2-12-73

UNITED STATES OF AMERICA

ATOMIC ENERGY COMMISSION

In the Matter of	)	
	)	
	)	
Consolidated Edison Company of	)	Docket No. 50-247
New York, Inc.	)	
(Indian Point Station, Unit No. 2)	)	

REDIRECT - REBUTTAL TESTIMONY

OF

JOHN R. CLARK

February 12, 1973

This redirect-rebuttal testimony is submitted in response to outstanding questions raised by the Board and points made by Con Edison and the AEC Staff in the hearings or in later testimony submitted in writing. In some instances, the later testimony does not contain a sufficiently full statement of newly presented facts to allow full analysis, in those cases further testimony by me may be appropriate when all the facts on which the later testimony is based are available.

<u>Effects of Entrainment</u> (Testimony of Gerald J. Lauer, "Effects of Entrainment on *Morone sp*. (striped bass and white perch) eggs and larvae at Indian Point," undated).

An initial difficulty in analysing Dr. Lauer's testimony must be pointed out. In December, we received the data on the entrainment study on striped bass and white perch conducted at Indian Point 1 last summer. This is included in the record as HRFA Exhibit II. That data did not go beyond August 1, 1972; it did not distinguish between striped bass and white perch and no sizes for the fish were given. In his rebuttal testimony, Dr. Lauer indicates that samples were taken through August 19, 1972, that the white perch and striped bass samples have now been distinguished from one another and that the sizes of the organisms have been analysed (pp. 5-7). But this new data has not been presented in a sufficiently detailed form to allow independent analysis by others. Therefore, my testimony is based primarily on the detailed data reported earlier, further testimony may be appropriate after I have had an opportunity to examine the detailed data which Dr. Lauer has reviewed.

Dr. Lauer's presentation of the data on entrainment mortality at Indian Point 1 effectively masks the relevant result; i.e., at the 15° ΔT expected at Indian Point 2, over

90% of the larvae were dead or stunned before leaving the discharge canal. The figures are set out in Table 1.

The masking was accomplished by adding together the data for D-1 (the net station in the discharge canal which is closest to the plant) and D-2 (the net station at the far end of the discharge canal) and thus concealing the progressive mortality during transit down the canal. The larvae arrived at station D-1 only a minute or two after leaving the condenser pipes; this is surely not time enough for all the injured fish to die or show visible symptoms of damage. For many fish, it could take some minutes for the injuries sustained to become apparent as death or visible damage. Also, samples collected at D-2 are exposed to the discharge temperature for a much longer period. Consequently, the answer we are seeking is not an average that combines the percentage dead at each end of the canal, but the percent that are dead by the time they reach the far end of the canal - over 90% at the full ΔT, according to NYU's station D-2 results.

Consequently, the pooled percent survival should not be 61% (Lauer testimony, Table 3 - 34/56 x 100) but rather should be less than 10%. Once one corrected for any additional time-linked mortality and for the increased vulnerability of the larvae to predation, mortality would

TABLE 1

EFFECTS OF ENTRAINMENT ON MORONE SP. (STRIPED BASS AND WHITE PERCH) EGGS AND LARVAE AT INDIAN POINT UNIT 1 ON AUGUST 1, 1972 WITH A Δ T OF 15°F (79-94).

Station & Depth	Time Begin	Time End	Meter Reading	Condition of Eggs & Larvae: Alive	Dead	Stunned
I-2-B	2050	2055	72	0	0	0
I-2-M	2050	2055	429	5	0	0
I-2-S	2051	2056	582	7	1	0
I-2-S	2148	2153	455	1	0	0
I-1-M	2355	2400	320	3	0	0
I-1-S	2355	2400	780	6	2	1
I-2-B	2355	2400	570	8	2	1
I-1-B	2355	2400	145	0	0	0
I-2-M	2355	2400	889	8	0	0
			INTAKE TOTALS	38	5	[illegible]
D-1-M	2111	2116	505	9	1	0
D-1-B	2111	2116	372	8	0	0
D-1-S	2112	2117	692	0	0	0
D-1-M	0023	0028	722	1	0	0
D-1-S	0023	0028	882	7	2	3
			TOTALS AT DISCHARGE 1	25	3	3
D-2-M	2120	2125	438	0	0	0
D-2-S	0023	0028	500	1	4	0
D-2-M	0022	0027	610	0	4	0
D-2-B	0023	0027*	470	0	5	1
			TOTALS AT DISCHARGE 2	1	13	1

Only standard tows of five minutes or less have been included in the table. The only tow of less than five minutes is that marked with an asterisk (*). Any variation between four minute tows and five minute tows should vary to indicate a better condition of the eggs and larvae in the four minute tows than in the five minute tows.

