are being withdrawn?
4 THE WITNESS: It is subject to similar problems,
5 but the real limiting feature is related to the assumptions
6 concerning the location of the salt front because that
7 determines the highest concentration point. That has been
8 observed not only in the Hudson River, but in other
9 estuaries as well.
10 MR. BRIGGS: Excuse me. Is that the limiting
11 feature in the calculation or the limiting feature in what
12 would happen in the river or both?
13 THE WITNESS: Both. The computations are
14 sensitive to the assumptions concerning the location of the
15 salt front and the factor of 2 is related to the assumed
16 point where the circulation belt terminates.
17 That whole zone moves up and down with the fresh
18 water -- with changes in the fresh water flow.
19 MR. BRIGGS: I believe in your recent testimony
20 you indicated that this feature only existed in the region
21 where -- the region below the salt front. Is that right?
22 That was in your calculation? Was it in Lawler's
23 calculations that said that in both the fresh water region
24 and salt water region that this was the area --
25 THE WITNESS: The circulation belt produced

in my model terminates at the salt front and the Applicant's model, a similar effect is produced throughout the entire length of the river and the result is that the concentration does not build up at the salt front and below it.

MR. BRIGGS: In the years that you have used in your calculations, has the salt front been mostly above the Indian Point 2 Plant or below the Indian Point 2 Plant?

THE WITNESS: Mostly just above the plant. The median flow conditions tend to locate the salt front in the region between Peekskill and Cornwall.

MR. BRIGGS: So that in years of median flow, then, this effect would be present?

THE WITNESS: Yes.

MR. BRIGGS: In years of high flow it would not be present?

THE WITNESS: Yes.

MR. BRIGGS: We have two models that we have to look at. We have your model and we have the model that Dr. Lawler uses. You have indicated in your testimony some problems with the values that are used for the F factors and some problems with the compensation that is used in his model. Are there other problems that you have with the model that he uses or are those the principal ones?

THE WITNESS: Well, another major problem is that it does not simulate the observed behavior of the larvae

1 either in the Hudson or in the San Joaquin system in terms of
2 the relationship of the location of the population, the larval
3 population in respect to the location of the salt front.
4 There is no increased abundance generated in the
5 vicinity of the salt front. There is also an arbitrary
6 cutoff point for the downstream end of his river configura-
7 tion, his model.
8 The last comportment of his model has no transport
9 downstream and has a -- well, it is just an arbitrary selection
10 of an end point which is not exactly described by any kind of
11 realistic relationship that I know of.
12 Also, the use of migration factors which are
13 related to sampling data on the distribution of fish is
14 biased in an unknown way, really, because the distribution
15 of fish is also a function of other things like flow, and by
16 imposing migration factors that are not based on analytical
17 considerations, it is very easy to introduce errors into
18 the computations which result.
19 In the model you see, the result from the model
20 I have used, it uses a combination of two parameters to
21 compute the equivalent of the migration factors which are
22 referred to in Dr. Lawler's model, and in effect what they
23 are is a combination of percent of the surface area of the
24 segment that is being -- a percent of the surface area of
25 the segment which is considered to be a shallow area; in

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1 other words, less than 18 feet in depth.

2 This fraction is multiplied times a value

3 determined by age group of the fish which produces a number

4 which is then used as a migration effect and the movements

5 of the fish out of the comportment in either direction are

6 reduced by that product.

7 The theoretical basis for that assumed situation

8 is that the fish are visually associating themselves with

9 the substrate.

10 Now, light penetration is an important

11 component of their ability to associate themselves with

12 the substrate. If they can't see the surroundings, their

13 ability to orient and actually to be able to resist flow

14 if they so choose is considerably hampered.

15 Anyone who has been in an estuary anchored

16 and watched the fish schools go by with the tide can be

17 very easily convinced of that relationship.

18 The effect of this is that once fish start

19 becoming more associated with their substrate rather than

20 pelagic in nature, the more sedentary they become.

21 This is believed to be a function of the available

22 area, the probability of their contacting an area where they

23 can associate with a substrate which gives them visual

24 orientation.

25 I can't think right offhand now of any other

factors.

MR. BRIGGS: Have you examined your calculations and the calculations that Dr. has made where the conditions were similar, the parameters were similar, and assessed the degree of agreement between the resUlts obtained by the two methods? Have you attempted to do that?

THE WITNESS: I have done this. I have to go back and check. As I remember, it was a factor of about 2 difference.

MR. BRIGGS: Your numbers being a factor of 2 greater than his?

CHAIRMAN JENSCH: Did you answer the last question of Dr. Briggs; is that correct?

THE WITNESS: Yes.

MR. BRIGGS: Now, let's see, in connection with your testimony of April 23rd, the analysis of artificial propagation to replace Hudson River fishes killed by the power plant, you make an estimate of the hatchery -- the efficiency of hatchery production -- and get a value of .008. On page 4, I guess it is, and in Table 1, and then you make some numbers on the efficiency of natural reproduction in the Hudson River on page 8, and come to the conclusion on page 9 that the production in the Hudson River, the natural production at least the efficiency is greater than that than one would get in the nursery; or in a hatchery.

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There are three things about this that I would like to get clarified. One, on page 9, you indicate that, in the second full paragraph, that the value of 30 million fish is equivalent to the total survival of striped bass equivalent to Phase 1 survival in the hatchery operations. You at least assume this for the moment.

Yet when you compare the survival in the natural environment with the survival in the hatchery, you use the survival at the end of Phase 2 in the hatchery. Why did you not use the survival at the end of Phase 1?

THE WITNESS: The reason for proceeding with, or for including Phase 2 survival, was that it was what was considered to be an estimate of a stocking mortality which might be associated with the fish which are removed from a pond and then replaced in another body of water.

In effect, it is an estimate that is directed at trying to determine the number of fish which you would have to stock to get a desired result. In other words, if your assumption is that you have to replace 15 million fish, then the hatchery production to replace that should be sufficient to account for all of the mortality in handling and the experience of the fish you introduce into the aquatic system.

MR. BRIGGS: I believe you indicated in answer to other questions that these fish that you are talking about here, these 30 million fish, would be roughly seven-tenths of an inch in length, and you indicate here on page 11 that although there would be some additional mortality, it wouldn't be expected to reduce the number of fish very much. You say that this additional data is summarized in the final environmental statement, and I don't happen to have that here and haven't looked at it recently.

But in looking at Dr. Clrak's testimony at the end of stage one, in his three weeks after hatching, he estimates about 63 million fish of eight millimeters in length. Then at the end of stage three, which has an end point of one and a half inches, corresponding to an age of ten and a half weeks -- well, let me go back a bit.

The population at the end of six weeks he gets as being 16 million, and then at the end of ten and a half weeks he is down to about five million and the length of the fish is only one and a half inches.

Then he goes down from there to about 2.4 million at the end of 16 weeks in his analysis.

Now, this is in Clark's testimony of October 30, 1972, and it is on pages 15 through 24. This seems to indicate a substantial mortality, a reduction from, let's say, 16 million at the end of six weeks to 2.4 million at the end

of 16 weeks.

How did you conclude that the 28 million figure that we have here wouldn't change very much in the time from when the fish was seven-tenths of an inch long until it got to be three inches long?

THE WITNESS: There are two components of the answer to that question. The first, as indicated in the discussion, in my testimony, the standing crop estimates over that three-week period would not include fish which had already left the planktonic mode. They would be eliminated form the concentrations estimate.

The mortality, the additional mortality which could be expected to reduce that 28 million figure would be partially offset by the additional fish which are not measured in that estimate.

In connection with the Clark testimony, the numbers which he was utilizing in his testimony included in their formulation an error due to sampling techniques such that the part of the change in the standing crop that is observed is associated with mortality, and part of it is associated with decreased efficiency in sampling.

MR. BRIGGS: You conclude that the sampling of fish that are one and a half to three inches long is less efficient than the sampling of larvae that are three-quarters of an inch long?

1 THE WITNESS: Yes. It is considerably less
2 efficient, too, than the sampling of eggs, the yolk sac larvae.
3 Actually, the three-quarters of an inch figure would be most
4 applicable to the last day of the three-day period.
5 MR. BRIGGS: We have discussed the efficiency of
6 sampling at some length. Where do you get your information
7 that you used as a basis for judging the efficiency that the
8 sampling of juveniles is less than the sampling of larvae?
9 THE WITNESS: My experience comes from sampling,
10 but I think Dr. Raney was quite clear in his discussion of
11 the efficiency of trawling, which is the best method of
12 estimating the population of juveniles that we have.
13 He indicated that the very large percentage could
14 be lost from their own ability to escape the trawler.
15 I might also add that that component can be tested
16 by sampling -- for changing the efficiency of the trawls
17 through changes in procedure. One can quantify that effect.
18 MR. BRIGGS: In the calculations that you make of
19 the concentrations that are shown in these evidences, or in
20 this evidence, that was just introduced, do you come out
21 with number percent of eggs and numbers of juveniles that give
22 you the kind of efficiencies that you have estimated here
23 for the Hudson River?
24 THE WITNESS: To a certain degree. The estimates
25 that are presented in these graphs include sampling efficiency

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losses in the larval stages. Sampling efficiency loss is not a component of the juvenile estimate.

In other words, the method of producing the mortality rates which are a component of the model incorporated the sampling losses as a part of the mortality rate estimate. So, at the termination of the planktonic mode, the mortality rate estimate was actually assumed estimated from that point on.

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MR. BRIGGS: So you used a mortality factor of 1 in going from the larvae to the juveniles. Is that what you are saying?

THE WITNESS: It is essentially one. I think it was point 999. The effect of that is to result in an abundance of fish. It is numerically equivalent to the number of post-larvae that survive, and it includes the component of the mortality that is assumed to be associated with the sampling deficiency.

MR. BRIGGS: With regard to the eggs, are these viable eggs, or are these total eggs that are obtained from the females in your analysis of what goes on in the river?

THE WITNESS: In the river?

MR. BRIGGS: Yes.

THE WITNESS: They are total eggs. The breakdown which I have been -- as far as I have been able to decipher, nobody has been able to distinguish the number of ripe eggs, developing eggs, from the number of dead eggs. There is no real way for me to tell what the ratio is, or in fact if dead eggs have been the predominant portion of the eggs which have been collected.

In other bodies of water the dead eggs can be collected a great distance from the spawning sites and in the same physical distribution in the water mass.

MR. BRIGGS: Have you looked at the number of

1 females age 4 through 15, or whatever age you want to take,
2 that would be in the Mid-Atlantic fishery to support the
3 fishing industry that one has there, and then compare this with
4 the number of females that would be required to lay the number
5 of eggs that you estimate in the Hudson River, and found out
6 that there were ample females there, or there were 10 times
7 as many as you needed, or some large number like that?
8 In other words, have you tried to relate the spawning
9 activity in the Hudson with the number of fish in the fishery
10 on the assumption that the fish caught in the Mid-Atlantic
11 fishery come from the Hudson?
12 THE WITNESS: I have at one time done that. There
13 are a couple of points that I think should be brought out.
14 If you consider the number of females who actually spawn,
15 and the size of those females, then the four year old partici-
16 pation is quite low.
17 MR. BRIGGS: Yes.
18 THE WITNESS: The smallest fish which has been found
19 spent in three different bodies of water, and two I can think
20 of right offhand, have been no less than 21 inches in length.
21 The determinations which have been made on fecundity
22 include the younger fish, but this has not been verified to
23 my knowledge, from obtaining spent fish, which are known to
24 have spawned during the year.
25 The result is that the comparison really has to be made

on fish that are in their fifth year of life and spawn at the beginning of their sixth. So it is the comparison between the resulting five year old for the number of fish which are alive at the beginning of their sixth year and beyond that are comparable to the number that are spawning in the river.

This comparison is made, I believe it was a factor of about 3 or 4 difference with the Hudson being below. I would have to check as to the exact numbers.

MR. BRIGGS: How old is a ten pound fish?

THE WITNESS: The ten pound fish -- I have that someplace.

MR. BRIGGS: Just roughly, Dr. Goodyear. Is it five years old, six years old?

THE WITNESS: Six or seven, I think.

MR. BRIGGS: According to some numbers we have had a ten pound fish might lay 500,000 eggs.

THE WITNESS: Correct.

MR. BRIGGS: In order to get 330,000 eggs, then, one would have to have less than a thousand of these ten pound fish in the Hudson.

I am sorry. If I said 30,000, I mean 300,000. This was one of the numbers that you used for the number of eggs, I believe.

THE WITNESS: Right. During one of the years of the sampling, the gear was found to be inefficient in collecting

eggs. One of the things that causes a reduction in efficiency is the ability of the eggs to withstand the collecting procedure.

They are not very sturdy, and the effect of this is if you are not careful in your sampling procedure you can destroy the eggs before you can count them, and the mesh diameters that they were using for the 1967 egg sampling apparently had that effect.

MR. BRIGGS: We should ignore those numbers then, and that percentage that was associated with them?

THE WITNESS: As far as being absolute estimates, yes.

MR. BRIGGS: Even as far as being relative estimates. It was that number that was used to get the 7.8 percent efficiency of utilization.

THE WITNESS: I don't think you can exclude it because the sampling efficiency of the larvae is also quite low. The ratio of the sampling determines the validity of the number, and I don't -- I myself can't exclude the possibility that the sampling efficiency of the two groups would be the same.

I don't propose that that is the actual production efficiency of the estuary.

MR. BRIGGS: Dr. Goodyear, were there data for 1966 and 1968 for larvae in the estuary?

THE WITNESS: Yes.

MR. BRIGGS: Why did you choose to use the 1967 data rather than the average data for 1966, 1967 and 1968?

THE WITNESS: The 1968 data could not be computed as the standing crop in the same manner, because the data for distribution for each compartment that they were sampling in 1966 and 1967 was not known in 1968.

The 1966 data on larval abundance is biased in the opposite direction to that which is present in the 1967 data, because they changed to a less efficient net when they should have changed to a more efficient net to allow for collections of larger individuals.

So 1966 is more heavily negatively biased in terms of the larval estimates than the 1967 is. I might also point out that the procedure for collecting the larvae in 1966 included -- the procedure was to anchor the net and use the flow of the river to collect the -- to use the flow of the river as the mechanism by which water is strained.

In 1967, the nets were towed at a higher velocity, which also increases the efficiency.

MR. BRIGGS: Thank you.

CHAIRMAN JENSCH: Dr. Geyer has some questions.

DR. GEYER: Dr. Goodyear, looking at table 1 on your testimony of April 23, do the numbers under the column headed "Phase 1" represent the survivals in the process of

1 moving them from the hatching portion of the hatchery into
2 rearing ponds and keeping them there for from 30 to 60 days?
3 THE WITNESS: The conclusion of the transport
4 portion of the loss is not considered in the Phase I survival.
5 The transport loss, is presented in the pre-stocking loss.
6 DR. GEYER: Presumably, then, the Phase 2 involved
7 moving from the rearing ponds into the wild environment of
8 the river, is that correct?
9 THE WITNESS: It was used for that. Actually, it
10 represented loading the fish from one pond to another. It is
11 not a very good estimate of the probability of survival of the
12 fish raised in ponds that are stocked into a river or an estuary.
13 DR. GEYER: Yes. I understand from yesterday's
14 testimony that there really isn't any data that can be used
15 as a basis for comparing the survival of a hatchery-reared
16 fingerling with a naturally reared fingerling of the same size.
17 Is that correct?
18 THE WITNESS: That is correct.
19 DR. GEYER: Would you expect the hatchery-reared
20 fingerling to have a better or poorer chance of survival than
21 the natural fingerling?
22 THE WITNESS: It is really hard to say. The hatchery-
23 reared fingerling could be reared to a larger size in the same
24 time period that would apply to the field, so that when you
25 introduce the larger fingerling into the population in the field,

it may be able to survive in a -- may be able to survive better than the naturally reared ones.

However, there would be some loss prior to that. The fish has got to adapt its feeding habits, its motivations for escaping predators. He has to do everything that the natural fish has already done.

DR. GEYER: He has to learn to do everything the natural fish already does.

THE WITNESS: Yes. The problem I have with that is that I don't know if there are any implementing procedures in young fish. I don't know many things about the behavior of the young fish which would allow that particular facet to be evaluated.

DR. GEYER: Thank you. I would like to ask another question that probably should be addressed to Dr. Stevens, but if the Applicant wants to supply information from him, it would be quite satisfactory.

But I will ask you this question, too. Conceivably, if striped bass could be stocked for either one of two purposes, one to supply bass that would be harvested by fishermen, and that would be the evidence of the Inidan Point, and the other purpose is to develop a self-sustaining population in an area where one did not previously exist.

Of the striped bass stocking operations, how many fall under one or the other of these purposes?

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THE WITNESS: Well, almost all of the stocking is done in reservoirs. It is intended to produce a population where it had not previously existed. In most cases where that has been attempted, the adult fish have no place to spawn. As a result of that condition, they have to be maintained by hatchery production, so that they don't become self-sustaining populations.

DR. GEYER: Has that been done with striped bass?

THE WITNESS: Yes. That is the common procedure for most of the lakes that have striped bass populations. There are only a few that have naturally reproducing populations. Two of those, I believe, were established by natural stock that were land locked.

Actually, the observation that they were reproducing themselves is what started the second efforts to produce hatchery stock of striped bass. The only population that I know of right now which has been introduced to an estuary with the result of a sizable sustaining population is in the San Joaquin system.

I think the rate of flow of the population in the first few years after the fish were released is a good indication of the high survival, because there were very few fish that were actually stocked, and those big fish that were caught in the New Jersey area and transported across the Continent.

I don't know of any population that is being sustained by hatchery production for use of a fishery other than the

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1 put-and-take situations in reservoirs, and in those cases
2 the mortality rates are very much lower than fish mortality
3 rates in estuaries.
4 The reason for that is that anglers are not very
5 efficient at catching striped bass.
6 DR. GEYER: I think that answers the question. Thank
7 you.
8 CHAIRMAN JENSCH: Are there any further questions
9 of Dr. Goodyear?
10 MR. MACBETH: I have none.
11 CHAIRMAN JENSCH: Do you have, Applicant?
12 MR. TROSTEN: May I have a two-minute recess?
13 CHAIRMAN JENSCH: We won't formally recess.
14 (Pause)
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CHAIRMAN JENSCH: Do you have something on the transcript you want to correct?

All right, proceed.

RECROSS-EXAMINATION

BY MR. TROSTEN:

Q You have -- the X axis is labeled Hudson 3 point. Do you see that?

A Yes.

Q What does that mean?

A It is a three point average of the landings of striped bass. That is, in the Hudson River, commercial landings, three years, the average of three years of landings in the Hudson.

Q Is it a 3-point running average?

A Yes, except that there are broken places where sampling data was not available. It essentially is a 3-point running average.

Q What did you do with the points where the data were broken?

A They were not included. If the data were broken two data points were lost. One moment. Two data points would be lost. This is the reason why there are fewer data points in this regression than there were in the original, because in the original regression, I interpolated between the missing data points.

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So the effect was that some of the points had to be two point averages rather than three point.

MR. TROSTEN: Thank you.

CHAIRMAN JENSCH: If there are no further questions of Dr. Goodyear, thank you, Dr. Goodyear. You are excused.

(Witness Excused.)

CHAIRMAN JENSCH: Does that complete the presentation by the Staff?

MR. KARMAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Is the Applicant ready to proceed?

MR. TROSTEN: I have one question that I wish to put to Mr. Woodbury, Mr. Chairman.

CHAIRMAN JENSCH: Do you want to take a recess?

MR. TROSTEN: It is not necessary, We can proceed right now.

MR. KARMAN: Might I interrupt a moment? As of yesterday's hearing, Mr. Chairman, we indicated that Mr. Neal would return in response to an inquiry by Mr. Briggs. Now any time that the Board is desirous of having that information Mr. Neal is present.

CHAIRMAN JENSCH: Very well. If he desires to submit it in writing, he may. I think it was a question of what parameters he would compute on pressurized fuel. If he desires to do that in writing, he may.

MR. KARMAN: Whichever way the Board prefers.

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Mr. Neal is here.

MR. TROSTEN: Mr. Chairman, would it be possible for us to conclude that aspect of it today, if we could? Somehow when these things end up in writing, we end up with more time being taken.

CHAIRMAN JENSCH: Mr. Neal is here. We will be glad to have him testify orally. Would you proceed?