Reference: HRFA Exhibit II

be predicted at nearly 100%.

Further masking is introduced in Dr. Lauer's testimony by combining data with ΔT's of 6, 10, 11, and 15 together, rather than presenting the results of each series separately so that the effect of the full ΔT (15°, that simulates the Indian Point 2 discharge) cannot be examined.

Failure to consider the effects of many obvious sources of error further reduces the acceptability of Dr. Lauer's statement; e.g., effects of varying the length of tow; non-synchrony of sampling at intake and discharge 1 and 2; net avoidance in relation to size of fish; differences in flow rate through the various nets.

This misconstrual of results has carried over to, and similarly affected, the validity of Dr. Lauer's testimony and the McFadden-Woodbury testimony.

Temperature Tolerance of Striped Bass (Testimony of Gerald J. Lauer, "The Temperature Tolerance of Striped Bass Eggs and Larvae Relative to Their Seasonal Occurance and Expected Indian Point Plant Discharge Temperatures," undated).

The Moncks Corner experiments upon which Dr. Lauer's testimony is based are inappropriate for determination of entrainment mortality. Dr. Lauer's plant study at Indian Point showed, by his own calculation, that 51% $\pm$ 12 of the larvae were dead at the discharge (as opposed to 43% $\pm$ 12 at the intake) and that 23% $\pm$ 10 were stunned at the discharge (as opposed to 8% $\pm$ 6 at the intake) when the plant was off line and there was no Δ T. ("Effects of Entrainment . . . ," undated, at 11 and 12). Thus, heat is not the sole factor causing damage and death to larvae in the course of passage through the plant.

The Moncks Corner experiments were with a southern strain of striped bass that appear to have a tolerance to high temperature. Specifically, Dr. Lauer concludes from this work that larvae would be safe for a 60-min. exposure at 88°F and for an 11-min. exposure at 88°F + 2°-3°, or 90°-91°, whereas Chadwick concluded from his recent work on San Francisco Bay striped bass (presented at the Johns Hopkins meeting, February 5, 1973) that maximum discharge should never exceed 85°-86°F and recommended use of the Countant 2° safety margin for a practical plant performance specification, whereby the maximum design discharge temperature would, of course, be limited to 83°-84°F. The latter conclusion is more appropriate than Dr. Lauer's, because Chadwick used fish derived, by transplant, from the Hudson stock (transported cross country by rail in the late nineteenth century), but neither of them has included the effects of mechanical and other damage that occurs during passage through the cooling system and which adds further mortality in synergy with heat.

One can predict an Indian Point 2 discharge temperature of 83°F or higher from mid-June through the whole summer period, thus including the majority of weeks of larval life of striped bass. See Lawler, "Expected Water Temperature at Indian Point During Entrainment Period", February 5, 1973.

The Effect of Pressure Changes on Entrained Organisms

(Testimony of Gerald J. Lauer, "Studies of the Effects of Rapid Pressure Changes on Striped Bass Eggs and Larvae by New York University," undated).

One would expect the negative (less than atmospheric) pressures to affect fish with fully formed gas bladders, i.e., those 8-10 mm. or larger. In neither his October 30, 1972 testimony nor in the present testimony does Dr. Lauer indicate the size of fish which he exposed to the pressure experiments. Without such knowledge, it is impossible to make a realistic estimate of the relevancy of the experiment to the conditions which will be experienced by fish passing through the cooling system at Indian Point 2.

<u>Impingement</u> (Testimony of Ronald A. Alevras, "The Estimation of Fish Impingement at Indian Point Units 1 and 2," February 5, 1973).

The basic assumption of Mr. Alevras' analysis is that the Unit No. 2 impingement kills can be calculated simply by using a ratio factor of 3x based on the ratio of flows between Unit 2 and Unit 1. From the available winter kill data for Unit No. 2 (Winter 1971 and 1972, see Stipulation) this low rate of kill would not appear to be attainable because the indications are that Unit 2 has a very much higher impingement kill potential than Unit 1 per volume of water withdrawn (See Alevras testimony at 18-19, paragraphs 3-5). Mr. Alevras' assumption results in a lower than reasonable estimate for Unit 2.

Con Edison's Unit 1 impingement estimates are based upon a selected "representative series of days during which sampling was done in a consistent manner and without interruption" (Alevras testimony at 14). Fish kills at Indian Point appear to be sporatic and to occur at irregular intervals when masses of fish are impinged due to conditions that are poorly understood and when sampling attempts may not meet the standard set forth by Mr. Alevras. Mr. Alevras' standard for selecting days may well result in ignoring days of high kills. This would tend to markededly reduce the monthly kill estimates.

The parasite argument presented by Mr. Alevras is not relevant (Alevras testimony at 22-23). Specifically, 94% of those impinged had no parasites, therefore, the impingement of 94 out of 100 of these fish could not be explained by parasites.

The weak fish argument presented by Mr. Alevras is not relevant, since the River catches used for comparison are taken by nets that select the fatter fish of any length in the net selection range (Alevras testimony at 22-25). The effect of the experiment would be to show River fish as fatter than they really are.

Response to Mr. Briggs, Tr. 8299.

The question is whether the ratio used to relate kills at Unit 1 to expected kills at Unit 1 and 2 combined (1:5) might be exaggerated because the fish kill at Unit 1 in 1971 and 1972 might have been substantially higher than it was in 1966 and 1967. This was not the case, apparently, and the Applicant has reported that the kills were, in fact, lower. Mr. Alevras finds the ratio higher than 4 to 1 during winter months when the large kills are made (Alevras testimony at 18).

Effect of Thermal Discharges in Winter (John P. Lawler, "Behavior of the Indian Point Thermal Effluent During Winter Conditions and its Effect on Hudson River Striped Bass," February 5, 1973).

Dr. Lawler's recent study of the Bowline Point plume supports my contention that the heated effluent frequently does not sheet off at the surface in winter and that it produces anomalous high temperatures within the water column and far from the source of the discharge. Though I have no data by which to test for a correlation between the sinking plume and the high kills of fish at Indian Point 1, it is possible that the variation in fish kills at Indian Point may be accounted for, in whole or in part, by the sinking plume phenomenon.

Effects of Plant Operation on Fish Populations (James T. McFadden, "Effects of Indian Point Units #1 and #2 on Hudson River Fish Population," February 5, 1973).

The figures I used on fish populations in the Hudson which Dr. McFadden criticizes (McFadden testimony at 9), are from Con Edison's studies. See Clark testimony, October 30, 1972 (Final) at 50. I have already stated that some of these figures could be seriously in error. Ibid.

The calculations of Dr. McFadden which attempt to show that the plant kill would be 14-22% instead of 39% are invalid (McFadden testimony at 21-27). One cannot use the absolute mortalities calculated in my method as the basis for rate mortalities as Dr. McFadden has attempted to do. My plant removal kill of 39% is appropriate and is within the range (30-50%) calculated by the Staff.

Movements of Striped Bass (Testimony of Edward C. Raney, "Striped Bass," February 5, 1973).

The statement that the Rathgen-Miller study was "[t]he most comprehensive study of the distribution of spawning grounds of the striped bass in the Hudson River" is false. (Raney testimony at 1). In discussing the distribution of juvenile striped bass, it is important to note that the Rathgen-Miller experiments were restricted to seining in shallow waters at the edge of the River. Other investigators, including Carlson-McCann and Raytheon, have found juvenile striped bass distributed across the width of the River, particularly in depths of 12 feet or less. See, e.g., 1 FES at V-45.

Dr. Raney's statement that "It is at a length of 1-1/2 inch or more (fork length) that young striped bass are not vulnerable to entrainment . . . " is misleading. (Raney testimony at 5). See Carlson-McCann, Table 24.

Both Dr. Raney and Dr. Lawler ("Contribution of the Hudson River to the Middle Atlantic Striped Bass Fishery," February 5, 1973) discuss the contribution of the Delaware to the Atlantic coastal striped bass fishery. I agree with Dr. Lawler that the Delaware is "a contributor of striped bass to the Atlantic population" (Lawler testimony at 5).

In my opinion, the proportion which the Delaware contributes is probably very low in view of the pollution of the estuary, particularly the oxygen block in the reach around Philadelphia.