Whereupon,

HARRY G. WOODBURY

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

REDIRECT EXAMINATION

xxx

BY MR. TROSTEN:

Q Are you familiar with Mr. Clark's testimony of April 23, 1973, and particularly the second paragraph on page 2?

CHAIRMAN JENSCH: Excuse us for a moment so we all get that. Proceed, please.

THE WITNESS: Yes, sir, I am.

BY MR. TROSTEN:

Q Have Consolidated Edison study contractors been able to capture female striped bass during the spawning season?

A Yes, sir. Mr. Bidco was an employee of NYU last year, and went with Dr. Lauer to South Carolina, the Moncks Corner Hatchery in the spring, where in the course of about

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three weeks he and Dr. Lauer conducted experiments with eggs and larvae, and then for a week or ten days at Edenton conducted further experiments from the fry.

These experiments were the basis for the -- let me see that -- were the basis for Dr. Lauer's testimony which appeared on figure 17 and was introduced in the February hearing. This is dated, I believe, February 5th. You will recall that.

There he showed the temperature tolerance of eggs and larvae in different stages of their life. Upon returning to the Hudson River, Dr. Lauer with help from Mr. Bidco sought to verify the behavior of the Hudson River stock, that is, sought to relate the behavior of the Hudson River stock with respect to temperature and time to the South Carolina and North Carolina stock, and so attempted to get a spawner from the Hudson River.

Never having done that, they sought the assistance of the Hudson River Fishermen's Association, and were put in contact with a Mr. Charles White, who operates a bait shop at Verplanck, and who has a scientific collector's permit from the State of New York, and who was reported to have collected brood stock for Mr. Dick Pound of ~~Stripless~~ Striper Unlimited in Attleborough in preceding years.

Mr. White and Mr. ~~Bidco~~ Bibko then attempted late in the season to collect by a seine a ripe female in the vicinity of the Veterans Hospital close to Indian Point, and were unable to do so, and Mr. White expressed the view that they had started too

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Al 7 Reba 5 late in the season and that the fish had already gone upstream.

2 Thereafter, utilizing commercial fishermen's gill nets,

3 they obtained a ripe female one evening. The female had been in

4 the gill net for quite sometime and was seriously distressed

5 at the time she was located. She was taken over to Verplanck and

6 nursed through the night and succumbed in the early morning.

7 Her ovaries were removed, the eggs were fertilized, and

8 2,000 fry were raised from those eggs and those fry were used

9 to supply the data which appears on figure 17 in the form of

10 the open circles showing the temperature, time survival relation-

11 ship of Hudson River eggs and fry to the Moncks Corner eggs and

12 fry.

13 MR. TROSTEN: That concludes the redirect testimony,

14 Mr. Chairman.

15 CHAIRMAN JENSCH: Are there any questions?

16 RECROSS-EXAMINATION

17 BY MR. MACBETH:

xxxx 18 Q Mr. Woodbury, would you tell me what the present

19 scheduled operation of Indian Point Unit 1 for the months of

20 May, June and July of this year is?

21 A Would you define for me what you mean by operation,

22 please?

23 Q I really mean two things. The power levels at which

24 the plant is scheduled to operate, and also the plans for

25 pumping, and obviously the plant at 100 percent of power will

be pumping constantly, but it is now planned to be at a hundred percent of power.

You may have plans to pump when the plant is down. So I wanted both the power levels and the pumping plants.

A Yes. Indian Point 1 has been down for maintenance as has been reported earlier in this hearing. It is presently scheduled to go back on the line on the 1st of August.

Inasmuch as this interferes with part of our entrainment testing program for 1973, the circulators will be started for the purpose of testing mechanical and pressure effects, and the relationship to larvae entrained or to larvae in the river, starting on the 1st of May.

It will be run at such times as they are required to be run, only for the purpose of scientific collection. Because of the difficulty in getting Indian Point 1 back on the line, the entrainment program for this year has been modified to include Bowline, Lovett, Roseton and ~~Danskammera~~ Danskammer as well as Indian Point 2.

MR. MACBETH: Thank you. I have no further questions.

MR. KARMAN: No questions.

CHAIRMAN JENSCH: Thank you, Mr. Woodbury.

(Witness Excused.)

CHAIRMAN JENSCH: At this time let's recess to reconvene in this room at 11:30.

(Recess)

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CHAIRMAN JENSCH: Please come to order.

MR. KARMAN: Can we switch speakers at the moment, Mr. Chairman? I have Mr. Kniel present.

CHAIRMAN JENSCH: Proceed, please.

Whereupon,

KARL KNIEL

resumed the stand as a witness on behalf of the Regulatory Staff and, having been previously duly sworn, was examined and testified further as follows:

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EXAMINATION

CHAIRMAN JENSCH: Please proceed, Mr. Kniel.

WITNESS KNIEL: I had some discussion with Mr. Briggs and Chairman Jensch at yesterday's session concerning the technical specifications, and I digested that discussion down to a question as follows:

Should additional technical specifications describing design features of the fuel be required in Section 5.3 in light of our fuel densification review?

I considered that question, and my answer is as follows:

The present requirement in Section 5.3 of the Indian Point Unit 2 technical specifications with respect to fuel design is that the reload fuel will be similar in design to the initial core.

The other specifications provided address the

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fuel materials, the number of fuel rods, the number of assemblies, and limits on enrichment. The fuel densification report is referenced in this Section 5.3, thus incorporating fuel design and design basis by reference.

The safety significance of fuel design relates principally to its design performance in the reactor core. Design performance of other fuel as reflected in reactor core performance requirements are defined in technical specifications Secton 2.1, safety, 2.3, limiting safety system settings, and 3.10, limiting conditions for operation, control rod and power distribution limits.

Therefore, any change in fuel design significant to safety would require a change in one or more of the above specifications and in accordance with Part 50.59, any such change proposed would have to be reviewed and approved by the Regulatory Staff.

Reload fuel of similar design and consequently of similar performance which did not require a change in any of the core performance specifications in the sections listed above would not require staff review by regulation. However, a review of past practice indicates that details of the reload fuel have generally been provided to the Staff by licensees regardless of the reload fuel design conformity with the FSAR.

The Staff concludes that since the safety

1 significance of the fuel design and performance are extensively
2 addressed in Sections 2.1, 2.3, and 3.10 of the technical
3 specifications, no additional detailed description of the
4 fuel design is required in Section 5.3 of the technical
5 specifications.
6 The Staff believes that this conclusion is in
7 accordance with Part 50.35, paragraph C-4 of the Commission's
8 regulations addressing technical specifications, which
9 requires design features to be included in the technical
10 specifications only if the modification would have a
11 significant effect on safety and are not covered under
12 safety limits and limiting safety system, limiting
13 conditions for operation or surveillance requirements
14 sections of the technical specifications.
15 CHAIRMAN JENSCH: I think the question, Mr. Kniel,
16 wasn't perhaps clear to you. We said supposing the Board
17 decides differently from the Staff. What would be the
18 pressure on the pellet densification in the new fuel? If
19 you haven't had a chance to consider that, you might like
20 to do that and come back. I think we will be here this
21 afternoon.
22 We appreciate the statement, but there has been
23 nothing added by it that is new. We are happy to have you
24 here, but I think the question was, suppose the Board thinks
25 differently about it, what would be the difference on the

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1 pressure in the fuel densification? Could you address
2 yourself to those two items?
3 WITNESS KNIEL: I don't think we have a sugges-
4 tion -- we don't suggest that be included.
5 CHAIRMAN JENSCH: We understand your view, but
6 the Board might think differently.
7 WITNESS KNIEL: I understand.
8 CHAIRMAN JENSCH: And we would be glad to
9 consider your suggestion, or otherwise we will proceed
10 without the benefit of the Staff's suggestion in that regard.
11 We will be here, I think, this afternoon, in case you would
12 like to come back, and we will interrupt whatever we are
13 doing to accommodate your presence.
14 Is there anything further from the gentleman?
15 If not, thank you, Mr. Kniel.
16 (Witness Kniel excused.)
17 CHAIRMAN JENSCH: Are we ready to proceed with
18 the Applicant's witness?
19 MR. TROSTEN: Yes, we certainly are.
20 Mr. Chairman, at this time I would like to recall
21 Dr. James C. McFadden to the stand.
22 CHAIRMAN JENSCH: Dr. McFadden, having previously
23 been sworn, need not be sworn again.
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1 Whereupon,

2 JAMES C. MC FADDEN

3 resumed the stand as a witness on behalf of the Applicant and,

4 having been previously duly sworn, was examined and testified

5 further as follows:

6 MR. TROSTEN: I am recalling him with respect

7 to the query on transcript page 1099, and I have several

8 questions to put to Dr. McFadden in that respect.

9 CHAIRMAN JENSCH: Could I have the page again?

10 MR. TROSTEN: 11099.

11 CHAIRMAN JENSCH: Thank you.

xxxxx 12 DIRECT EXAMINATION (Continued)

13 BY MR. TROSTEN:

14 Q What are the implications of past failures in

15 fish stocking for the probability of success as you would

16 judge it for stocking of striped bass in Hudson River?

17 A I would like to give a general answer to that

18 question, because I think there is a clear relevance to

19 the question of possible success of striped bass stocking,

20 a clear relevance from the past history of experience in

21 the use of fish stocking as a management tool.

22 The history has been as follows: In the early

23 years when stocking was used as a fish management tool, it

24 was used in a fairly ad hoc and insensitive way, and the

25 general theory seemed to be that if you put more fish in

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1 through any device, fish at any stage of the life history,
2 that it is bound to be good.
3 So there wasn't very much attention paid to the
4 quality of the fish which were stocked, their vigor, their
5 genetic background, and there wasn't very much attention
6 paid to the ecological characteristics of the system into
7 which they were introduced, and as a result, the early
8 history of stocking as a fish management tool was one of
9 a few successes and widespread failures.
10 Over the years, hatchery techniques have been
11 vastly improved, and ecological and managerial insight into
12 populations has been very greatly increased, so that the
13 current trend against the background of early failures, I
14 think, is one of an increasing number of successes in the
15 use, in the intelligent use, of fish stocking as a manage-
16 ment tool.
17 Q What are some of the factors which determine
18 success or failure in fish stocking programs?
19 A One of the important factors determining success
20 or failure in fish stocking as a management tool has been
21 clarification of the management objective, and in many
22 cases, as I indicated in my previous response, stocking
23 was used just on the general assumption that anything you do
24 has got to be good. It is important to clarify your
25 objective, that is, whether you are stocking in order to

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1 introduce a fish where it doesn't previously exist, whether
2 you are stocking in order to maintain a population which,
3 say, can't reproduce in a particular environment, but a
4 species for which, once young are present, the rest of the
5 life history can be accommodated. For example, there are
6 many rivers in which certain species can't spawn because
7 the bottom substrate isn't of the right type, mud instead
8 of gravels. It is not uncommon in those situations, if
9 you introduce a young fish, that all the requirements for
10 the subsequent stages of the life history are present, and
11 you then have survival, and we think of that, we call that
12 maintenance stocking. As long as the stocking continues,
13 using young fish, you can maintain a healthy population.
14 The third stocking strategy is to use stocking
15 to supplement natural production of fish, and that is the
16 idea that has been proposed in relation to the Hudson River
17 for striped bass. Here you have natural reproduction,
18 and natural survival, but there may be some reason to feel
19 that either a removal from the population can be replaced,
20 or that by supplemental stocking you might raise the general
21 level of productivity of the population.
22 If you don't have those different strategies
23 clearly defined and know which of them you are pursuing,
24 the likelihood of success is very low.
25 A second factor that has an important bearing on

the success of stocking, and this has been proven repeatedly, is the quality of the fish that are stocked. Are they physiologically vigorous, have they been exposed to debilitating disease in the hatchery? Are they of the right genetic background? That is, will they behave appropriately into the environment into which they are released? Will they make the right migrations? Will they seek out the right kinds of habitat where they fill find food and shelter?

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It is possible to have a fish of the right species, but of the wrong genetic makeup, and he doesn't behave right where you stock him, and in those cases the stocking isn't going to ~~proceed~~ succeed.

A third important consideration in the success of stocking is the ecological situation that you are dealing with.

We have talked a lot in these hearings about the operation of the compensatory processes in fish populations. If you were to introduce a large number of stocked fish -- in other words, increase the density of the fish population -- at the stage immediately preceding the operation of compensatory processes, natural forces would simply tend to level that number back down and the stocking wouldn't have very much effect.

If you can cite a stage in the life history of the fish which is beyond the major influence of compensatory processes and is a stage at which there are no serious limitations in environmental requisites and stock fish there, the chances of success are very much greater.

In the case of a fish like striped bass, to me this would mean stocking near to the end of the first year of life, because the various studies of striped bass that have been cited have, I think, quite uniformly tended to support the notion that compensation leading to the adjustment of

1 density of a particular year class takes place mainly during
2 the early parts of the first year.
3 Okay. If you can introduce young striped bass
4 beyond that critical stage and there are ample environmental
5 requisites, food and space, and, of course, in the not too
6 distant future these fish will be going to sea or at least
7 toward the more open estuarine environment where there is a
8 large food source and a great amount of space, then the
9 chances of succeeding are much greater.
10 Other examples of taking into account ecological
11 factors would be avoiding introducing fish where they would
12 be subjected to severe temperature shock, which they
13 wouldn't survive, or avoid introducing them into an environ-
14 ment where they would face massive predatory pressures --
15 things of those kinds.
16 It is very important to plan carefully what life
17 history stage you are going to use in stocking, and to match
18 the hatchery output with the processes that are going on in
19 the natural environment.
20 Q Dr. McFadden --
21 A Mr. Woodbury reminds me that in an environment in
22 which compensatory processes aren't operative, which is the
23 Staff's description of the Hudson River for striped bass,
24 though not mine, introducing hatchery fish at any time,
25 the success of the stocking would be determined strictly by

1 density-indpendent considerations and the fact that there
2 were large numbers of fish already present, and all the
3 general implications relating to the numbers of fish pre-
4 existing plus the number stocked.
5 It wouldn't have any influence, that it -- it
6 wouldn't diminish the survival of the stocked fish. Whatever
7 percentage surrival was prevailing in the system would be
8 imposed on the stocked fish themselves, even if you stocked
9 at a pre -- no matter how early in the life history stage you
10 stocked the fish. There wouldn't be any density-induced
11 increase in their survival.
12 Q Would it be possible, Dr. McFadden, to measure
13 the survival of striped bass stock in the Hudson River?
14 A I think there is a fairly simple and direct way
15 to measure the survival of striped bass stock in the Hudson
16 River, and I think it is important to distinguish here
17 measurements of impact on the system whether they happen to
18 be removals of fish, say, through the operation of a power
19 plant, or supplemental measures such as stocking.
20 It is important to distinguish the effects caused by
21 a point source such as stocking fish or operating a power
22 plant from changes in numbers due to what I call very
23 diffuse factors in the environment such as the predatory
24 influence of other species or a general change in water
25 chemistry due to pollution abatement, and an attendant

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1 possible change in the food supply of the fish.

2 It is a lot easier to measure the effect of

3 these point impacts than it is to measure the effects of very

4 diffuse causes of changes in numbers of fish.

5 In the case of the stocking alternative, the

6 simple and time tested measure of success or failure would

7 be to mark the fish which were introduced in such a way that

8 in our subsequent trawling and seining studies, which are

9 part of the Indian Point ecological study, we would be able

10 to determine whether we were recapturing significant

11 numbers of marked fish. That would be fish of hatchery

12 origin.

13 This is exactly the method that is employed in

14 both fresh water and marine situations elsewhere in North

15 America.

16 The second fix under present sampling programs

17 that we would get on the survival of these marked hatchery

18 fish would be -- let me backtrack just a moment.

19 Assuming that hatchery fish were stocked in early

20 fall, say September, we would get our first fix on their

21 survival through the experimental trawling on beach seining

22 in October, November and March, subsequent to the stocking.

23 So we would immediately expect to observe the

24 presence or absence of these marked hatchery fish. That is,

25 during the first critical several months when they were at

1 liberty in the population.

2 The second fix under present sampling commitments

3 would be about two years later when these fish, fish of this

4 age, would be expected to appear in significant numbers in

5 sport and commercial catches.

6 In addition, it would be possible through

7 sampling, not presently planned, to recapture some of these

8 fish during the interim period in the estuary, that is, when

9 they were between the age of one and three years, and offhand

10 I would expect that gill netting would be the most feasible

11 kind of gear to control.

12 If those sampling programs didn't turn up some

13 appreciable numbers of the marked fish, I think it would be a

14 clear indication that the stocking wasn't successful.

15 On the other hand, if you turn up marked fish, then

16 clearly the stock fish are surviving.

17 Q Have you completed your description of how you

18 would measure the survival of the striped bass?

19 A Yes, unlessthere are specific questions.

20 Q Can you cite any cases where early failures in

21 stocking of fish were followed by later successes in the same

22 waters?

23 A Yes. There are some striking examples. I guess

24 the most current one being the success of stocking of various

25 salmonid species, most specifically the Pacific salmons, Coho

1 Chinook, in the Great Lakes.

2 Introductions of salmon to the Great Lakes were
3 attempted before the turn of the century and none of these
4 attempts met with any success.

5 This corresponds to the early historical phase
6 of stocking that I alluded to earlier where it was simply a
7 introduction of fish without much concern for the environment,
8 or the particular life history stage of fish that were being
9 introduced.

10 In more recent years, in the past five years or
11 so, renewed attempts at introducing Pacific salmon have met
12 with spectacular success. A number of factors account for
13 this.

14 One such factor is changes in the native fish
15 populations in the Great Lakes. Another factor is the
16 improvement in hatchery technology. The third factor is
17 the greatly increased understanding of the life cycle of the
18 fish themselves and the ecology of the system into which they
19 are being introduced, and when these three factors are put
20 together it is now possible to achieve really a staggering
21 measure of success through this type of stocking program
22 where previous efforts had met with complete failure.

23 I might add that I was associated with some of
24 the early thoughts of attempting to introduce salmon into
25 the Great Lakes. There were an awful lot of prophets of doom

1 around who said it had been tried before and wouldn't work.

2 A second example which is in salt water situations

3 is the success in supplementing natural populations of steel-

4 heads on the Pacific Coast, and there are numerous runs of

5 steelheads now in which a very, very significant fraction of

6 the catch is made up of hatchery fish which are introduced into

7 the rivers and subsequently migrate to the estuaries and then

8 to the high sees to return some two to five years later.

9 This, too, was a management application of

10 stocking that was very hesitantly moved into by the

11 governmental agencies because past failure and a tendency

12 to oversell the use of hatcheries had occurred, but the

13 States of Washington and Oregon have made significant steps

14 in using hatchery stock as a basic component of their

15 management programs, and adjacent governments such as

16 British Columbia are now really, I think, unable to withstand

17 the pressure to move in this direction.

18 There has been a general increase in confidence,

19 too, in the use of stocking of warm water fishes in fresh

20 water situations, and that is an area characterized by early

21 failure and for a time was discredited, but here again, both

22 hatchery technology and understanding of the processes and

23 natural populations are increasing to a point where I think

24 the single success story that I would cite would be that of

25 the Muskellunge in the Midwest area, but the general feeling

among managers is that breakthroughs in a number of other significant species are iminent.

Q In summary, is it your opinion that by January 1977 at the conclusion of the Indian Point ecological study program, that you would be able to have a reliable determination of the success of the stocking of striped bass in the Hudson River?

A Yes.

MR. TROSTEN: That concludes the redirect testimony of Dr. McFadden, Mr. Chairman.

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CHAIRMAN JENSCH: Are there any questions by the Hudson River Fishermen?

MR. MACBETH: Yes, I have a few.

RECROSS EXAMINATION

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BY MR. MACBETH:

Q Dr. McFadden, you spoke generally about the increased achievements in the hatchery field stock programs. Would you tell me whether any of the programs and hatcheries to which you are referring hatched any of the following fish:

tomcod;

bay anchovie;

blueback herring;

alewife;

American eel; or

white perch?

A I don't know of any case in which those species have been the object of hatchery technology. There are species whose general life history requirements are closely similar to some of those which have been propagated for a good number of years.

Q Which are the ones that are similar, or to which there are similarities?

A The whole group of North American spiney ray fishes provide numerous instances of close counterparts to the white perch.

dor 2

1 Q In the course of the last few days, there has been
2 considerable discussion of the hatching and stocking of
3 striped bass, and three particular instances of stocking in
4 natural esturine and marine situations that were discussed,
5 one in Florida, one in Alabama, at Mobile Bay, and one in
6 Mississippi, at what was described as, I believe, Mississippi
7 Bay.
8 I wasn't able to locate that on the map. I have
9 a feeling it is Mississippi Sound.
10 There was an earlier discussion of the stocking
11 into the Sacramento-San Joaquin system, which, of course, took
12 place in the last century.
13 Do you know of any other successful stocking of
14 striped bass into esturine and marine situations besides
15 those four examples?
16 A No, I don't.
17 MR. MACBETH: I have no further questions.
18 CHAIRMAN JENSCH: Regulatory staff?
19 MR. KARMAN: I have no questions.
20 CHAIRMAN JENSCH: Thank you, Dr. McFadden.
21 Dr. Geyer has some questions.
22 DR. GEYER: Referring to your April 24 response
23 to staff comments on applican'ts research program on page 2
24 in the thrid line from the bottom and about the 7th line from
25 the bottom, you use the word "serious," and would you put this

in percentage terms? At what percent level of productivity in the Hudson do you think you could make a measurement?

THE WITNESS: I am having trouble finding that. Page 2?

DR. GEYER: It is the first page, I am sorry. It is near the bottom of the page. You use the term "to detect probable serious enfironmental impacts," or "to detect serious impacts."

At what level of impact could you establish this?

THE WITNESS: In terms of percentage reductions, the statistics that we are collecting seem to provide an assurance of being able to detect reductions of the order of 25 percent.

Now, a lot depends on the timeframe within which you pose a question of reduction. What I am referring to here is a 25 percent reduction in a particular year.

DR. GEYER: Use your own definition of impact. What do you mean by "impact"?

THE WITNESS: Yes, if the situation postulated by the staff exists, that is, there is no compensatory process operative in the population, and the power plant reduces the population by 1 percent per year, in absence of compensation, the population would essentially become extinct, and all that would be required, if that were the case, would be to wate a few years and observe a continuous

1 drastic and fairly precipitous decline. I have argued
2 repeatedly that I think that is an absurd situation to postu-
3 late, and I don't believe it is consistent with any of the
4 natural populations which have been studied, so I don't think
5 that will happen, but taking their postulates, I would
6 say it is going to be easy to detect impacts of the kind
7 and magnitude that they postulate.
8 Our expectations are that we will be able to
9 take reductions in the order of 25 percent, and that, further,
10 we will be able to determine whether such reductions, if they
11 do take place, will tend to have a cumulative effect. That
12 is to say, if they aren't adequately compensated, they will
13 accumulate for several years until compensation effects
14 a leveling off of the population, or the drastic postulate
15 of the staff's, if it were the case, would express itself
16 even more vividly in the population.
17 Does that relate to it?
18 DR. GEYER: I think that is satisfactory.
19 THE WITNESS: Could I add one more point?
20 DR. GEYER: Sure.
21 THE WITNESS: There is objective basis to my
22 knowledge, before answering the question of how large an
23 impact is serious or not. That is sort of a general, elastic
24 term.
25 DR. GEYER: That was the problem with the word

1 "serious".

2 THE WITNESS: I recognize that. Certainly an

3 impact reduction which leads to continuing long term decline

4 toward extinction of the population would be called serious.

5 When a, say, 25 percent reduction of the population, a 10

6 percent reduction, a 50 percent reduction, which of those, if

7 any, you would call serious, could be answered on an economic

8 cost-benefit basis, I suppose, and I expect deficiencies

9 of that basis have been explored here, too.

10 DR. GEYER: Turning to the next page, the fifth

11 line from the bo-tom, you say "Since April, 1973," and this

12 is still April, 1973.

13 That is on page 3. It is the fifth line from the

14 bottom.

15 THE WITNESS: I have got it now.

16 April 1973?

17 DR. GEYER: What this is saying, it just started

18 this month, is that correct?

19 THE WITNESS: That is a typo. That should be

20 1972. It refers to the Texas Instruments full-scale

21 ecological study, and in the earlier McFadden-Woodbury testimony,

22 I cited other phases of the testimony that had begun back in

23 1969.

24 DR. GEYER: That is all. Thank you.

25 MR. BRIGGS: In judging whether the plants are

1 having an effect over the short term, at least, I assume you
2 have to determine the fraction of the survival taking, or
3 getting the number of eggs that are provided in the spring
4 and then the number of small fish that you have in the fall.
5 Well, do you have a number for that from last
6 year, what the fraction survival of eggs, or conversion of eggs
7 into smahl fish is?
8 THE WITNESS: No, sir. There were no usable
9 estimates of the initial egg complement made last year, nor
10 were there any estimates of the numbers of young surviving in
11 the fall.
12 The ecological program provides for obtaining
13 both of those estimates this year for the first time. There
14 are the various relative measures of survival that are
15 available to us for the different life history studies dating
16 back to the Cornwall studies of 1965, and 1968.
17 I take your question to relate to a pretty accurate
18 fix on what is out there in the river subject to entrainment
19 and impingement on Indian Point 2, is that correct?
20 MR. BRIGGS: There seem to be rather large
21 uncertainties of the fraction that survive, at least, based
22 on the previous data.
23 That is the impression I have gotten from the
24 testimony, and I wondered if we were going to get better data
25 for this uncertainty to be reduced.

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THE WITNESS: Yes, sir. In a simplified form, the plan entails estimating the abundance of preentrainment organisms, estimating at the plant the numbers that are entrained and impinged and estimating the number surviving in the estuary.

It is then possible by standard computational devices to separate the percentage mortality due to the plant and the percentage due to other causes in the estuary.

MR. BRIGGS: The measurements that are made by the plant are of some cause for concern, because suppose you run into the situation where you find a decline in population, and you say "Well, my measurements at Indian Point indicate that it is not responsible," and somebody else says, "My measurements at Roston indicate that Roston is not responsible."

Who will be responsible? There will be an unknown, where someone will say, "Something else we don't know about is causing the change."

THE WITNESS: Yes. I think it is entirely possible that a fairly long-term decline in the populations could take place and that the studies underway would be able to perhaps exclude the power plants, or incriminate them, as the cause of that decline. It would be possible for the studies to exonerate the power plants as the cause of decline and have the decline underway, measurable, but the cause not identifiable.

A look at the historical data on the striped bass populations will show very clearly that there have been both short run and long run rises and declines in that species fo fish, which are largely unexplained.

There have been a whole host of theories advanced to account for those fluctuations in numbers, and in my opinion, the numerical patterns of record could all be looked upon as a measure of the numerical range within which we could expect these populations to continue to fluctuate.

Many species of fish characteristically undergo such fluctuations.

MR. BRIGGS: There is some concern, I suppose, that the situation is one in which the plants are guilty until they are proven not guilty, and then there is a question of the quality of the proof that the plant is not guilty and the quality of the measurements that are required at the plant to provide this assurance, and one can foresee if there is a drastic decline in the population of the striped bass over a period of, let's say, the next theee or four or five years. that you are going to have a difficult job convincing anybody that it is not the plant.

Hopefully, the data will be good enough to show yes or no.

THE WITNESS: Yes, sir, and I know there is a bit of an unavoidable paradox based on the nature of scientific

1 evidence that is faced here.

2 As you call for a higher and higher quality of

3 proof, the size of the fluctuation in numbers that can be

4 ascribed to particular causes has to get larger and larger.

5 As you accept, let's say, flimsier and flimsier

6 proof, you enter around where, on the basis of flimsy proof,

7 you can ascribe a small fluctuation to a particular cause.

8 That is inherent in the scientific method.

9 CHAIRMAN JENSCH: Dr. McFadden, I don't know if I

10 made complete enough notes in your presentation here this

11 morning.

12 Let me ask you: You suggested that, as I recall

13 it, that any restocking program might involve, say, taking one-

14 year-old fish, say striped bass, and tagging them and putting

15 them in the Hudson River, and then if you recapture a fair

16 proportion of those in the succeeding March, you would assume

17 that the restocking program is working out all right. Is

18 that a correct recollection?

19 THE WITNESS: That is right.

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1 CHAIRMAN JENSCH: Do you know what predators would
2 affect one year old striped bass in the Hudson River?
3 THE WITNESS: Striped bass of older ages would
4 be potential predators. Large tomcods -- there are many
5 of the species of fish present in the Hudson which are
6 large enough and predatory by habit that they could be
7 considered potential predators of young striped bass.
8 We have data from trawling which show which species
9 occur in common together in particular types of habitats which
10 I can't recall here, but which would provide us even
11 better insight into what other species are the most likely
12 potential predators for young striped bass.
13 I would consider a substnatial portion of striped
14 bass for nine months or so, and older, up to a size of,
15 let's say, probably from the 9th to the 24th month of life,
16 I would imagine that a fairly sizable fraction of that
17 mortality is due to predation.
18 CHAIRMAN JENSCH: Those two illustrations you give
19 are the only two predators you could think of that would
20 affect the one year old striped bass in the Hudson River;
21 is that correct?
22 THE WITNESS: They are the only ones I would
23 comment offhand about. I would want to look at our data
24 on habitation by the different species in order to have a
25 better fix on what others were likely.

CHAIRMAN JENSCH: Have you had an opportunity to read some of the transcripts of the last few days in this proceeding? Particularly the testimony by Dr. Clark?

THE WITNESS: With respect to hatcheries?

CHAIRMAN JENSCH: Yes.

THE WITNESS: Yes, sir.

CHAIRMAN JENSCH: My recollection of his testimony was that he was equally enthusiastic as you are about restocking programs for fish other than the striped bass. Everybody is for restocking. I think it works well in many places with some kind of fish.

I think the problem we were discussing here just a few days ago is what luck do you have with striped bass, and I took his testimony to be almost 100 percent negative.

Now, can you tell us something about the restocking programs of striped bass in estuarine surroundings similar tothe Hudson River?

THE WITNESS: No, sir, I can't. It seems to me that Mr. Clark's testimony alluded principally to the kinds of poorly informed attempts at striped bass stocking which I characterize as typical of the early history of the use of stocking and fish management.

I myself am not sure that striped bass stocking would be successful in the Hudson. I am sure that credible

1 witnesses have testified that the hatchery technology is
2 in hand, and I am further sure that the techniques for
3 assessing the survival of stocked fish are available, so that
4 we can determine whether it has succeeded or not.
5 I don't particularly feel qualified, having read
6 the literature and having been involved in the studies that
7 ~~death~~ dealt with striped bass estuarine stock, and stocking to
8 make a prediction of probability of success.
9 The main point of my testimony is to say that
10 success and failur can be determined.
11 CHAIRMAN JENSCH: Yes. I also inferred from
12 some statements by Mr. Clark that the hatchery techniques
13 were improving and he recognized that, but strangely enough,
14 I thought he indicated that the restocking of striped bass
15 was in freshwater areas, and they have had marvelous
16 results, but he keeps searching, and I have heard no such
17 suggestion from anybody, that striped bass restocking in
18 estuarine surroundings has worked at all, and they keep
19 looking for those data. You don't know of any?
20 THE WITNESS: No, sir.
21 I guess I haven't been able to get as concerned
22 over the differences between the fresh water and estuary and
23 situations as has Mr. Clark. There are predators in both,
24 there are diseases in both, competitors in both, currents
25 in both, et cetera, et cetera.

1 The principal differences are in the salinity of
2 the two environments. Those aren't the only differences,
3 but the salinity is the main difference.
4 CHAIRMAN JENSCH: I didn't get the first part
5 of your last answer. You weren't what?
6 THE WITNESS: I am not quite as concerned about
7 the significance of the difference between freshwater
8 and estuarine environments as Mr. Clark appears to be from
9 his testimony.
10 CHAIRMAN JENSCH: There are no data that seem
11 to indicate that striped bass can be restocked in estuarine
12 circumstances. Does that persuade you that the restocking
13 cannot work and there is no experience that it will? Whether
14 it persuades you or not, does it dampen your enthusiasm about
15 this?
16 MR. TROSTEN: Are you excluding the estuarine
17 situations that have ben referred to that have been
18 restocked?
19 CHAIRMAN JENSCH: Where is that?
20 MR. TROSTEN: In Mobile Bay, and the San Joaquin
21 situation.
22 CHAIRMAN JENSCH: I am glad you mentioned that,
23 so that Dr. McFadden will keep in mind his answer. I
24 understood as to the Choctawatchee and the Mobile Bay that
25 they just aren't living in this saline area. They are going

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1 up the freshwater streams adjacent to it. You can correct
2 me, but I had the impression that they dropped this restocking
3 in the bay and they go up the river, so we really are
4 concerned as to how we are going to replenish the Hudson
5 Bay area and the Mid-Atlantic species. I don't know about
6 the San Joaquin situation, but I understood those two
7 down south you can write off as far as getting any restocking
8 in the saline areas.
9 If I am incorrect in that, I would like to be
10 corrected.
11 But in any event, let me continue with the
12 witness. I am very sorry that Dr. Stevens left. I recognize
13 he was excused, but there were several things that Dr.
14 Goodyear mentioned about some of the factors that Dr.
15 Stevens had mentioned that I wish we could have had a
16 further comment on from Dr. Stevens, but we won't ask him
17 to come back.
18 I am trying to pick up from Mr. Clark on this
19 situation about the restocking of the striped bass.
20 I would exclude from my questioning the Mobile
21 Bay and the Choctawatchee.
22 The other evidence seems clear that the fish
23 are not surviving in the saline areas, so my question is,
24 and I think just to summarize, you don't know of any situation
25 where the striped bass has survived in saline estuaries?

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1 THE WITNESS: None other than the San Francisco
2 Bay, San Joaquin River system. I think what we are
3 talking about in these other instances isn't the failure
4 to survive, but a failure to behave in such a way that
5 they inhabit the saline estuary environment. The fact that
6 they have chosen to go elsewhere doesn't mean they haven't
7 survived.
8 CHAIRMAN JENSCH: I don't know that they went
9 elsewhere. There are so many predators, perhaps they were
10 waiting for them to drop in, and that is as far as they
11 got. I don't know whether it is necessarily true that they
12 went to the freshwater areas. Some did get away, perhaps,
13 from the predators. I think the whole situation hasn't
14 been fully dealt with at all.
15 THE WITNESS: No. On balance, I see the record
16 containing examples of success, in the San Francisco Bay,
17 the San Joaquin examples, and failures.
18 In the case of failures, it isn't clear to me
19 from the testimony whether they were fair tests or not.
20 We have had failures in fish stocking that have been
21 reversed by later efforts.
22 CHAIRMAN JENSCH: I think you indicated that if
23 stocking doesn't succeed at first, you try again, and you
24 found the coho to be in that pattern. So far as I know,
25 that theory has not worked for striped bass in estuarine

1 situations.

2 If there are data, I would like to have a

3 presentation in that regard. I take it that the whole

4 restocking situation depends on the kind of fish and

5 the kind of surrounding.

6 Do you not agree?

7 THE WITNESS: Yes.

8 CHAIRMAN JENSCH: I have no further questions.

9 DR. GEYER: I would like to ask one more question.

10 I always understood the salmon didn't feed when

11 they are on a spawning run; is that correct?

12 THE WITNESS: That is partly correct. They

13 don't feed significantly. They return to the rivers with

14 sufficient stored food reserves to live off them in order

15 to reach the spawning grounds. Some of the other migratory

16 salmon feed more consistently when spawning. Steelheads

17 enter the rivers as early as June or July in the summer

18 preceding the spring when they will spawn. They might be

19 in the river from June or July of one year until April

20 of the next when they finally spawn.

21 So there is quite a variation in the degree of

22 feeding that is carried on by different migratory fishes.

23 DR. GEYER: What is the situation with regard to

24 striped bass and shad in this regard?

25 THE WITNESS: The striped bass linger in the

1 estuary for a good many months, and they do some significant
2 amount of feeding.
3 In the case of shad, it is my understanding that
4 the run is of a shorter period of time and the amount of
5 feeding that takes place is fairly incidental.
6 DR. GEYER: Thank you.
7 CHAIRMAN JENSCH: Are there further questions?
8 MR. TROSTEN: Mr. Woodbury has an appointment
9 of long standing. I wonder if it would be acceptable to
10 the Board if he could be excused at this point.
11 CHAIRMAN JENSCH: So long as he is subject to
12 immediate recall, we have no questions.
13 Is he likely to be gone the rest of this session?
14 Why don't we recess, and would it be inconvenient for Dr.
15 McFadden to be here after lunch?
16 MR. TROSTEN: I beg your pardon?
17 CHIARMAN JENSCH: Would it be inconvenient for
18 Dr. McFadden to be here after lunch?
19 MR. TROSTEN: I am advised by Dr. McFadden that
20 if it would be at all possible to conclude with him before
21 lunch, it would be appreciated. He does have another
End #11 22 appointment.
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CHAIRMAN JENSCH: Maybe we can take this up later. Maybe we will get a suggestion after lunch as to the agenda for the ensuing times of hearing in this proceeding. Whether we are likely to finish this week or next week.

MR. TROSTEN: Mr. Chairman, I really believe we can conclude. The only other thing that we have to offer is a brief amount of redirect testimony by Dr. Lawler which directly pertains to what has come up in the last day or so.

CHAIRMAN JENSCH: Let's not take that up.

Mr. Woodbury is excused.

(Witness Woodbury excused.)

CHAIRMAN JENSCH: Let's take the questioning to Dr. McFadden.

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RECROSS EXAMINATION

BY MR. MACBETH:

Q Would you consider a decline in the landings of fish from 1,950,000 pounds a year to 1,300,000 pounds per year over a five-year period to be a serious decline in a fishery?

A For what species?

Q Striped bass.

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

(The reporter read the pending question.)

MR. TROSTEN: What is the foundation for this question?

CHAIRMAN JENSCH: I don't think that that question calls for anything related to the record so far. It is just that he is trying to get, as I understand, the scope of the definition of "serious."

MR. TROSTEN: Mr. Chairman, do you not agree that it is incumbent upon Mr. ~~McFadden~~ Macbeth to state whether this is a hypothetical assumption, or whether he is relating this to something in the record? I think it is important for the witness and the record to reveal what the basis of his numbers is.

MR. MACBETH: I am interested in the notion of seriousness which Dr. Goodyear was inquiring about, and the

1 figures I have taken are my reading of the chart which Dr.
2 Lawler included in his testimony of April 20, following page
3 15.
4 MR. TROSTEN: Would you give us a moment so that
5 Dr. McFadden can scrutinize this, and then we can proceed
6 with the questioning, and then I would like to have you
7 identify the points we are discussing here.
8 MR. MACBETH: I have scaled this off. One reason
9 I was putting it in hypothetical terms is that I may not
10 have these numbers exactly correct, but I was looking at
11 what seems to me to be the decline from 1,950,000 to
12 1,300,000 in the Atlantic landings between 1967 and 1972.
13 THE WITNESS: The Atlantic landings?
14 MR. MACBETH: Yes.
15 MR. TROSTEN: Dr. McFadden, do you accept the
16 premise of the question?
17 THE WITNESS: Yes, I do.
18 CHAIRMAN JENSCH: Maybe you could step back a
19 little, so that the witness can see the interrogator.
20 BY MR. MACBETH:
21 Q We can return to the earlier question. Would
22 you like to have it reread?
23 A No, it is not necessary.
24 First, I answer in the context of observing a
25 set of commercial fishery data such as we have here without

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1 any additional interpretative data base to operate from.
2 In that case, I would say that the decline that you cite
3 would not in itself be significant. It is well within the
4 range of natural variability which has taken place over
5 the historical record for the fish stock.
6 Q If that decline could be ascribed to any single
7 source of impact, would you consider that to be a serious
8 impact on the fishery?
9 A It would depend on what the cause of the impact was.
10 If a decline of that kind had taken place in response to
11 some environmental variable, for example, which fluctuated
12 up and down over the historical record and which could be
13 expected to reverse itself subsequently, once again I
14 wouldn't consider it a serious decline.
15 Another possibility would be that the decline
16 was associated with an impact that was imposed, say, in a
17 pollution method or the operation of power plants or something
18 of that kind, and that the population -- the data base
19 showed that natural environmental parameters had all been
20 favorable over that period so that the decline seemed to be
21 ascribable to these external impacts, one would like very
22 much in that case to have data of the kind that are now
23 being collected in the Hudson River available to see if
24 the picture actually being taken into account, the power
25 plant, or, say, it is a pollution impact to, say, whether the

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chemical parameters of the river actually are exceeding tolerance levels for the fish, I would like to have some direct, not just distant circumstantial evidence, before you interpreted the seriousness of that decline.