The Murawski paper relied on by Dr. Lawler indicates that Murawski found larvae in any abundance only in one eight mile segment near the entrance of the Chesapeake and Delaware canal to the Delaware. These larvae may well have been spawned in the Chesapeake and Delaware Canal. Only small numbers of larvae were found upstream of Philadelphia - in three years of sampling Murawski found only 25 larvae upstream from Philadelphia. In fact, only 122 larvae were found during the three year period in the Delaware in the proximity of the Chesapeake and Delaware Canal. An additional 18 larvae were found in tributaries to the Delaware. These meager results came from an extensive sampling program taking more than 200 sample tows in the tree year period. W. F. Murawski, The Distribution of striped bass, Roccus saxitilis, eggs and larvae in the Lower Delaware River, N.J. DCED Mis. Report No. 1-M, (Nacote Creek Research Station), June 1969.

Dr. Raney includes the upper Chesapeake Bay in his discussion of the Delaware contribution which makes these remarks inappropriate (Raney testimony at 5). The Bason studies relied on by Dr. Raney are not quantitative and are not helpful in determining the contribution of the Delaware to the mid-Atlantic striped bass stocks.

The meristic characters discussed by Dr. Raney are unsuitable for quantitative analysis of origins of the middle Atlantic population of striped bass because the stock in the coastal waters has never been analysed quantitatively for their meristic characters.

The Weller study shows a preponderance of May (spawning season) river returns in the Hudson River, confirming the importance of the Hudson as the primary source of the middle Atlantic stock of striped bass. (Raney testimony at 7 et seq.).

Dr. Lawler also discusses the rivers where striped bass were found in the spring as reported in my earlier study. (Lawler testimony at 16-17). Striped bass are not known to spawn successfully to the north and east of the Hudson, therefore, the 60% figure for the Hudson contribution which Dr. Lawler suggests is inappropriate and irrelevant.

Research (Testimony of James T. McFadden and Harry G. Woodbury, "Indian Point Studies to Determine the Environmental Effects of Once Through vs. Closed Cycle Cooling at Indian Point Unit No. 2", February 5, 1973).

This study plan lacks the comparable, statistically valid, pre-operational baseline data against which to compare the population situation after the plant is in operation. Therefore, it depends upon an essentially theoretical approach, with collection methods of unknown efficiency. It is oversimplified, unrealistic, and has little chance to succeed in all its major objectives.

The program of research proposed by the AEC Staff in the Final Environmental Statement (1 FES V-71 to 73) is also impractical in the sense that it will take up to ten years to collect much of the fundamental information on fish which the Staff would like to have. See, e.g., Tr. 9311-9320. Enough is known at the present time of what the effects of plant operation on the striped bass will be to warrant the immediate taking of protective measures.

Value of the Fishery.

A more specific approximation to the value of the fishery influenced by the Hudson can be made as follows. Using the 1965 data for the sport fishery, an estimated 5.7 million striped bass were taken from Connecticut to Delaware, and if 80% of these were Hudson fish the Hudson would have supplied 4.5 million fish with an aggregate weight of 16 million pounds (average weight 3.6 pounds) which would be worth, at a minimum, $32 million (at $2 per pound). The commercial fishery (about 1.5 million pounds) had a retail value of $1.5 million of which 80% gives a value of $1.2 million for the Hudson. The total value of the Hudson-supplied catch in the mid-Atlantic region plus Connecticut would be $33.2 million. If Indian Point's operation reduced this catch by 39%, the loss would be approximately $13 million annually.

In addition a loss may be anticipated in the New England striped bass fishery, of which the sports catch alone comes to 8.2 million fish in 1965. The Hudson contribution to the New England fishery is unknown, but is probably worth some millions of dollars.

These values, of course, reflect only the monetary value of the striped bass. The operation of

Indian Point will probably affect other species as well for which it is impossible to provide a dollar value.

Fundamentally, all dollar values assigned to a great natural resource such as the striped bass of the Hudson are flawed. This type of natural heritage is a source of spiritual wealth and well-being, an essential element to vast amounts of recreation, and the basis of large numbers of personal livlihoods for which it is impossible to account fully in dollars and cents. Thus the figure of $13 million annually must be taken as the bare minimum of the damage which the Indian Point plant will cause.