Is that sufficient?

Q Yes.

Assume that the environmental conditions were favorable, and further assume that the decline could be associated, or was associated, with the effect of power plants in the Hudson River. Would you then consider this to be a serious impact in the operation of the power plants on the fishery?

MR. TROSTEN: Serious from what point of view?

CHAIRMAN JENSCH: I know what your question is, but use the language he has used. He talked about serious environmental impact, and in order to detect serious impacts. I think he is trying to get the definition of the words he has used, is that correct?

MR. MACBETH: Yes.

CHAIRMAN JENSCH: All right.

MR. TROSTEN: If the question is related, as is apparent from the Chairman's request, to "would this be considered to be serious in terms of Dr. McFadden's testimony," I would have no problem with that.

THE WITNESS: There would be one additional piece

1 of information that I would like to have in order to judge
2 the seriousness or nonseriousness of that kind of decline.
3 That would be some indication as to whether the decline
4 was likely to continue, or whether the population was likely
5 to level off at, say, the 1972 level.
6 If the population were going to level off at
7 the 1972 density, it would still be comfortably within the
8 historical range of densities that have occurred. If
9 indications were that the population were going to continue
10 to decline beyond 1970, and of course the slope is pretty
11 steep from 1967 to 1972, the population, if all indications
12 were that the population were going to continue to decline
13 along that trend, in terms of the striped bass population,
14 taking that as a value unto itself, that would be a
15 pretty serious indication -- it would be an indication of
16 what I would term a serious impact on the striped bass
17 population.
18 Q Are you pointing out that if the striped bass
19 population were to level off in 1972, that it would not be a
20 serious impact?
21 A It would be a different kind of situation, and the
22 definition of serious would then change. Then we would not
23 be asking the question, "Is a continuing decline on a trend
24 that is rapidly going to approach a level of extinction
25 serious?" And I am saying, yes, ecologically, in terms of

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1 the striped bass, that looks serious indeed to me.

2 If you ask does a decline of 25 percent from

3 that peak year and stabilization at that point represent

4 a serious decline, I think my response would be no. I don't

5 think that does represent a serious decline, because the

6 population still lies comfortably within the limits of

7 its even recent historical abundance.

8 Actually, I find the question a little difficult

9 to respond to, because I think that the sharp ups and downs

10 on this graph don't represent something like true long-term

11 trends. Those are short-lived surges in abundance or

12 declines in abundance, and it would be more useful to

13 phrase the question in terms of some, you know, long-term

14 average level of abundance and say, "Has the impact caused

15 the population to be reduced below that long-term average

16 level of abundance?" That would be a more meaningful way

17 to phrase it.

18 Q Assume that the population of striped bass in

19 the Hudson River has been growing for the last four years

20 at an average of 5 percent a year, and has now reached a

21 population level without further environmental perturbations,

22 and that the population would be increasing.

23 There is then an impact -- there is a decline

24 in the population -- associated with the operation of

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25 power plants which reduce that population to 25 percent, over

1 a period, say, of five years and held at that level.

2 Would you consider that a serious impact from

3 the operation of power plants?

4 A From the standpoint of the ecology of the popula-

5 tion, I would say that is not a serious impact, in that the

6 population has now absorbed an impact and reduced its

7 average level of density, but it is persisting in a healthy

8 state at that level.

9 You can ask the question whether this is a serious

10 impact or not from an economic point of view, and then you

11 would invoke a cost-benefit analysis, power plant versus

12 fish production. You could invoke it from a political

13 point of view and possibly come out with another answer.

14 From the standpoint of managerial ecological approach to

15 populations, having reduced it by 25 percent, and having

16 increased that impact and stabilized at a new equilibrium

17 is of no particular problem to a population.

18 If you choose to manipulate it further by

19 removing the impact, you can at some subsequent point in

20 history. The population is stabilized at a level from which,

21 if you want to continue to maintain the impact, you can over a

22 period of time try to implement various kinds of supplemental

23 measures with that species or other species of fish, on and

24 on and on.

25 The really serious situation is one in which you

1 kick a population into a continuing decline, or reduce it to
2 so low a level of abundance that it is in jeopardy of going
3 to extinction from some further natural fluctuation, or
4 something of that kind.
5 Q What level of reduction do you think that would be
6 in the striped bass population of the Hudson River?
7 A It would be a drastic level of reduction, but I
8 don't know in anything like percentage or absolute numerical
9 terms what it would be.
10 Q Just give me a range. I don't want to pin you
11 down to some specific number, but are you thinking of 50
12 percent, or 90 percent?
13 A When you go to a large order of that kind, I
14 would think that reducing the population to 90 percent
15 of its 1950 level of abundance or something like that, you
16 know, might put it down there. Once again, it is, you know,
17 a difficult scenario we are dealing with here, what popula-
18 tion over how broad a geographical range, et cetera, et
19 cetera. Those are very important qualifiers.
20 MR. TROSTEN: Excuse me. When you say reducing
21 the population to 90 percent of its previous level --
22 MR. MACBETH: The 1950 level.
23 THE WITNESS: I meant by 90 percent to 10
24 percent. It is an important distinction.
25

BY MR. MACBETH:

Q You did mean 10 percent of its 1950 level, is that correct?

A Yes, that is right.

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MR. MACBETH: I have no further questions.

CHAIRMAN JENSCH: You can go through these figures one by one, but I don't want to pose that kind of question. I think one of the questions put to you by the Hudson River Fishermen's Association, if the population declined from 1 million 600,000 to 1 million 300,000, would you regard that as a serious impact, and I think you said that you didn't think it would be, because it might be a normal cyclical change of some sort.

Will you put a figure on where you do think it would be a serious impact, say, starting from 1,600,000 down to what before it would be serious?

THE WITNESS: I answered my previous question, you know, within several different contexts. Let me answer this: within the context of an observed decline, no supplementary data available to give insight into the mechanisms offered in the population. Is that an acceptable context?

CHAIRMAN JENSCH: Yes.

THE WITNESS: I want one further qualifier, and I will agree to whatever stipulation you want to make here. I have to know whether I am answering serious from the Hudson River Fishermen's point of view, or serious from society's point of view, or my personal point of view.

CHAIRMAN JENSCH: Just whatever you meant by your language in your own document. I don't know where you put it. Put in all the qualifications you desire, but you said

1 the Staff has criticized the program, and it is doomed to
2 something, and you see it isn't necessary to analyze all the
3 cause and effect relationships in order to detect a serious
4 impact.
5 Now, start with a figure, 1,600,000, as Dr. Lawler
6 did, and tell us when it gets down to the figure when there is
7 a serious impact as you use the term.
8 THE WITNESS: No, sir, I can't answer that. I
9 can't answer that in terms I am sure are, you know, acceptable
10 to all of the different parties.
11 CHAIRMAN JENSCH: Just with your own. You don't
12 have to make it acceptable to a single soul.
13 THE WITNESS: From an ecological point of view,
14 starting at point 1972, I would say that a decline to levels
15 of 60,000, if there is no direct evidence that the power plant
16 is causing them, would be well within the range of natural
17 variability in the population, and wouldn't represent a serious
18 decline.
19 In fact, I wouldn't be a bit surprised under the
20 assumption that power plants have no impact at all on this
21 population, I wouldn't be a bit surprised to see the trend line
22 continue to drop beyond 1972, at least down to levels of, say,
23 60,000, and to repeat something like the pattern we have seen
24 in this figure over subsequent 40 year periods of history.
25 CHAIRMAN JENSCH: So that is clear, you mean a decline

from 1,600,000 down to 60,000?

THE WITNESS: That is right. 600,000, it should be. I dropped a decimal point.

CHAIRMAN JENSCH: I am trying to be clear again. How far did you say it could go down before it would be serious?

THE WITNESS: From 1 million -- what is the 1972 figure we started from? Your figure was 1.6 million. From 1.6 million down to 600,000.

CHAIRMAN JENSCH: If they go down to 600,000, then there would be a serious impact?

THE WITNESS: There would be a serious -- if the decline took the population below 600,000, the index to the population below 600,000, I would say that the decline was serious.

CHAIRMAN JENSCH: But it wouldn't be serious until it got to 600,000, is that your view?

THE WITNESS: It wouldn't be serious in terms of the ecological history of the population, that is, in terms of the ups and downs. I am sure that certain economic interests would consider it serious in terms to them, but that is not the definition of serious that I was using in my testimony.

CHAIRMAN JENSCH: What is the definition you are using?

THE WITNESS: An indication of an ecological disruption that threatens the continued health of the fish stock.

CHAIRMAN JENSCH: Now, will you put that into a number?

THE WITNESS: In the example that we are discussing, I believe that the decline from a level of 1,600,000 to a level of 600,000, a decline exceeding that would be evidence of a possible serious disruption within the fish population.

CHAIRMAN JENSCH: And not until they reached that figure, is that correct?

THE WITNESS: That is right. Under the postulates we agreed on in this answer, he wouldn't feel there is anything serious there. I would feel the population would be -- quite possibly would fluctuate down to levels of that order due to natural causes, and there would be every reason to expect it to subsequently fluctuate back up again.

CHAIRMAN JENSCH: In your second sheet, and I guess it is the first page of your text, it says five or six lines from the bottom of the page -- do you have that before you?

THE WITNESS: I do now.

CHAIRMAN JENSCH: It says that, "The fact that it is not necessary to analyze all the cause and effect relationships which govern population of striped bass and other fishes in the Hudson River in order to detect serious impact from the power plant and implement managerial procedures," and so forth.

If it is not necessary to analyze all the relationships, what would be the necessary cause and effect relationships

which should be analyzed? Will you enumerate those?

THE WITNESS: Yes. It would be necessary to separate those effects caused by operation of the power plant or any mitigating managerial measures that were implemented from natural causes of fluctuations in fish population numbers.

CHAIRMAN JENSCH: Can you specify with something more than that generality? What would be ---

THE WITNESS: Some of the natural ones that I say don't need to be understood?

CHAIRMAN JENSCH: No, I am trying to find out the ones you think should be analyzed.

THE WITNESS: Those concern the measurement of the abundance of fish at large in the estuary and the numbers or percentages of those fish which are entrained or impinged.

CHAIRMAN JENSCH: That is all?

THE WITNESS: Yes, sir.

CHAIRMAN JENSCH: If you have a program that will just measure the number of fish at large in the river and the percentage entrained, that is enough for the program. Is that your view?

THE WITNESS: That is right. That is a very simplified outline of what is set forth in greater detail in the testimony explaining the ecological study.

CHAIRMAN JENSCH: Yes, I understand, but I am trying to get your own sentence here, and you say it is not necessary

1 to analyze all the cause and effects. I am trying to find
2 out what ones in your opinion, not those offered in some other
3 document, but what you mean when you mention two items, and
4 that is if you have a program to determine the impact of the
5 power plant operation. Is that correct?
6 THE WITNESS: That is correct.
7 CHAIRMAN JENSCH: All right. On page 2, you say
8 that nothing is to be gained by developing a model which is
9 a complicated and hence to understand and manage as the real
10 world problem situation.
11 Why isn't it necessary to develop a model and relate
12 it to reality?
13 THE WITNESS: If I understood your last statement,
14 sir, you said why isn't it necessary to develop the model which
15 is related to reality?
16 CHAIRMAN JENSCH: Yes.
17 THE WITNESS: I hope I didn't say that.
18 CHAIRMAN JENSCH: You said it isn't necessary to
19 get a real world model. I assume that is reality.
20 THE WITNESS: That isn't the same as saying it
21 is related to reality. What we would like to do is develop
22 a model which is simpler than complex reality but which closely
23 enough mimics it ---
24 CHAIRMAN JENSCH: I think that is a good objective.
25 Does anybody ever reach it?

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Reba 7

THE WITNESS: It is the standard objective in management of an ecosystem of this type, and the form that it will usually take is a set of data which provide insight into the gross effects caused by different kinds of impacts.

For example, if the power plant removes a certain proportion of the striped bass at a particular life history stage, and no reduction is subsequently counteracted by some natural compensatory processes, who cares what the process is? Why bother obtaining an interesting but academic level of understanding of the articulate process when you have sufficient overall understanding to answer your management question?

There is a second point that is important here, too, and that is that, as I state in my testimony, if you did have all this insight and understanding, its complexity would overwhelm you and you couldn't effectively use it in a management situation.

A third consideration is that there is a great deal of random variation operative in the natural system, and by that I mean changes in population parameters which are not ascribable to any measurable cause, and factors of philosophical question, you know, as to how far -- whether there is a component of some kind of pure randomness in these systems that is simply inherently unmeasurable or not.

So the standard management situation is one of managing in the face of a large component of uncertainty.

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Reba 8

Some team from a very academic point of view might view that as a very undesirable situation. We don't have enough insight into the system, that is. I think it is the standard real world situation.

Management decisions in business and these kinds of scientific managerial situations, too, are routinely made in the face of great uncertainty. That doesn't mean that they aren't, with a suitably high batting average, made successfully.

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1 CHAIRMAN JENSCH: I think the diffuclty here,
2 Dr. McFadden, is something Mr. Briggs indicated. The proposal
3 that the Applicant has made here is to defer the whole thing
4 about the ecological effect until three or four years, and
5 then you try to prove that Con Edison has anything to do with
6 what is happening in the Hudson River.
7 It is an attempt to throw the burden on something
8 else to say the plant is causing something.
9 You say, well, take a standard management
10 technique, and it has been good enough here and there, but
11 we have a more precise situation here and now, and the
12 question is whether we can wait until some years to get all
13 these molecules counted and the eggs tagged and floating
14 and things like that.
15 It gets to be kind of an academic exercise,
16 interesting and wonderful for literature and something that
17 will thril people to read when the program is over, but
18 right now there is a question that we have got impingement
19 and entrainment possibilities, the measure of which is not
20 known, but you have a contributing factor, apparently, from
21 the Indian Point plant.
22 Instead of saying you are going to point figures
23 somewhere else as to who is causing the most of it, the
24 question really here is if there is a causative factor known
25 now, the extent of which we don't know, perhaps, or perhaps

1 we do know, is sufficient to say "Well, this is one corrective
2 step we are going to take in the process right now. We are
3 not going to wait until we see what Roseton and Bowline, and
4 the Applicant feels somewhat resentful that no one is
5 proceeding against Roseton and Bowline.
6 I don't know whether the New York State Agency,
7 whether it is the Department of Conservation or who is going
8 to do something about it, but that is not our problem.
9 I get back to your situation. You say "Well, we
10 don't have to have anything related to the real world." You
11 say you don't want anything in reality. We have a problem
12 here that is different under standard management problems,
13 and it requires a precision to justify the escape from
14 present resolution of the matter.
15 I think that is where the Applicant finds itself.
16 The Applicant has come in with a presentation of saying,
17 "Well, we have a lot of things here. We are going to get a
18 lot more later. And don't do anything now.
19 "I know we will have impingmeent and entrainment,
20 but it is something that, well, it is a calculational model
21 or something that we will develop later."
22 In fact, we have models here neither one of which
23 reflects the fact that there is entrainment and impingement,
24 and I think it is lovely to wait for years and years and years
25 because Con Edison has been on the Hudson River since 1960 with

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1 Indian Point 1, or 1963, or whatever it is, and now you have
2 the program going and I just wonder whether your suggestion
3 in here, that, well, we will take a general approach to it,
4 and we have gotten along with it for some time, and this is
5 good enough here.
6 I just think the approach you have is unrealistic
7 to the problem we have before us.
8 Would you care to comment?
9 THE WITNESS: Yes, sir. The burden of proof
10 controversy is a very old one in resource utilization, and
11 it isn't a scientific controversy. So a great deal of your
12 commentary was relative to that question.
13 Indeed, there is one direction in which you can
14 decide the burden of proof question which completely
15 obviates our discussion here.
16 If the burden of proof lies with an Applicant
17 before utilization of the resource can be effected, then you
18 simply won't have allowed any of the kinds of resource
19 developments upon which our civilization is built.
20 CHAIRMAN JENSCH: I didn't hear the last.
21 THE WITNESS: I said that if the burden of proof
22 that no serious impact upon environmental resources would
23 occur, if that burden of proof were imposed on all
24 prospective resource developers before the development were
25 allowed it would have precluded all the resource developments

1 which have taken place and upon which our society is built.

2 CHAIRMAN JENSCH: The times have changed now. We

3 have the Environmental Protection Act and we have the

4 Atomic Energy Act, and some of these standard techniques

5 and standard management analyses that kind of are under

6 scrutiny, and great has been the development of America,

7 and we should all take another place and compliment

8 ourselves or those who preceded us, I don't know which, but

9 in any case, we can't say what we have done before we are

10 going to do again.

11 So I think you can take that premise out of your

12 analysis, too.

13 MR. TROSTEN: Mr. Chairman, may I comment

14 briefly?

15 I think the enactment of the National Environmental

16 Policy Act has not placed an absolute burden upon applicants

17 for licenses to demonstrate that no environmental harm is

18 going to occur.

19 CHAIRMAN JENSCH: I don't think that is it at

20 all. I agree with you.

21 MR. TROSTEN: It calls for the assurance that

22 environmental values are properly placed and taken into

23 account, and to the extent this hasn't been done in the

24 past, it should be done now.

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25 CHAIRMAN JENSCH: Yes. I think we will want to

1 discuss that later.

2 The Board is thinking of asking the parties for

3 oral argument on that further.

4 My questions relate to Dr. McFadden's discussion

5 here that we take a standard management technique that has

6 been good before, and it will be good now. I wonder if that

7 approach is the one we have before us. I wonder if he isn't

8 saying whether America would be developed under the laws

9 we have today. I don't know whether it would or wouldn't.

10 But we are in a different ballpark today.

11 THE WITNESS: I would be glad to answer, if given

12 a chance.

13 The respect in which I referred to the standard

14 management situation was not in relation to past approaches

15 to environmental problems of this kind having been adequate.

16 I was simply making the point that uncertainty in management

17 decisions is an inherent property and isn't going to be

18 escaped through any device that we can utilize in the future.

19 What we have proposed here is a study which in my

20 opinion is sufficiently accurate and deals with the right

21 assemblage of parameters to determine whether a serious

22 environmental impact is being caused by these power plants.

23 The second significant consideration to this

24 position is that there may indeed be an impact, the study

25 that we proposed is by and large far superior to studies that

1 have at the present or in the recent past been carried out
2 in answer to this kind of a problem, and that the study is
3 of an appropriate time scope and of sufficient sensitivity
4 that it would detect a serious environmental impact if one
5 occurs in sufficient time to implement corrective measures.
6 The final point that I think is significant is this:
7 the impact itself is an important source of the information
8 that is generated to determine whether it is serious or not,
9 and if studies -- if one required that studies were carried
10 out -- take the example of the Hudson River -- studies
11 were carried out that would determine whether any serious
12 environmental impact could take place or not in the absence
13 of any kind of impact on that system, in my opinion the
14 studies would be inconclusive.
15 The best chance of determining whether an
16 environmental impact will be serious or not is to carry
17 on studies while some substantial potential impacts are being
18 imposed on the system.
19 What it really amounts to is from a scientific
20 point of view experimentally manipulating the system, and as
21 long as you provide adequate safeguards so that the
22 experimental manipulation won't run out of control and
23 irreparably damage the system, you are in a safe and sound
24 managerial position.
25 Now, you can invoke a more stringent approach and

1 insist that all these matters have to, a priori, be settled,
2 and only when there is conclusive evidence of no impact, no
3 serious impact, will these kinds of developments take place,
4 if that is the position to be taken I think it is going to
5 consign us to an inability to predict serious impacts, and I
6 think it is going to consign us to a stalemate in development.
7 It is simply a trade-off in alternative values to
8 society, as I see it. You can go either direction.

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CHAIRMAN JENSCH: Have you completed?

THE WITNESS: On that question. Before we finish, I would like, and we don't need to interrupt this line of questioning, I would like to add a comment about the fishery data that we discussed earlier that I think would be useful to you.

CHAIRMAN JENSCH: I want to give you a chance for that, but I also want to give you a chance to answer my question, and you have not further in that record.

THE WITNESS: No.

CHAIRMAN JENSCH: You gave us a 3 point program here of "Why bother to trim this particular process?" I think those were the words you used, and in my comments I am indicating to you that it is necessary, because if the Applicant's proposal is accepted in this proceeding, it throws the burden on the Regulatory Staff or the public to come in and say, "Con Edison is causing it all", whereas we know we have a causative factor here to some extent, which requires the balancing that Applicant's counsel just indicated.

But your further said that if you get all the insight that you have about this, you couldn't use it anyway, and that is, of course, a concern that might be entertained in reference to this ecological study that you are undertaking.

Another factor that of course, I am sure is in the minds of many people, is that with Con Edison having been

on the river and having made some studies which they felt were adequate at that time, is in a sense asking for more time now, and I think the question also comes up, can Con Edison have the time any more? You have two plants going, and we don't consider here Indian Point 3. We can't close our eyes to the fact that it is there.

Sure you can get more data, but if you have it, would it be within the scope of a definite fixation of responsibility? Now that is my point of difference with you in your using standard management techniques. Any business determination involving the future involves uncertainty.

That is a premise that is aside. You don't have to dwell on it or expand on it. But it seems to me that when you say, "Well, the Staff presented something that looks like it is too casual,"or something to that effect, I think that you cannot balance the requirements of this proceeding in dealing with a situation that you think that the previous standards are adequate.

I will give you a further chance.

THE WITNESS: The effect of the testimony on page 2 is that we are going to collect the data that are requisite for making the determination about serious impact or no serious impact. I am saying that this approach has been, I think at least implicitly -- no, it has been explicitly -- criticized by the Staff who say, "You don't understand all the other things

Al 15 Reba 3

1 going on out there." I say that we don't propose to take the
2 length of time that would be required to understand all the
3 other things, because you don't have to understand all the
4 other things in order to make the determination of serious
5 impacts or no serious impact of this power plant.
6 We are focussing on identifying the plant impact,
7 separating it from the very complex set of natural processes
8 going on. We don't feel compelled to answer the question
9 about, well, what if the tomcod population goes up or down
10 over the next 3 years, what effect will that have on the striped
11 bass?
12 You can set forth an endless set of questions of
13 that kind and insist that you can't make a reasonable deter-
14 mination of the power plant impact until all those questions
15 are answered, and my position is that that is not correct.
16 You don't have to answer or be able to answer all
17 those questions in order to be able to answer the primary
18 question about the impact of the power plant. I think it is an
19 unfair criticism of the research program to point out the things
20 that it can't do and insist that those have to be done in order
21 to understand the system, when the level of the understanding
22 of the system that you are referring to is a level beyond that
23 necessary to make the primary determination about whether the
24 power plant is causing a significant impact or not.
25 CHAIRMAN JENSCH: It is a balancing of the measurement

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of the level you are seeking. I think that is what it comes down to. Did you want to add something further?

THE WITNESS: About the fisheries, yes. Referring to the graph that we talked about before, I looked at the ups and downs as ---

CHAIRMAN JENSCH: You are referring to Dr. Lawler's presentation?

MR. TROSTEN: Yes. I would like to just make clear at this point, Mr. Chairman, that Dr. McFadden's testimony in response to the Board's question as to what he would regard as a serious impact was given on the basis of the data presented in the table appearing after page 15 in Dr. Lawler's testimony of April 20, 1973, on the contribution of the Chesapeake Bay to the striped bass fishery in the Middle Atlantic States.

CHAIRMAN JENSCH: I don't know if that premise is correct. I asked him to take a series of numbers to get this impact. The Hudson River Fishermen's Association referred to figures in Dr. Lawler's statement as your suggestion, but really the question that I propounded to Dr. McFadden was to get the scope of his definition of "serious impact."

I don't care what figures are used.

MR. TROSTEN: I think we should have the point clarified.

CHAIRMAN JENSCH: I don't think it will do any good

to discuss it between you and me at the moment. We can do it later. Dr. McFadden can go ahead with whatever he desires.

THE WITNESS: When I presented the graph on the historical abundance of the population, such as the one in Dr. Lawler's testimony, on the various drafts in the Final Environmental Statement, which are essentially the same set of data, I don't look at, say, a particular point and say the peak 1967 level of the population, and say that that is good, and anything that happens to diminish the population from that is a significant reduction in the population.

I look at the historical data and try to sense or do it in an ad hoc way. I try to sense some kind of historical trend, what kind of level represents the average level at which the stock persists, and then I don't get much excited about these squiggles up and down around that line.

Some people attach more significance to the squiggle than I do, and they consider an up or a down a much more significant event than I do. I think that that is something you would want to bear clearly in mind when you try to interpret my testimony here.

I am looking at a population, as I see it, that has some kind -- the data certainly suggest an upward trend in abundance. Whether it is continuing to trend upward or whether it is leveling off is not clear to me from the data, but the big jumps are not particularly impressive in my mind

A1 15 Reba 6 as departures from historical trend. So when somebody talks
2 about the 1967 to 1972 decline, if they are thinking in terms
3 of some kind of a permanent shift in abundance of the popula-
4 tion, they are thinking of a very different thing than I am
5 thinking.
6 CHAIRMAN JENSCH: Yes. I think the only question
7 to you was, where would you put a figure on what you consider
8 a serious impact of power plant operations?
9 I don't care what figures you use or anything else.
10 Just give us an illustration. I think you have.
11 THE WITNESS: Yes, sir.
12 CHAIRMAN JENSCH: Is there anything further?
13 MR. TROSTEN: I have one question to ask of Dr.
14 McFadden. Is it correct that on pages 31 and 32 of your
15 testimony of February 5, 1973, you have listed the sequence
16 of calculations that would be carried out with respect to
17 determining mortality and survival rates?
18 That is, of striped bass?
19 THE WITNESS: Yes, that is correct.
20 CHAIRMAN JENSCH: Does that conclude? You said
21 you had one question.
22 MR. TROSTEN: I have one additional one, Mr. Chairman.
23 Is it also correct that on pages 33 and 34 of the same testimony
24 you have listed the ten specific criteria which you propose
25 for assessing the impacts on fish populations?

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THE WITNESS: Yes, sir.

MR. TROSTEN: That concludes my questioning.

CHAIRMAN JENSCH: In other words, his testimony remains as originally presented. Is that the summary of that?

MR. TROSTEN: It remains as originally presented, but I have the impression that the record might be left a little unclear by virtue of one question in which you asked Dr. McFadden to state the particular criteria that he wished to apply, and I didn't want the record at this point in the transcript to be obscure.

CHAIRMAN JENSCH: Yes. At this time, let's recess, to reconvene in this room at 2:45.

(Whereupon, at 2:25 p.m., the hearing recessed, to reconvene at 2:45 p.m.)

end Al 15

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AFTERNOON SESSION

2:45 P.M.

CHAIRMAN JENSCH: Please come to order.

Have you some further evidence to adduce before cross-examination of your witness?

MR. TROSTEN: We do not at this point in time, Mr. Chairman. I would like to make this observation. I regret that Dr. Stevens was not here. I didn't realize that the Board would want him here. If you wish to have him back, I feel confident we can get him, or if you have questions, I am sure we can provide a response in writing.

CHAIRMAN JENSCH: I really hoped he would hear Dr. Goodyear's answer to the criticism that Dr. Stevens had given, and I thought it might have been very helpful if Dr. Stevens could have heard it and would have desired to make a comment.

MR. TROSTEN: I think it would be helpful for Dr. Stevens to see the transcript in which Dr. Goodyear made certain observations, and we will furnish the transcript to him.

CHAIRMAN JENSCH: Very well.

Are your witnesses ready for cross-examination?

MR. TROSTEN: May Dr. McFadden be excused?

CHAIRMAN JENSCH: Dr. McFadden may be excused.

(Witness Excused.)

MR. TROSTEN: We are prepared for any cross-examination that the parties may have of Dr. Lawler or any

A1 16 Reba 2

questions that the Board may have of Dr. Lawler. I then have some redirect testimony to offer by Dr. Lawler.

CHAIRMAN JENSCH: Do you want to put the so-called redirect on now?

MR. TROSTEN: I think it would be easier if we have the cross-examination first.

Whereupon,

DR. JOHN P. LAWLER

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

CHAIRMAN JENSCH: Mr. MacBeth?

MR. MACBETH: Yes, I have some questions.

CROSS-EXAMINATION

BY MR. MACBETH:

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Q This is one the question about page 12, where, beginning on page 12, there is a lengthy quotation from the paper by Schaeffer. I simply wanted to get the reference for the Schaeffer paper.

A I had thought that was in my testimony, but apparently it isn't. It is the New York Fish and Game Journal, 1972, Volume 19, number 2.

MR. MACBETH: Thank you. That concludes my questions.

MR. BRIGGS: What volume was that?

THE WITNESS: 19.

MR.KARMAN: We have no questions, Mr. Chairman.

DR. GEYER: Dr. Lawler, referring to your April 20th submission entitled, "Response by John P. Lawler to Board Request On Striped Bass Matchery Stock Replenishment Estimates," on page 4 in the middle of the page you say, "Using our transport model for the 1967 flow conditions."

How representative are those -- I will put it this way. What are the flow conditions for 1967 representative of, the median, or the low range, or high range?

THE WITNESS: We did some analysis of that, Dr. Geyer, and the June and July flow for that period in 1967 was just about the median flow, 50 percent of the flows during that period would be equal to or greater than the ---

DR. GEYER: Median for those months, or median for the year?

THE WITNESS: Median for those months, for that period of time.

DR. GEYER: On page 6, table 2, under the next to the last two columns, the heading for which is "Impingement", what are the possible ranges in those numbers?

You have except in case 4 the same number all the way down.

THE WITNESS: Dr. Geyer, I wouldn't be able to put a range on the impingement numbers. These impingement numbers,

A1 16 Reba 4

I think I described in the October 30th testimony, and these are data obtained from Con Edison's staff people in the impingement on the fish screens at Indian Point 1 and then scaled up for the Indian Point 1 and 2 operations, according to flow.

I presume what you are asking for is a plus or minus range on impingement numbers. I wouldn't even want to guess at it, because I haven't analyzed it in that way.

I am just reminded that we do know that the numbers observed in 1972 were lower than the numbers that were used to generate the particular impingement estimates used in the model.

DR. GEYER: In your April 20th testimony entitled, "Contribution of Chesapeake Bay to Striped Bass Fishery in the Middle Atlantic States", on page 2 at the center of the page, is the argument here that neither hypothesis can be established, either that they do or that they don't come from the Chesapeake?

THE WITNESS: I think that what I would rather say is that it seems to me that the hypothesis that the Middle Atlantic fish that support the Middle Atlantic Fishery come from the Chesapeake, is one that I don't agree can be rejected.

I think that is about as far as I would like to go on this particular notion, at least in terms of the way I have presented it. I am not trying to say that the alternate hypothesis that they don't come from the Chesapeake isn't valid.

I think I have tried to make it as clear as I can here, simply saying, "Look, it seems to me based on a set of

calculations that the hypothesis that the major support or significant support of the Chesapeake -- of the Middle Atlantic Fishery can come from the Chesapeake Bay is simply one that you can't reject based on this analysis."

DR. GEYER: On available data and your analysis of it?

THE WITNESS: Right.

DR. GEYER: On page 6, about six lines up from the bottom, there is a phrase "Since age 4 and 5 fish have a low rate of exploitation in the Chesapeake". What is the basis for that? My opinion and my thought was that these age fish were exploited in the Chesapeake.

THE WITNESS: I am not sure just where you are. Oh, I see.

DR. GEYER: Six lines from the bottom on page 6.

THE WITNESS: Yes, sir. I think that is said on a relative basis. I think in the previous testimony to which this -- my own previous testimony which Dr. Goodyear responded to and in turn which I am responding to here -- I think brought out estimates of the exploitation of various ages in the Chesapeake, and as I recall, I had indicated that by and large the one pound, plus or minus, fish was the major size that would be exploited in the Chesapeake.

Therefore, the overall exploitation rate that one uses primarily of a fish of a given age and a given size, namely the smaller one pound fish.

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DR. GEYER: I am still confused. What is the weight of an age four and five fish, do you recollect? I am not sure.

THE WITNESS: As I recall, it is three and a half pounds, or thereabouts.

DR. GEYER: What I have seen from seeing rock in the market, it is the most common size caught in the Chesapeake.

THE WITNESS: I must confess to not being a resident in that area, but what I would gather from the various literature that I used to construct this analysis in the first place, it seemed that the roughly 12-inch, one-pound plus or minus fish was very popular, a very popular size, and by and large the largest exploitation was there. I am not suggesting for a moment that the other sizes aren't exploited. I had the impression that the one-pound or thereabout fish was the major portion of the catch in the Chesapeake.

DR. GEYER: I think we can resolve this, perhaps, if you can direct me to the place that is the basis for it.

THE WITNESS: It was a paper by Mansueti and Hollis, which was entitled "Striped Bass in Maryland Tidal Waters."

DR. GEYER: Mansueti is still down at the Chesapeake Biological Laboratory?

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THE WITNESS: Dr. Mansueti is dead. His wife is there, or his widow. She is remarried.

DR. GEYER: This is out of my field.

THE WITNESS: The only reason why I mention that is because his wife was the illustrator of all of the work that he did, and her name is quite well known in this field, too.

DR. GEYER: On page 7, under the quotation at the top of the page, the second line, it says, "About 50 percent of overage classes." Do you know what is meant by an overage class here?

THE WITNESS: I presume that what Dr. Goodyear was referring to was five years and older. That is what I was referring to.

DR. GEYER: Again a question about the basis for some figures on Table 1, page 9. The last few categories, age groups six and seven-plus, are indicated as all migrating out of the bay. What is the basis for that?

THE WITNESS: The basis for that was brought out in my previous testimony to which this refers, and it was Dr. Raney's testimony in December -- on December 8 in this hearing record -- to the effect that large striped bass have been captured and tagged in the Chesapeake this year, I believe, and this is in the Choptank River, and this was during the spawning season, and subsequent recapture of these

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1 fish had all taken place outside the Chesapeake.

2 DR. GEYER: If there is a tagging study basis

3 for this, I guess it is a legitimate number, but it doesn't

4 sound right to me, to expect that they would all leave the

5 bay.

6 THE WITNESS: I was relying on Dr. Raney's

7 testimony on this.

8 DR. GEYER: From personal experience, you catch

9 these large fish in the bay all year round, as far as I know.

10 THE WITNESS: I see what you are driving at. I

11 am suggesting here that at some time during the year,

12 probably a very large percentage of these fish move out of

13 the bay. As I say, the basis for that number was Dr.

14 Raney's testimony.

15 DR. GEYER: I will look up Dr. Raney's December

16 8 testimony and see if that shows the origin.

17 THE WITNESS: The transcript number for that

18 particular date is given in my testimony of February --

19 rather, my testimony of March 30, in Appendix D.

20 DR. GEYER: Thank you.

21 That is all I have.

22 CHAIRMAN JENSCH: Are there any further questions

23 of this witness?

24 MR. MACBETH: None, Mr. Chairman.

25 MR. KARMAN: No.

CHAIRMAN JENSCH: Do you have redirect?

MR. TROSTEN: Yes, we do. We are going to provide Dr. Geyer with the transcript reference now.

THE WITNESS: I am sorry. It is given here, but it is incorrectly. It says on footnote 1 on page D-17 that the testimony of the present hearing, October 30, 1972. Dr. Raney did submit testimony on that day, and there may be additional references there, so I won't say this is incorrect, but I specifically was relying on the comments that Dr. Raney made in his oral testimony on December 8, which was a Thursday. It was very close to the end of the day on Thursday, December 8.

CHAIRMAN JENSCH: If you can get the specific reference, would you send it in?

MR. TROSTEN: We will do that.

CHAIRMAN JENSCH: Proceed.

xxxxx REDIRECT EXAMINATION

BY MR. TROSTEN:

Q Dr. Lawler, do you have any additional comments to offer on the model used by ~~Lawler and Matusky~~ Clark Lawler with respect to striped bass impingement?

A Yes, I would like to comment on Dr. Goodyear's responses to Mr. Briggs' questions earlier this morning with specific reference to the question that Mr. Briggs asked Dr. Goodyear what he saw as to the differences

1 between the Applicant's model and the Staff's model, or
2 what I think, better, he saw as deficiencies or some such
3 word to that effect in the Applicant's model.
4 Dr. Goodyear first commented on the Applicant's
5 circulation and that in his mind the only difference
6 between circulation as it appears in the Staff's model and
7 by contrast to the way it appears in the Applicant's model
8 is that the circulation of the Staff's model is limited
9 to the saline zone wherein in the Applicant's model it is
10 not limited to the saline zone.
11 Several comments, I think, are in order. Dr.
12 Goodyear must be referring to the inclusion of longitudinal
13 dispersion in the Applicant's model, at least in the early
14 stages of the Applicant's model. There has been substantial
15 discussion of this in January, and I also made comments to
16 this effect in the testimony of April 20, entitled "The
17 Additional Testimony of John P. Lawler," on page 16 of that
18 testimony. I had offered some additional comments, and this
19 was one of them.
20 But my point here is, I think I don't agree
21 that the difference is simply in the Applicant's model
22 circulation appears outside the saline zone and in the
23 Staff's model circulation appears only in the saline zone.
24 The longitudinal conversion coefficient is not
25 circulation in the same way that the Staff's treatment of

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1 density-induced flows models it. It is true that the
2 longitudinal dispersion coefficient that we have used in
3 the saline zone does implicitly include the existence of
4 the density-induced circulation.
5 I would like to also state that we believe the
6 existence of a significant longitudinal dispersion outside
7 the saline zone, but still in the tidal zone has been
8 demonstrated in a number of past models that we have used.
9 In fact, I would find it quite difficult to model the Hudson
10 River without the inclusion of that term.
11 CHAIRMAN JENSCH: Excuse me. Dr. Goodyear, do
12 you feel you should comment in response to that?
13 WITNESS GOODYEAR: I think it was a pretty
14 accurate description of my understanding of the Applicant's
15 model.
16 CHAIRMAN JENSCH: You were referring to the
17 longitudinal dispersion?
18 WITNESS GOODYEAR: The effect of the longitudinal
19 dispersion. I was not indicating that the mechanism was
20 the same regarding how the organisms would be transported.
21 CHAIRMAN JENSCH: Thank you.
22 Will you proceed, Dr. Lawler?
23 WITNESS LAWLER: Yes, sir. The second observa-
24 tion Dr. Goodyear made was that in his mind the model does
25 not simulate observed behavior either in the Hudson River or in

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1 the San Joaquin.

2 I have two comments there in my October 30

3 testimony. I tried at some length to indicate that rather

4 than simply trying to simulate observed behavior in the

5 river, the model that we produced was capable of bringing

6 out information that the data collection program does not,

7 namely that the model addresses itself to all stages of

8 the life history of the fish, and in fact accounts for each

9 day in its life cycle whereas because of the activity

10 problem as indicated in the very tight range of lengths

11 of larvae, for example, that one finds, you simply don't

12 pick up all ages of the larvae.

13 So I tried to indicate I would not expect the

14 model to simply simulate the observed behavior without at

15 least recognizing these things, and I think I tried to point

16 out in the October 30 testimony that we, one, for example,

17 in the early larval stage focused on bringing out as output

18 the larval distribution of a given age bracket, say 10 to

19 20 days, you did get better agreement with the observations

20 in the estuary.

21 I think I also brought out the problems of gear

22 efficiency, where unless the ratio of the relative abundance

23 of eggs, the absolute abundance of eggs, was the same as the

24 relative abundance of larvae to the absolute abundance of

25 larvae, you would have a real problem trying to get agreement

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between the model and the observations.

The whole point was that I didn't look at it as a verification, but rather I think I posed that calibration would be a better word.

CHAIRMAN JENSCH: In other words, Dr. Goodyear was correct, but you expect some data to be developed by the use of your model that isn't yet developed?

THE WITNESS: No, I don't agree. I am trying to say that -- well, I don't really know what Dr. Goodyear means by the fact that it didn't simulate.

CHAIRMAN JENSCH: Let's ask him.

What did you mean, Dr. Goodyear?

WITNESS GOODYEAR: In both areas, there has been an observation of increased abundance of larvae and juvenile striped bass, and that is essentially the point that I was referring to, the concentration in the area of the salt front in both the San Joaquin and in the Hudson as observed by several different investigators.

CHAIRMAN JENSCH: Try that for size.

WITNESS LAWLER: Fine. I think I indicated that one way in which the model we used would very definitely give this effect would be the inclusion of differential mortality rates in various segments of the river.

Now, it is true that in general, in using the model, we did not use what I referred to as a differential mortality rate because I simply feel we don't have enough information to refine the mortalities that well at this time.

I am glad you mentioned the San Joaquin, because I am sure you didn't mean to imply that this particular model

has been used by anyone to simulate the San Joaquin.

WITNESS GOODYEAR: I didn't mean to imply that, no.

WITNESS LAWLER: The point that was brought up was that the model was stopped at an arbitrary point. We constructed the model down as far as mile .20, which is just below the so-called Ppermont Pier, which is the end of the Tappan Zee, and some people refer to the entire area as Haverstraw Bay, but really, below Croton Point it is known as the Tappan Zee and above there as Haverstraw Bay, and it is the widest section of the river, and extends to about mile .20.

The reason for stopping the model at that point is because we use the Carlson-McCann data to guide us in the construction of the model, and Carlson-McCann data didn't indicate the existence of eggs and larvae down below mile .20. We used as a boundary position that there were no eggs and larvae below that point, and furthermore, did not permit eggs or larvae to move out of the estuary, that is, below mile .20.

It is conceivable that there may be some downstream movement beyond this point.

If that were included, I suggest that the distributions would be even flatter than were used, and the effect of the plant would therefore be lower.

On the question of migration related to fish sampling data, we did that because we feel for those sizes

of the fish, it just simply isn't known what makes them move, how much of their movment is due to the downstream motion of the river, and how much is due to their own desires to go to particular places in the estuary, so what we used was the observed distributions.

I think that is the sum and substance of my comments on the model.

CHAIRMAN JENSCH: Is there any interrogation?

MR. MACBETH: No questions.

CHAIRMAN JENSCH: Regulatory staff?

MR. KARMAN: No questions.

CHAIRMAN JENSCH: Any further presentations?

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: Proceed.

BY MR. TROSTEN:

Q Dr. Lawler, Dr. Goodyear, in his testimony this morning, indicated that data used in Table 6 of your testimony of April 20, 1973, concerning striped bass hatchery stock was not an appropriate -- was not appropriate to use in connection with the staff's estimate of multiplant effects that were presented in Dr. Goodyear's testimony of February 8, 1973.

Do you have any comments to offer in this respect?

A Yes, I do.

I certainly would hope that Dr. Goodyear would correct it if he believes this interpretation is incorrect,

but first of all, it is important because this information was used not only in this document, but also in a subsequent table. It was submitted yesterday.

When we received the testimony on February 8, 1973, dealing with the effect -- the staff's estimate of the effect -- of the multiple plant, or plants, on the river, we immediately requested from the staff the input and the output data for those, or whatever computer runs were made to generate the percentage losses that are given in the staff's February 8 testimony.

The information that we received is the information that is plotted in Table 6.

Furthermore, the numbers that one obtains when determining the percentage reduction between the Indian Point 1 and 2 plants as well as the combined multiple effect are identical to the percentage reductions that are given in the February 8 testimony.

So, I really -- if I have misinterpreted what that February 8 testimony was supposed to imply, I would like to know about it.

CHAIRMAN JENSCH: Dr. Goodyear, would you give us a response?

WITNESS GOODYEAR: Just the point that I was trying to bring out this morning, was that you should have received two sets of output, one of which contained all of the

rusn that were included in the table.

The other should have contained the same years but not all of the cases which were tallied.

I may be mistaken on that, but the inclusion of columns entitled "Recruits to Year Class 1" in Table 6 does not reflect my use of the model. It is based on a carrying capacity estimate of about 50 pounds per acre of the shoal, and that is not considered to be realistic, nor is the method of cropping that is applied in the model to obtain the values listed in the recruits year class 1.

That portion of the model itself, the computer program, is not present in the runs which were made and utilized in preparing the table that was submitted in the testimony.

WITNESS LAWLER: Well, let me put it this way: In Table 6 of my testimony, of February 20, which is entitled "A response to the Board's Request on Striped Bass Hatchery Stock Replenishment Estimate" --

MR. TROSTEN: April 20.

MR. KARMAN: April 20.

WITNESS LAWLER: I am sorry. April 20.

If you take the number entering the nursery for the case of Indian Point Unit 1 and 2, and ratio that and take the difference in that from the same column under the title "No plants," you get a difference of 3,700,000 fish, and that

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is what we proposed in this analysis as the hatchery replacement.

When you ratio that number to the total of 13,900,000 fish entering the nursery, you get a value of 26.7 percent, which agrees with the 28.8 that was in the testimony.

So all I am trying to say is what we were asked to do here was to make comparisons using both the estimates made by the staff as well as our own estimates on the population and the number of recruits that would be necessary to make up for any projected losses.

I don't hear you suggesting anything other than the numbers that I have used here. These do, in fact, reflect the runs summarized in the February 8 testimony.

WITNESS GOODYEAR: Yes, they do, except the output is not to be considered as anything except a relative number, rather than an absolute estimate of the size of the population.

WITNESS LAWLER: All right, fine.

end 18

MR. TROSTEN: Dr. Lawler, do you have any points of clarification to offer concerning the table that was submitted yesterday under your sponsorship and that of Dr. Stevens following the transcript page 11,115, which is entitled "Comparison Between Number of Mature Females Required for Hatchery Replenishment Computed Using the Applicant's and Staff's Models"?

WITNESS LAWLER: Yes. I have several comments which I think are appropriate because of the rapidity with which this table was put together and the fact that it does relate to the testimony that we were describing a moment ago, namely, the April 20, 1973 testimony entitled "A Response by John P. Lawler to the Board's Request on Striped Bass Hatchery Stock Replenishment Estiomate."

What we tried to do here was to take the estimates of the percentage reduction as well as population in the estuary made by the Applicant as well as by the Staff, and then convert that to the number of female brood stock that would be necessary to make up for the projected number of lost two-inch fingerlings.

The assumptions that are listed are the assumptions that Dr. Stevens testified to yesterday with the exception ofthe total number of eggs per female of 600,000.

There was, I think I indicated yesterday, that

1 this would be comparable to a 10-pound fish. Our testimony
2 of October 30, 1973, deals with the fecundity -- rather, 1972 --
3 the fecundity values that were applied to each age of the
4 striped bass from age 4 through age 13. The 600,000 simply
5 represents a weighted average of all the females through the
6 entire range of age that would be contributing to the
7 spawn in the Hudson River each year.

8 The 270,000 viable eggs per female simply repre-
9 sents Dr. Stevens' estimate that some 45 percent of the eggs
10 that might be stripped from the female would essentially
11 hatch, and the hatchery efficiency of 25 percent is the
12 value that Dr. Stevens referred to yesterday.

13 The combination of all these numbers yields
14 67,000 two-inch fingerlings per female striped bass, which
15 corresponds to roughly a requirement of 15 female striped
16 bass per million two-inch fingerlings, adn the numbers in
17 the table are obtained by taking Table 7 in the testimony
18 I referred to a moment ago in our response to the Board's
19 request on striped bass hatchery stock replenishment estimate,
20 and dividing by the rate of 15 striped bass per million
21 fingerlings.

22 The three cases used are described as the impact
23 paramters used by the Applicant, which are F factors and
24 compensation factors that were used and presented in earlier
25 testimony, the impact parameters approximating the STaff's

1 input conditions reflecting the so-called runs 14 and 16
2 of earlier testimony which Mr. Briggs had asked us to
3 respond to, and these reflect all F factors equal to 1 and no
4 compensation.
5 The Staff's model refers to the numbers I was
6 disucssing a moment ago in Table 6 of the April 20 response
7 on the hatchery stock replenishment estimate.
8 The numbers 150 to 225 simply reflect the Staff's
9 estimate of 10 million to 15 million fingerlings. Dr.
10 Goodyear commented on that to some extent yesterday. It
11 is probably more appropriate to place the 225 number in the
12 row entitled "Indian Point, Bowline, and Roseton."
13 While I am on that topic, I was going to -- well,
14 I will hold that for a moment.
15 Well, let me bring it out right now. This 225
16 females per, or to replenish the estimate, is based
17 on the estimate of 15 million fingerling fish requirement
18 that Dr. Goodyear has introduced and discussed at some
19 length this morning. I am not sure whether it is quite
20 appropriate to make an estimate of 30 million 7 - 10 inch
21 fish and then suggest that the complications of gear
22 selectivity and gear efficiency and several other factors
23 that were mentioned are offsetting to the effect that there
24 is still a requirement of 30 million striped bass fingerlings,
25 or an estimate, a population estimate of 30 million striped

1 bass fingerlings in the river.

2 I think it would be more appropriate to scale

3 down to the order of 4- to 6 million as Mr. Briggs suggested

4 this morning would be found if you used either Mr. Clark's

5 testimony or our own modeling effort, the population estimates

6 for the two-inch fish are roughly the same using Mr.

7 Clark's survival curves or our own population predictions that

8 come out of the model, and then from that point recognizing

9 the problem of gear efficiency and gear selectivity.

10 I have some problems with this gear efficiency

11 and gear selectivity problem.

12 First of all, the word "efficiency" has been

13 used usually, and gear efficiency refers to the fact that

14 in your effort for sampling for a certain segment of the

15 population you only succeed in getting a certain relative

16 amount of that segment.

17 The word "selectivity" refers to the fact that

18 in any of your sampling gear, you are never sampling for

19 the entire population over the entire gamut of ages, but

20 you are in fact sampling only for a portion of that population.

21 So I am reluctant to apply any factor for gear

22 efficiency at this moment, although I do agree that, as has

23 been indicated any number of times in the hearing that the

24 problem of gear efficiency is one that suggests that the

25 populations are probably higher than the relative abundances

would indicate.

At the bottom of this table that was introduced yesterday, there is a note that says "54 percent of the table values would be needed if we were to use the Staff's estimate of 500,000 viable eggs per female."

That was inserted because in the listing of the assumptions, and I think it is Set 1, the responses on the part of the Staff to the Applicant's interrogatories, the word "viable eggs" was used.

Dr. Goodyear subsequently testified that the 500,000 would be a total eggs per female rather than a viable fertilized ~~egg~~ eggs.

So that note would not be appropriate unless anyone is estimating that there are 500,000 viable eggs per female.

One thing I think would be -- well, if you give me a moment, I want to check my notes and see if there is anything else.

(Pause.)

MR. TROSTEN: Dr. Lawler, do you have any supplemental comments on the range of reduction of young of the year striped bass reported by the Staff in its February 8, 1973 testimony and later reiterated in Dr. ~~Eastman's~~ Ostlmann's testimony of April 6, 1973, wherein it was stated that the estimated reduction in striped bass young

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1 of the year ranges between 14 to 43 percent with only
2 Indian Point Units No. 1 and 2 in operation?
3 WITNESS LAWLER: Those ranges of the estimated
4 effect of the Indian Point 1 and 2 on the striped bass young
5 of the year population were given in Dr. Goodyear's February
6 8 testimony on the so-called multiple plant effect.
7 There were a series of some six or seven conditions
8 given, all for different conditions of flow on the Hudson
9 River.
10 We took those values and converted them to a
11 probability distribution based on the probability distri-
12 bution of flows in the Hudson River during the entrainment
13 period of the year.
14 We find that the median value, the 50 percent
15 value, which I think is a pertinent value because we are
16 talking here about not simply a single year's effect, but
17 an effect that might take place over a number of years.
18 So I think it is important to evaluate the
19 impact not in terms of a single high year flow or low year
20 flow, but in terms of an average over a period of time.
21 The 50 percent flow yields a percentage reduction
22 of some 27 percent, using the Staff's model. In other words,
23 if you take those conditions from 14 to 43 percent and you
24 put them on a probability distribution based on the flows
25 that each one corresponds to, you find the median percentage

reduction value on a yearly basis is 27 percent.

MR. TROSTEN: Dr. Lawler, do you have any other testimony to offer at this time?

WITNESS LAWLER: Just one correction in the piece of earlier testimony that I think is important, and it only came to our attention several days ago.

We inserted in the record on February 5, 1973, a document entitled "The Sensitivity of the Applicant's Model." This was in response, Dr. Geyer, to a question of yours in December.

Inthat table, we summarized -- in that document we summarized -- a series of runs which we felt best reflected the Applicant's belief as to what might be occurring in the river.

Three runs were used, and, Mr. Briggs, you will recall that you questioned me on these. The two that you questioned me on in detail are correct as stated, but the other one which shows as Run 2 should really be Run 1, and there are some slight changes in the percentage numbers on that run, if we can get the page.

This is page 12 in the testimony referred to, and Run 2 should be replaced by Run 1, and the ~~loses~~ losses in the first year of the young of the year rather than ~~2.7~~ 2.1 percent are ~~3.7~~ 2.3 percent, and the 10-year adult value rather than being 3.9 percent should be 4.9 percent.

MR. TROSTEN: This document appears following transcript page 9405 of the March 5, 1973 transcript.

WITNESS LAWLER: This introduces a slight lifting of the curve on the middle section of the curve on the following page. This is the curve where we try to relate the rate at which compensation occurred; and what happens to that curve when you put in these new numbers is that the curve flattens out in the middle, which brings out to a little greater extent the point I was trying to make about the curve.

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CHAIRMAN JENSCH: Will the Applicant undertake to see that there are corrections made in the original transcript, and also the Staff, and the Hudson River Fishermens Association, if you think that makes a significant change in your original presentation. Will you submit revisions? Otherwise, there may be a separate reconsideration of the matter. I don't know what the effect of your change is, whether it is substantial or not, but I think the parties should have an opportunity to review the matter, and if they believe it is of significant import that you revise your curve of your graph or whatever it is as indicated.

Will you do that?

THE WITNESS: I will do that.

CHAIRMAN JENSCH: Does that finish your questions?

MR. TROSTEN: Yes, it does.

CHAIRMAN JENSCH: Regulatory Staff?

MR. KARMAN: I have no questions.

CHAIRMAN JENSCH: Hudson River Fishermens Association?

MR. MACBETH: I would like to put a question to Dr. Goodyear. I have none of Dr. Lawler.

CHAIRMAN JENSCH: Proceed.

MR. MACBETH: I thought I understood in questioning you this morning or yesterday that it was your position that Indian Point 1 and 2 would in years of medium flow take forty percent, plus or minus ten percent, of the annual production

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Reba 2

of striped bass in the Hudson, and that that was so both in light of the evidence presented in the Final Environmental Statement and the additional testimony on the multiplant effects presented in February.

Am I correct in that statement?

MR. GOODYEAR: Yes.

MR. MACBETH: Has anything Dr. Lawler has just said on redirect changed your opinion on that matter?

MR. GOODYEAR: The conclusions that he just presented don't correspond to my interpretation of the flow data. So I really can't -- it does not presently, no.

MR. MACBETH: Thank you. I have no further questions.

MR. TROSTEN: I guess I have to ask Dr. Goodyear a question. Is it, or is it not the Staff's testimony that the reduction in striped bass young-of-the-year ranges between 14 to 43 percent with only Indian Point 1 and 2 in operation? If the answer to that is yes, I do not understand how this is consistent with your answer to Mr. MacBeth's question just a moment ago.

MR. GOODYEAR: The difference between the two is that the 40 percent has a confidence interval around it of plus or minus 10 percent.

MR. TROSTEN: Does the 14 percent have a competence interval associated with it?

MR. GOODYEAR: Yes, it would.

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MR. TROSTEN: Is that plus or minus ten percent?

MR. GOODYEAR: I have not evaluated that in that regard. I would suspect it would be roughly equivalent from the percentage point of view. In other words, the range would be half of the estimate of the size.

MR. TROSTEN: So it might be plus or minus 7 percent, roughly?

MR. GOODYEAR: Something like that.

MR. TROSTEN: Well, I perhaps just don't understand, but I simply don't comprehend why, because there is a confidence interval around the 43 percent number, and around the 14 percent number, that that means that the number of striped bass, that the reduction, rather, would be 40 percent plus or minus 10 percent.

You said that it ranges from 14 to 43 percent. If I don't understand, could you explain that to me?

MR. GOODYEAR: I thought that the question that Mr. MacBeth asked me was directed at specifically a median flow year.

MR. MACBETH: It was.

MR. GOODYEAR: We seem to have some difference in interpretation of what a median flow year consists of.

MR. TROSTEN: May we have a brief recess?

CHAIRMAN JENSCH: At this time, let's recess to reconvene in this room at 3:55.

(Recess)

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CHAIRMAN JENSCH: Please come to order.

Are there more supplemental comments or additional statements? Would you like a further recess?

MR. TROSTEN: We were not quite finished, Mr. Chairman, if you could just give me a moment.

Dr. Goodyear, would you explain how you computed "median flow year" as you used that term?

MR. GOODYEAR: The median flow year I was referring to was made by listing the data which I had available. I believe it is from Geise and Barr for each of three months, May, June and July, and obtained the median value for each of those three months in preparing the values which I had used and had available to me for the periods I ran the model.

MR. TROSTEN: Dr. Lawler, do you have any comment on Dr. Goodyear's computation of the median flow year?

DR. LAWLER: I have a comment on the whole question. Apparently Dr. Goodyear has testified earlier in the day that the percentage reduction of the Indian Point plant using the Staff's model corresponding to the median flow year is 40 percent.

I have just indicated that ---

CHAIRMAN JENSCH: Are you using your microphone, and is it on?

MR. LAWLER: I am sorry. I have just indicated that when we took the median flow year in the Hudson River, again

1 using the Staff's model we obtain a reduction of 27 percent.
2 The difference is pretty clear, and it seems to be in the
3 definition of median flows.
4 Dr. Goodyear appears to have taken the median flow
5 for the month of May and then the median flow for the month
6 of June and then the median flow for the month of July. We
7 have done something different. We have taken the median flow
8 for the two-month period June through July. The reason for
9 doing this is that, I think in doing the probability distri-
10 bution you have to look at the -- for any given year that goes
11 into the probability distribution -- you use the entire period
12 over which the entrainment takes place.
13 In the analysis we performed, we did not use May
14 because our experience has been that the response of the salt
15 front to flow in the river is rather rapid, and there is quite
16 a fluctuation of flow inthe river in May, but the location of
17 the salt front in June and July would be much more a function
18 of the flows in June and July than it would be of the flows in
19 May.
20 So it is clear where the difference is.
21 MR. GOODYEAR: I would like to offer one comment.
22 The location of the salt front is very much, of course, dependent
23 on flow, but so is the distribution of the larvae and juveniles,
24 and the portion of larvae which have been washed downstream
25 with the flow at the time that the salt front begins to stabilize

Al Reba 6 is the critical factor in determining what proportion on a
2 year -- it is a critical factor for a particular year if the
3 salt front ends up in the region of Indian Point, or just above
4 Indian Point.
5 I think that particular feature is not adequately
6 incorporated in flows for just June and July.
7 MR. LAWLER: I am not so sure it wouldn't be, because
8 the distribution of the spawn, it seems to me, begins in May
9 and not in the beginning of May, but in the early part of May,
10 and only begins to reach a peak in the later part of May.
11 So I am not particularly sure that the inclusion
12 of May flows is that important to the question we are discussing.
13 CHAIRMAN JENSCH: Did you happen to figure it
14 including May and then without it?
15 MR. LAWLER: No, sir.
16 CHAIRMAN JENSCH: You didn't use May at all. You
17 don't know how it would have come out if you had used May for
18 your calculations?
19 MR. LAWLER: No.
20 CHAIRMAN JENSCH: Do you have anything further?
21 MR. TROSTEN: We have a factual stipulation.
22 CHAIRMAN JENSCH: Is it all typed out?
23 MR. SACK: No, it is not typed out. This relates
24 to the answer to the interrogatories of the Hudson River
25 Fishermens Association which was submitted by Mr. Schwartz,

20 Reba 7

and was placed in evidence by Mr. MacBeth.

Tables 1-A-1, 2, and 3 omit data for weekends and holdays because records are not kept for those days.

Loads are substantially less on these days and Con Edison takes advantage of this load reduction to perform short-term maintenance.

CHAIRMAN JENSCH: What do you mean by "short-term maintenance"?

Clean off the fish?

MR. SACK: No, these are of other plants. We are talking about system loads. The loads for the system as a whole are lower on weekends and holidays, and during the summer season. This is a summer as defined for system planning purposes. During that season of the year, all our plants are going at maximum percent possible during the week time, and if anything goes wrong in any plant, we try to defer that maintenance to the weekend and take advantage of the reduced loads to perform that maintenance at that time.

CHAIRMAN JENSCH: You say records are not kept. What records, records of what are not kept?

MR. SACK: These are the records that would appear on tables 1-A-1, 2 and 3 of the response to interrogatories of Mr. Schwartz. The response of Mr. Schwartz, rather, to the interrogatories of Hudson River Fishermens Association.

CHAIRMAN JENSCH: Advance load forecast data, is

that correct?

MR. MACBETH: Yes.

CHAIRMAN JENSCH: You don't have any record for weekends for load forecasts, is that correct?

MR. SACK: For the daily load forecast for weekends and holidays.

CHAIRMAN JENSCH: Why don't you have those?

MR. SACK: They are not kept. We are not required to keep them and for some reason they are not kept. The forecast is made by the daily operator, and I understand and have been informed that they are made, but the records are not kept.

CHAIRMAN JENSCH: I understand. You said that. It seems to be puzzling that you go along and don't do it. Maybe there is a reason, and if you could give us a reason that would be helpful.

MR. SACK: It is not within my area of knowledge or expertise as to what the precise reason is.

CHAIRMAN JENSCH: I see. Is there anything further?

MR. TROSTEN: Mr. Chairman, we have no further evidentiary presentation to make. If it would be satisfactory to the Board, I would like to have Dr. Lawler excused. We are prepared for closing argument and other matters appropriate.

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1 CHAIRMAN JENSCH: I don't expect any closing
2 arguments right now.
3 MR. KARMAN: Could we have Dr. Goodyear excused
4 as well?
5 CHAIRMAN JENSCH: Yes. Dr. Lawler, you are
6 excused.
7 Dr. Goodyear, you are excused.
8 (Witnesses Goodyear and Lawler
9 excused.)
10 CHAIRMAN JENSCH: Is there further evidence?
11 MR. MACBETH: One further matter. On Tuesday
12 afternoon, Applicants asked Mr. Clark for a reference to
13 a paper by Norman Vick, and I wanted to report that that
14 was "Summarized Report of the Striped Bass Stocking Project
15 in Choctawatchee Bay, Florida," by Norman Vick, dated March,
16 1969, and a copy has been shown to the Applicant, was
17 shown to him yesterday.
18 CHAIRMAN JENSCH: There being no further
19 evidence to be presented in this expedited hearing, we are
20 ready to come to the proposed findings and conclusions.
21 MR. TROSTEN: Yes.
22 CHAIRMAN JENSCH: I don't know that I have the
23 latest, but I have a regulation about the time schedule
24 which I don't believe is absolutely mandatory if good cause
25 is shown. There is an allowance of discretion to change

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1 the schedule for filing according to the necessities of the
2 case.
3 One of the things that I think we would like
4 to have covered in the findings by all parties, and
5 particularly by the Applicant is a reference to the proposal
6 by the Applicant for some time to further study the Hudson
7 River, and in connection with that, did I understand
8 correctly, as we may have inferred from Mr. Woodbury's
9 statement, that there is a commitment on the part of
10 Consolidated Edison to undertake a fishery stocking program
11 now in the Hudson River?
12 MR. TROSTEN: Well --
13 CHAIRMAN JENSCH: What is the situation about
14 the fishery stocking?
15 MR. TROSTEN: The situation on that, Mr.
16 Chairman, is that for a considerable period of time the
17 Applicant has stated its willingness to replace striped
18 bass, which have been removed by impingement, or entrainment,
19 if this is deemed to be necessary and desirable in order
20 to replace losses.
21 This statement has been made in various ways.
22 There is no regulatory requirement at the present time
23 imposed by any federal or state agency that the Applicant
24 do this.
25 CHAIRMAN JENSCH: The only question is, is there a

1 commitment to restock the fish, yes or no?

2 MR. TROSTEN: In terms that I have just stated,

3 the answer is yes.

4 CHAIRMAN JENSCH: Very well.

5 MR. BRIGGS: There seemed to be some question

6 yesterday as to whether Dr. Stevens' company had made a

7 proposal and the proposal had been accepted for doing work

8 on restocking or whether the proposal had not been accepted.

9 MR. TROSTEN: It is my understanding that the

10 proposal has been accepted and that that work is going to be

11 done, and if there is any change in that, we will inform

12 the Board, Mr. Briggs.

13 CHAIRMAN JENSCH: One thing that I said the

14 Board is concerned about in reference to this proposed study

15 that the Applicant seeks to undertake. The Staff has proposed

16 in this proceeding that cooling towers, or rather that

17 some method other than once-through cooling be applied,

18 and presumably that would be done through cooling towers,

19 although I guess the option is open to adopt any other method

20 that the Applicant would find equally efficient as an

21 alternative to once-through cooling.

22 But with the abundance of data or proposals

23 about this program and how minutely the work would be under-

24 taken, the Board requests the Applicant to commit itself in

25 this proceeding to what standards or what criteria would be

1 met, or which standards and criteria would require, based
2 upon the evidence presented here, adduced from the program,
3 to compel the construction of cooling towers or some
4 alternative to once-through cooling.
5 Now, without such a commitment from the Applicant,
6 it seems that the whole proceeding would start over again
7 whenever the study is deemed to be concluded, and I am not
8 too sure that the time limit for a study has been firmly
9 committed by the Applicant in that regard. The Staff at one
10 time had suggested that a certain period be allowed, and I
11 think the Applicant has some alternative, but if there are
12 not some standards or criteria developed in this proceeding,
13 it seems that then the Staff would be in the position of
14 endeavoring to present a position after the collection of
15 data which would justify the construction of something as
16 an alternative to once-through cooling, and that burden
17 we do not feel should belong to the Staff, but, rather, it
18 should be on the Applicant, reflected by a commitment by
19 the Applicant's standards or criteria, which would compel
20 the construction of some alternative to once-through cooling.
21 In connection with that, you can indicate the
22 type of data which would fit those criteria or standards,
23 and the specific character, the specific nature of what
24 kind of data would fit those standards or criteria. It would
25 be very helpful for consideration by the Board.

We think that the present posture of this proceeding is that a delay to the construction of any once-through cooling automatically throws the burden on the Staff to urge or require its adoption at a later time, similar to the type of regulation that the Commission had.

In this respect, however, we believe the situation is different because we do have a causative factor of damage by entrainment or impingement, the extents of which may require the balancing to which Applicant's counsel referred this morning.

What is the suggestion of the Applicant as to the time in which it will be prepared to submit its proposed findings and conclusions together with a brief, with specific reference to transcript pages in support of its assertions for proposed findings and conclusions?

MR. TROSTEN: We would propose the following schedule, Mr. Chairman.

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CHAIRMAN JENSCH: Take just your own schedule for the moment. We will ask the parties for theirs.

MR. TROSTEN: Well, of just those things that we would file?

CHAIRMAN JENSCH: First, you do have an opportunity for reply. We would like to get that date later, too.

MR. TROSTEN: All right.

We would propose that -- well, if it would be satisfactory to the Chairman, it is a little difficult for me to propose this just by giving our aspects of this. May I give the whole thing at once?

CHAIRMAN JENSCH: We will come back to your comments later, but tive us your date. When will you be ready with your initial proposal and conclusions?

MR. TROSTEN: We would propose, Mr. Chairman, that the following schedule for the submission of our radiological findings and conclusions and our environmental findings and conclusions, which are on different dates -- all of this being predicated upon the assumption that the entire matter of findings and conclusions -- that the complete presentation is made to the Board by the first of June.

CHAIRMAN JENSCH: Let's mot go that far.

MR. TROSTEN: Our request is that we would file our findings and conclusions on radiological matters on the 7th of May, and that we would file our environmental findings

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on the 17th of May.

CHAIRMAN JENSCH: I have difficulty with the compartmentalizing of the radiological and environmental. The environmental policy act speaks of having the final environmental statement accompany the proceeding throughout the entire process, and somehow, these things get so interrelated that I think they should be combined.

MR. TROSTEN: Mr. Chairman, there is no thought on our part that the entire process would not culminate at one time. It is simply that, very frankly, the environmental findings are far more difficult to prepare and more complex than the radiological findings, and there seems to us to be no reason why, since we feel confident we can prepare the radiological findings and any necessary briefs in advance, why we should delay submitting the radiological findings.

CHAIRMAN JENSCH: Oh, you can send them in any time you want to, but I think they have to be interrelated, and I think you will find that the environmental should be related to your radiological even if you send in the radiological ahead of time.

You may prefer to have two submissions, but there should be an interrelationship.

If there is any separation begween radiological and environemtal concerns, I think you will accept that there is not a separation, referring to the environmental policy act.

1 MR. TROSTEN: I don't mean to dispute the Chairman,
2 but there are findings that re required to be made under the
3 Atomic Energy Act, and those which need to be made under
4 Appendix D of Part 50, and it seemed more appropriate that
5 we do this.
6 CHAIRMAN JENSCH: Applicant should feel free to
7 porceed as it desires.
8 It may complicate the consideration of the matter,
9 but if that is the way the applicant believes it can best
10 present its case, we will try to take it as well as we can
11 from that point of view.
12 MR. TROSTEN: May I offer a further explanation
13 of why we are doing this?
14 If we are operating as we feel it is entirely
15 appropriate that we operate, to get the matter concluded
16 at one time, if we delay submitting the radiological matters
17 until the environmental findings are all prepared, that means
18 that to the extent that there are complications arising associated
19 with the radiological findings and conclusions and the response
20 by other parties, all of that will have to be addressed in a
21 very, very foreshortened time by everything.
22 CHAIRMAN JENSCH: The time for the other parties to
23 respond and the submission of proposed findings and conclusions
24 will be after the complete submission by the applicant.
25 If you have two submissions, one on radiological

and one on environmental, the time for other proposed findings will begin from the time of your latter presentation.

MR. TROSTEN: You mean for everything on all matters?

CHAIRMAN JENSCH: We will take up the intervenor next and their time for submission can be measured by the regulation or such showing of good cause as they present in reference to the preparation of findings, and we will start from and after May 17, 1973.

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1 MR. MACBETH: Mr. Chairman, I don't have the
2 regulation in front of me, but it is my memory that it
3 would allow twenty days.
4 Well, Mr. Karman has just handed me a memo which
5 says twenty-five days.
6 MR. TROSTEN: After the record is closed.
7 MR. KARMAN: Fifteen.
8 MR. MACBETH: Excuse me. Let me be perfectly
9 blunt about what I would like in that case. What I would
10 like is twenty-five days from the time the Applicant has
11 presented its proposed findings and conclusions.
12 The case is extremely complicated. The record
13 is very long. There has been testimony running literally
14 into thousands upon thousands of pages from all three of the
15 major parties in the proceeding.
16 I will be working on it virutally alone. It
17 will be an enormous job, and I don't think that in that
18 situation a request for twenty-five days from the time I
19 have the Applicant's proposed findings and conclusions in
20 hand is in any way excessive.
21 CHAIRMAN JENSCH: Are you speaking for the
22 Citizens' Committee and the Environmental Defense Fund as
23 well?
24 MR. MACBETH: Mr. Roisman told me that would be
25 sufficient for him as well, for both of them.

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1 CHAIRMAN JENSCH: How do you compute that date
2 from May 17? If they are served by mail, you add a couple
3 of days and you come to a weekend and you skip that, and
4 start counting. Do you have a figure?
5 MR. MACBETH: I don't have a calendar in front
6 of me but adding three days to the 20th would bring us out
7 on June 14.
8 CHAIRMAN JENSCH: Very well. June 14 is fixed
9 as the date.
10 MR. TROSTEN: Mr. Chairman, are you going to allow
11 me to comment on these before you fix the date?
12 CHAIRMAN JENSCH: Yes, surely.
13 MR. TROSTEN: Shall we wait until all the
14 presentations are made?
15 CHAIRMAN JENSCH: Do you have an objection to
16 their having twenty-five days?
17 MR. TROSTEN: Yes, sir, I do.
18 CHAIRMAN JENSCH: Why is that?
19 MR. TROSTEN: First of all, it is inconsistent
20 with the regulations. The regulations allow a period of
21 ten days. The fixed period that we are talking about is
22 ten days after the other party, after the Applicant in this
23 case, has filed his findings. So that would say that if
24 we were to file on the 17th, that if they were given the time
25 allowed by the regulations, that that would be no later than

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while it doesn't perhaps relate directly to the regulations, but to the circumstance surrounding the Applicant -- there have been two occasions in the past when the Applicant has pushed for early resolution of matters to have decisions rendered immediately so that there would be no delay, and the wheels could start turning just as soon as possible, and on those two occasions, the Board has undertaken extra effort to accommodate that urgent request.

In neither instance has the Applicant been able to meet the projected schedule of readiness and urgency that had been urged upon the Board.

At the present time I think there is some question about the testing program that was authorized in July of 1972. I don't think that may have started with a 20 percent allowance for authority. I think that some pending requests are for a higher amount.

MR. KARMAN: As a matter of fact, Mr. Chairman, the Appeal Board came down with a decision on monitoring the 30 percent. A testing license was issued last week for 20 percent.

CHAIRMAN JENSCH: And they want 100 days to run through some tests. So that may extend over six months, and they will stop and start.

So I think that as far as the delay to the Applicant, the consideration that we should be mindful of and

1 the 27th of May.

2 CHAIRMAN JENSCH: Yes, unless for good cause the
3 time is extended, and I think when a case has been going for
4 a couple of years and we have had additional amounts of
5 testimony introduced even in this last session, which I
6 dare say in view of its late introduction will require some
7 time for evaluation other than the hurried review that has
8 been given in order to carry on cross-examination at this
9 session, that the complexity of the case and in view of the
10 request by the Applicant to delay the day for consideration
11 of alternative cooling methods requires something more than
12 the ordinary approach to a scheduled time.

13 I think the Commission expects that there will
14 not be a rigid schedule applied if circumstances warrant
15 more time, and I think what the circumstances are depend
16 upon the complexity of the case, the length of the case, the
17 difficulty and the analyses that must be undertaken, and I
18 guess also the relative position of the parties in the
19 preparation.

20 These are all factors, I think, that have been
21 reflected inthe course of this proceeding, and I think there
22 has been a general willingness by all parties to this
23 proceeding to expedite these considerations of this matter
24 in every way that circumstances permitted.

25 Of course, there is an additional factor that

1 the circumstances surrounding the Applicant's necessities
2 are things that we must keep in mind, but certainly the
3 record of the Applicant in this proceeding is not one
4 that seems to encourage rigid compliance with a time schedule
5 because on two occasions, as I mentioned, the Applicant,
6 despite his ernest entreaties, has never met its own
7 projections.
8 So we don't feel that there is any prejudice
9 befalling the Applicant. We think that is one phase to be
10 considered in setting a time schedule for proposed findings.
11 Whether that is part of the due cause consideration or not,
12 I think the due cause situation is generahly related to the
13 request by the person submitting it, but it is a factor that
14 wouldn't be overlooked.
15 The Applicant will not be prejudiced, will not be
16 harmed, will not be delayed if a few more days are given for
17 adequate consideration of these very extended matters.
18 Do you not agree?
19 MR. TROSTEN: No, sir, I am afraid I cannot agree.
20 In the first place, the delays that have occurred in the
21 past which are well set forth on the record such as the
22 matter of replacing the core were certainly not things that
23 the Applicant could be held responsible for. This has to do
24 with a fuel densification problem which was a problem that
25 arose in the nuclear industry which required as a matter of

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1 prudence the replacement of the core.

2 Had that core not been replaced, the July 14

3 decision could have been implemented very promptly. So I

4 don't really think that should be something held against

5 the Applicant in this particular case.

6 CHAIRMAN JENSCH: Were you directed to replace

7 your core?

8 MR. TROSTEN: No, but we felt it was in the public

9 interest --

10 CHAIRMAN JENSCH: There is no criticism of your

11 doing it. Maybe you could have gone ahead.

12 MR. TROSTEN: Perhaps so, Mr. Chairman. It

13 seemed to be an appropriate thing to do in the overall

14 interest.

15 CHAIRMAN JENSCH: No criticism about it at all.

16 I just was inquiring.

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MR. TROSTEN: With regard to the timing of the Board's consideration of this, Mr. Chairman, our view of this is really very simple. It is quite correct that we have stated a schedule of 49 days for testing up to 50 percent at full power, and we have said as a matter of prudence that this should be doubled to a hundred days. We feel it would be unwise and contrary to public interest and a poor procedure to follow to fail to take into account the possibility that we would be able to complete this testing on the ideal schedule that we have projected.

We have said it is ideal, it is true. We feel that it would simply be very, very contrary to the public interest to have the licensing procedure and the simple setting of schedules for preparation of findings and conclusions and the mechanical job of preparing this for the Board to become the limiting factor.

We just feel that it is imperative from the standpoint of the public interest and from the standpoint of the economic considerations invovlved, Mr. Chairman, that we not let the simple matter of the time needed to prepare these findings and conclusions become a limiting factor. We feel it is essential that everybody put in the necessary work, difficult as it is, and important as it is, to get this into the hands of the Board by the 1st of June.

CHAIRMAN JENSCH: I think that public interest is

a matter that perhaps hasn't been mentioned, and I think that is important. I think the public includes the Intervenors, and the Intervenors have, I think you will agree, made a substantial contribution in this proceeding, and their concerns and their interests are important. They are a party with the same status and standing before the Board as the Applicant, and as I say, the Applicant has never impressed the Board with its meeting its projected schedules. We can't be persuaded that there is any prejudice for the Applicant by a few more days given for the preparation of findings which are needful of the same care and study as those prepared by the Applicant.

MR. TORSTEN: I agree, Mr. Chairman. I have just one other argument to offer you on this, and that is that the Intervenors are going to have the full measure of the period allowed by the Applicant to prepare its findings. They can be working on their findings all of this time, and for them to request an additional 25 days, which is more time than the Applicant is requesting, is simply inconsistent with the whole theory and philosophy of the AEC's regulations.

The regulations allow the intervening parties to have two-thirds of the time allowed the Applicant in preparing its findings. The philosophy of this, obviously, is 15 days for the Applicant and another 10 days for the

1 Intervenor.

2 CHIARMAN JENSCH: I think you are overlooking

3 the fact that good cause can be shown, and good cause

4 involves the complexity of the case, the great volume of

5 data that has been presented, the issues presented, and the

6 fact that the parties, all parties, including the Applicant,

7 have shown a willingness to expedite this proceeding in

8 every way, and we think the same considerations attach to

9 their request for time to prepare their findings as is

10 shown by their reasonableness and their endeavors to expedite

11 the proceeding in the past.

12 MR. TROSTEN: May I say one other thing, then,

13 with regard to the need for time for the Citizens Committee

14 for the Protection of the Environment?

15 As I say, if the Board adopts the practice of

16 computing all of the time for all of the parties to

17 respond to findings and conclusions from the time that the

18 Applicant files its findings and conclusions, this means

19 that there is simply going to be a long period of time,

20 simply dead time, Mr. Chiarman, in which nothing is going

21 to be done, nothing productive, on the radiological findings

22 and conclusions, whereas this matter could be considered

23 and resolved, and any briefs necessary could be out of the

24 way.

25 That is the reason I feel it would be much more

1 productive to get the radiological aspect of this thing
2 cleared away so that all parties could devote their attention
3 to the environmental findings.
4 CHAIRMAN JENSCH: We do not believe that the
5 environmental and radiological can be disassociated. These
6 factors are interrelated as shown by the fact that there is
7 a lot of duplication when you try to separate the phases of
8 the proceeding, and it seems contrary to the requirements
9 of the Environmental Policy Act to do that.
10 What does the Regulatory Staff say?
11 MR. KARMAN: Naturally, the Regulatory Staff's
12 position, as it has been throughout the hearing, is to
13 expedite as is within the bounds of reason. The Regulatory
14 Staff will always use as a guide the provisions of 10 CFR
15 2.754. Of course, that particular chapter does indicate
16 that for good cause there may be a variation from the fixed
17 schedule in that chapter.
18 The case has been a rather long one, and while
19 the Regulatory Staff feels that the 15 and 25 and 30-day
20 schedule within the regulations may be just a trifle
21 short, we feel that possibly some time can be cut back
22 from the other end after the Applicant does file its sub-
23 mission.
24 Of course, from my calculation, it would seem
25 from May 17 to about June 14 looks closer to 30 days, which

1 I think possibly could be cut back just a little bit so that

End #242 this decision could be expedited to that extent.

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CHAIRMAN JENSCH: What date do you think the staff will be prepared to submit its proposed findings and conclusions?

MR. KARMAN: Our position, Mr. Chairman, would be that we could submit our findings and brief within 16 days after the applicant submits his, hopefully.

CHAIRMAN JENSCH: Well, that reservation is consistent, I think, with the projection.

MR. KARMAN: It is consistent with my wellbeing.

DR. GEYER: Is this based on past experience?

MR. KARMAN: No, this will be my burden, Dr. Geyer.

Some of the other delays were not necessarily of my doing.

MR. TROSTEN: Mr. Chairman, if the Staff can file its findings and conclusions within 16 days, it certainly seems to me that the other parties should be required to file their findings and conclusions at that time, that is, 16 days after May 17.

There is no reason for the three intervenors to have 25 days after this.

MR. MACBETH: Mr. Chairman, we would just like to say what is to me perfectly obvious, that the time I asked for is based upon the amount of work that I think is involved, with the recognition that there are other matters that I have

1 to attend to. I intend to work hard on this case, and I think
2 I have consistently since we began.
3 I am impressed by the time the staff says they
4 need, but it does not change the time I need. It will be a
5 big job. It would be ridiculous for me to say it isn't, and
6 I think the time I have aksed for is the time I need, and
7 nothing more.
8 Despite the fact that the staff believes it can
9 file in a shorter period, that doesn't change my opinion of
10 the time I need.
11 MR. TROSTEN: Mr. Chairman, I urge that the board
12 rule that all parties other than the applicant be required
13 to file their findings by June 2, which is 16 days after May
14 17th.
15 MR. MACBETH: That --
16 CHAIRMAN JENSCH: You wouldn't extent it even over
17 the weekend, is that your thought?
18 MR. TROSTEN: No.
19 CHAIRMAN JENSCH: The weekend can prove to be
20 pretty usable time for meeting deadlines.
21 Do you think you could bend that far, Mr. Trosten?
22 MR. TROSTEN: You mean to June 5?
23 CHAIRMAN JENSCH: That is further than I thought.
24 MR. TROSTEN: June 4, my calendar starts on a
25 different day.

1 Mr. Chairman, June 4 is better than June 11.

2 MR. MACBETH: I earnestly press the Board that I
3 be given that extra week. I do not think in asking for 25
4 days I am asking for a lengthy period. The record in this
5 case is utterly mammoth. I have virtually no assistance
6 in preparing the conclusions and findings.

7 I will work diligently at it. If there is
8 any way in which I can possibly get it in earlier than the
9 11th of June or the 14th of June, I will do so, but I think
10 I am trying to be very practical about it.

11 I think it will take me that amount of time. I
12 don't think the Board needs a review of the length of this
13 record or the mammoth amount of material involved in it,
14 and there are parts that I can't seriously begin on until I
15 see what the applicant turns in.

16 It isn't entirely true that I can get vast
17 quantities out of the way, but there is a lot that can't
18 be done until the applicant's material is at hand, and it
19 takes the applicant three weeks.

20 I find it surprising that the applicant thinks
21 the intervenors should be able to do it in much shorter time.
22 The applicant has much larger resources to work from, to be
23 perfectly frank, and I think a request for 25 days is in no
24 way excessive.

25 MR. TROSTEN: I think from a practical standpoint,

1 it is not proper for the intervenors to have that amount of
2 time. I think even June 4 is too long. I think as a
3 practical matter, it is going to be necessary for all the
4 parties to buckle down and get this job done and whether
5 you take a week longer or a week shorter, you are still
6 going to have to do everything, go all out to get this
7 job done on time.

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Reba 1

There is a great deal that can be done by all the parties as the Applicants have, during the period of time that the Applicant is doing its work. Under the regulations, they have an opportunity to respond to what we are saying, although I might add that though they are the ones who are proposing conditions, in a sense it would be entirely appropriate for the Board to require the Intervenors as well as the Staff to file at the same time we do.

But be that as it may, I really feel as a very pragmatic matter a great deal of the work can be done during the 21 days the Applicant is doing.

CHAIRMAN JENSCH: Will you address yourselves to the possible prejudice to the Applicant of a few more days. I am speaking as a practical matter now. You are going to be operating for a hundred days as an outside figure, and it may extend over a period of six months. You are into December of 1973, and your testing program is one possibility.

Bearing those factors in mind, what prejudice would befall the Applicant for a few more days?

MR. TROSTEN: I can imagine what could happen, Mr. Chairman, and I will be glad to set it forth.

CHAIRMAN JENSCH: Not just speculative imagination, something that is hard and fast, based upon the past record of not meeting a single projection on necessities for authorization in the past.

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Reba 2

1 MR. TROSTEN: Here is what could happen, Mr. Chairman.
2 We could find that the testing program for 20 percent and 50
3 percent goes very, very well, and that we are either at or close
4 to the ideal schedule that we project. Now as I said, we projected
5 it as an ideal, but we could be fortunate and we could be close
6 to that ideal, and we could find ourselves in a situation where,
7 because we were close to the ideal and because things were
8 going well, we had to then come back to this Board and make
9 some sort of a special request for authority to operate.
10 CHAIRMAN JENSCH: Is that the prejudice that you
11 think would befall you that you would have to make a special
12 request to expedite the hearing, the decisional aspect?
13 MR. TROSTEN: You would be concerned that we find
14 ourselves with no authority to proceed to test, and that would
15 be a serious prejudice to the Applicant.
16 CHAIRMAN JENSCH: That would be the first time
17 anything like that happened to the Applicants, wouldn't it?
18 MR. TROSTEN: I don't quite know what the Chairman
19 means.
20 CHAIRMAN JENSCH: None of your testing programs
21 have been on schedule, have they?
22 MR. TROSTEN: There have been delays, but it doesn't
23 mean that because we have difficulties we will have difficulties
24 in the future.
25 CHAIRMAN JENSCH: There are things that can be relied

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Reba 3

on as reasonable probabilities with reasonable assurances.

MR. TROSTEN: If I may address the practical point the Chairman is making, I think what the Chairman should do is to rule that the Intervenors are required to submit their findings by June 4, and if they find that they are abolutely unable to do so, they can submit a motion at that time and show good cause and let the Board consider at the time what the situation is.

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CHAIRMAN JENSCH: I think we get to over-judicializing these proceedings, and I know that that is an admonition we should bear in mind, by having so many papers and reconsiderations, and appeal.

I think we might as well face up to it now and not have some further proceedings about it.

MR. TROSTEN: I agree. But, again, looking at it from a practical standpoint, the Chairman will recall that when we submitted our findings and conclusions on the 50 percent testing license, there were several requests for extension that were considered to have good cause and were granted by the Board.

I think we all, before we leave the room, should understand that if we are operating a rigorous schedule --

MR. MACBETH: I am not suggesting --

CHAIRMAN JENSCH: You don't feel relaxed whatever the dates are, and don't ease over yours, even though you asked until May 17 to get in something, I hope you don't feel that is a relaxed schedule, do you?

MR. TROSTEN: No, sir, I don't.

MR. MACBETH: Absolutely. I am in no way suggesting a relaxed schedule.

MR. BRIGGS: Mr. Trosten, is the reactor critical now?

MR. TROSTEN: Not the reactor. The reactor is

ce – Federal Reporters, Inc.

1 not critical. No, sir.

2 MR. BRIGGS: It was my impression that it was

3 ready to go critical on the 19th of April.

4 MR. TROSTEN: It was ready to be critical on the

5 19th of April.

6 MR. BRIGGS: So it will be critical about the

7 19th of April?

8 MR. TROSTEN: The estimate that we have, Mr.

9 Briggs, is that it will be critical by the end of this week.

10 CHAIRMAN JENSCH: June 14 is fixed as the date on

11 or before which the Regulatory Staff and the Intervenors

12 Hudson River Fishermen's Association, Citizens Committee for

13 the Protection of the Environment and the Environmental

14 Defense Fund will file their conclusions.

15 Does the Applicant desire to have a date for

16 reply?

17 MR. TROSTEN: Mr. Chairman, you have set it three

18 days beyond what they asked for.

19 CHAIRMAN JENSCH: I thought the first date was

20 June 14. All right. June 11?

21 MR. MACBETH: June 11 will remove the three days

22 for mailing. If Mr. Trosten will see that I get a hand-

23 delivered copy on May 17 --

24 CHAIRMAN JENSCH: All right. We assume you will

25 hand-deliver your filings on May 17 by hand.

1 MR. TROSTEN: Yes.

2 CHAIRMAN JENSCH: Do you desire to have a date for
3 reply to those findings and conclusions? The Staff and
4 Intervenors will present their findings and proposed
5 conclusions by June 11.

6 The Board will add this phase, that if, during the
7 course of May it is found that your testing program is going
8 faster than you have projected or expected, if you will so
9 report that to the Board and indicate the status and develop-
10 ments of your testing program, the Board will set up an
11 expedited decisional period after the time of the submission
12 of the proposed findings and conclusions so that there will
13 be no delay to the Applicant, but we would expect a status
14 report, what you have done and what you have left to be done
15 at each time of the report.

16 Will you keep the Board so informed?

17 MR. TROSTEN: Yes, we will, Mr. Chairman.

18 CHAIRMAN JENSCH: We project the things that
19 still need to be done. Would you project the items that
20 need to be done?

21 MR. TROSTEN: Yes.

22 CHAIRMAN JENSCH: Thank you.

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MR. TROSTEN: Bear with me, Mr. Chairman.

I want to consider these dates.

Mr. Chairman, we would propose June 25 for the date or reply, applicant's reply to all other parties.

CHAIRMAN JENSCH: June 25, 1973. That is fixed as the date on or before which the applicant will submit its reply to all the previously filed proposed findings and conclusions.

MR. TROSTEN: May we set a schedule for transcript corrections, Mr. Chairman?

CHAIRMAN JENSCH: Yes.

MR. TROSTEN: It seems to me if all parties would submit transcript corrections by June 15, and reply by June 25,that would be suitable.

CHAIRMAN JENSCH: I would like to set an earlier date. I think the record should be established so that the proposed findings are directed to what the status of the record is, and how about May 24, the fixed date for the initial proposed corrections to the transcript and May 31 fixed as the date for reply to proposed corrections of the transcript?

Is that agreeable to the parties?

MR. KARMAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Hudson River?

MR. MACBETH: The only reason I am pausing is that

with the one week of the reply, are we also getting Memorial Day weekend. I am just afraid that the mail being what it is --

CHAIRMAN JENSCH: We are all in town here. Can't you get your system here in town to hand deliver?

MR. MACBETH: Again, if I have hand delivery, that is fine.

CHAIRMAN JENSCH: -Fine. Let's work on hand delivery.

The Board would like to have an oral argument in this proceeding, and we think that an oral argument can better be conducted after there has been thorough consideration of all the proposed findings and conclusions.

If your findings come in on June 25, we might get into the Fourth of July sometime. How is July 2 as a date for oral argument?

MR. TROSTEN: That is fine.

MR. MACBETH: It is fine with me.

MR. KARMAN: It is all right with me.

CHAIRMAN JENSCH: At a location in Washington, to be designated by a later order, after we find space.

end 28

CHAIRMAN JENSCH: Is there any other matter that we can take up before conclusion of this proceeding?

MR. MACBETH: Mr. Roisman requested that I make one statement for him on the record, which is that it is his position that at any time until the initial decision is rendered, he would be free to put any newly discovered matter before the Board. He does not know of any newly discovered matter at the present time. He anticipates that his position would be vigorously opposed by the other parties, and simply wanted to have it stated as his position so that it would be on the record, and does not expect the Board to rule upon it, or necesarily that the other parties accept it.

Of course, if the Applicant and Staff did accept it, he would be very pleased.

MR. TROSTEN: Mr. Macbeth has taken the words out of my mouth.

CHAIRMAN JENSCH: One of the projections that I think can safely be made is that there will not be an agreement.

Is there any other matter we can take up?

MR. TROSTEN: I move we close the record.

CHAIRMAN JENSCH: Does anybody object?

If not, this hearing is now concluded.

(Whereupon, at 5 p.m., the hearing was concluded.)

Set I

STAFF RESPONSES TO APPLICANT'S INTERROGATORIES ON SUPPLEMENTAL STAFF TESTIMONY ON THE FEASIBILITY OF A FISH HATCHERY AND REPLACEMENT BY STOCKING OF FISH IN THE HUDSON RIVER

1. What is the source of the statement on Page 1 in connection with stocking of the Choctawhatchee River that Applicant states that about 200 fish were harvested by sports fishermen?

   Response: March 8 Redirect Testimony of Harry G. Woodbury, Tr. 10,133, Line 23.

2. With reference to the material on Page 4, concerning shad, what percentage of eggs are recovered in stripping female striped bass by use of current techniques?

   Response: It is my understanding that the percentage of eggs recovered during the stripping of female striped bass which are successfully spawned in hatcheries is a very high proportion of the egg production of those fish and roughly equivalent to the production of eggs in the field.

3. On Page 6 there is a statement "No benefits to the commercial fishery were ever shown from the operation of the hatcheries ...". Does this refer to hatcheries which stocked yolk sac fry or fingerlings?

   Response: The statement refers to hatcheries which stock yolk sac fry.

Note: The staff is preparing a detailed evaluation of the proposed hatchery production in response to a request by Mr. Briggs.

4. Is the figure of two billion eggs on Page 8 based on the assumption of stocking fry or fingerlings?

Response: The two billion eggs refers to an estimate of the number of viable eggs which were produced by the Hudson population in 1967. This should be considered a minimum estimate. The hatchery egg production assumed in this discussion would be one billion eggs, stocked as fertilized eggs. This statement was presented only to provide some indication of the relative size of the hatchery effort which would be required.

5. If the answer to question No. 4 is "fry", what number of eggs will be required if fingerlings were stocked?

Response: Not applicable.

6. What assumption is made on the survival rates from the fry to fingerling stage in a hatchery as compared to survival rates for this stage in the river?

Response: Survival rates from post-yolk sac fry to fingerling stage in the hatchery were assumed to be roughly comparable to those observed in the river.

7. If the answer to question No. 4 is "fingerlings", what is the assumed survival rate from egg to fingerling size in the hatchery?

Response: Not applicable.

8. If yolk sac fry were raised in hatcheries to fingerling size, would this add a significant amount of protection beyond the 3 or 4 days referred to on Page 5?

Response: Yes.

9. What other assumptions were used in the calculation that led to the statement that 2,000 female bass would be required?

Response: 1) The hatchery production is 1/2 of natural production.

2) The overall survival rate of viable eggs is the same in the Hudson River as it would be in the hatchery.

3) The source of stock utilized for hatchery operation does not reduce the spawning stock of the Hudson River.

4) An annual average egg requirement of 1 billion eggs.

5) An average of 50,000 viable eggs per pound of nature female bass.

6) An average weight of 10 lb. per female bass.

Request for Additional Information

Section I

1. In reference to Dr. John Lawler's testimony of March 30, 1973, provide a complete listing of the computer programs used to generate the curves shown in Fig. III-8.

2. Provide the original input and output data used for all the actual cases run as shown in Figs. III-3, III-7, III-8, III-9. Specifically the longitudinal dispersion coefficient$^{(E)}$ and the thermal stratification factor (TSF) used should be given as a function of distance, since they were not specified anywhere in Lawler's testimony.

3. Figures III-7, III-8, and III-9 should be completed to show the extent of the curves below Mile Point 30. Provide input and output data and computer runs for such information.

4. Provide a copy of the report, "Application of the M.I.T. Transient Salinity Intrusion Model to the Hudson River Estuary," Technical Report No. 153, Ralph M. Parsons, Laboratory for Water Resources and Hydrodynamics, Department of Civil Engineering, M.I.T., prepared under the support of Quirk, Lawler and Matusky Engineers, Tappan, New York, September 1972.

5. Provide all empirical correction factors which have been used in all computer runs to derive the curves in the above mentioned figures.

6. For any of the observed cases for the power plants in actual operation which Dr. Lawler has relied on for calibrating his mathematical models or used for comparison with Staff predictions, provide all the actual meteorological conditions (wind velocity, humidity, dry and wet bulb temperatures, cloudiness, rain, equilibrium temperatures), actual river conditions (ambient temperatures, salinities, fresh water flows, tidal ranges or water surface elevations, equilibrium temperatures), actual ocean conditions (temperatures, water levels, salinities) and actual power plant conditions (intake temperatures, discharge temperatures, condenser flows, discharge velocities, actual power plant operating loads). Those conditions should be specified for the period of monitoring and data collection as well as those conditions for the period of at least two months prior to that time.

7. For the power plants in operation, provide complete lists of the data collected and measurements taken to monitor the thermal plume. The number of thermocouples used, their locations, and the frequency of measurements taken should be specified.

8. Provide the calculations made to evaluate the longitudinal dispersion coefficient, the heat exchange coefficient and the thermal stratification factors which existed during the observation period when the power plants were in operation.

This information for the above requests is definitely necessary in order for the Staff to make an evaluation of the calibration of Dr. Lawler's mathematical models.

Section II

Based on the present material of Dr. Lawler's testimony, the Staff has the following additional questions:

1. The Staff has calculated the heat released into the atmosphere based on Fig. III-8 and found that this heat is about 20% of the total heat dumped into the river by all the 5 power stations. It seems from this, that Dr. Lawler has used on the average a thermal stratification factor (TSF) of about 5.

   If this is correct, should the same thermal stratification factors be used for evaluating the 4°F excess temperature for the river surface width? How does the thermal stratification factor value of 5 compare with the previous values used in your earlier predictions made in as for example, Lawler's testimony of April 5, 1972 entitled, "The Effect of Indian Point Units 1 and 2 Cooling Water Discharge on Hudson River Temperature Distribution."

2. When using such a TSF, one takes advantage of the assumption that the temperature increases are mainly concentrated on the surface and that the lower layers stay relatively cold. For such a case, it may be

assumed that only part of the river depth is actually participating in heat absorption and dispersion. Was such reduced effective depth considered in Lawler's model? If yes, what value was assumed? If not, why?

3. For Fig. III-8, the excess temperatures were indicated but no mention was made of the actual river ambient temperature. Is this because the excess temperature predictions are almost independent of the ambient temperature? If yes, could you explain again the comments on page III-5?

4. In Fig. III-8 it is shown that the average temperature rise at the Indian Point site for simultaneous operation of all five power stations is about 1.3°F. However, in Dr. Lawler's testimony of April 5, 1972, mentioned above, and in Table 4, page 209 of Vol II of the FES, it is indicated that the temperature rise will be 1.75°F at the Indian Point site with the Indian Point Units 1 and 2 and Lovett power plants alone in operation. Please explain this contradiction.

   In connection with this comparison, explain how the values were obtained in Table 6 of Lawler's April 5, 1972 testimony (see also Table 3, page 207, Vol II of FES), based on the equation given on page 206, if "no empirical correction were employed" as claimed. The Staff has used this equation with no correction factor and with all the numerical values specified by the Applicant and finds for the

first case a ΔT of 1.14°F per 100 billion BTU/day instead of 0.84°F. This means a $\overline{\Delta T}$ of 2.24°F for the heat load of Indian Point Units Nos. 1 and 2 alone. Clarify this point in relation to the 1.3°F given in the present testimony for all five power stations operating simultaneously.

5. Dr. Lawler shows in his testimony that there will be no measurable Lovett-plant-induced temperature rises in the vicinity of Indian Point. The effect of Indian Point Unit No. 1 is evaluated by Dr. Lawler to be about 0.2°F area average temperature rise. In Section XII, pages 9-10 of the FES, there are tabulated temperatures which were observed by New York University Staff at the Indian Point site for two successive years. This observed data show an area average temperature of 80.4°F for August 1968 and 80.66°F for August 1969. In light of the Applicant's repeated position that the maximum ambient temperature of the river is 79°F, can one conclude that the area-average temperature rise observed by New York University is about 1.5°F as compared to 0.2°F claimed by Dr. Lawler, or should one conclude that the river ambient temperature was 80.3°F?

6. Based on the same observed data one can see that the thermal stratification factor (TSF) is very close to 1.0 (1.012 for August 1968 and 1.018 for August 1969). How do these values compare with the values Dr. Lawler has been using in his testimony of March 30, 1973 for the five power stations on the Hudson River?

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