APPENDIX A
EPRI COMMENTS ON THE EPA PHASE II PROPOSED 316(b) RULE AND NOTICE OF DATA AVAILABILITY

LWB

Environmental Services, Inc.

Barnthouse Comments

May 26, 2003

Doug Dixon
Manager, Fish Protection Research
Electric Power Research Institute
7905 Berkeley Drive
Gloucester Point VA 23062

Dear Dr. Dixon:

At your request, I have reviewed the Pisces, Ltd. comments on the benefits case study EPA performed to support the proposed 316(b) rule for existing facilities. In addition, I have reviewed EPA's March 2003 Notice of Data Availability (NODA), which described several significant proposed changes to the methods used to calculate benefits of reducing impingement and entrainment losses. My detailed comments are provided in the Attachment to this letter; summaries are provided below.

In its comments on the benefits case study, PISCES claimed that EPA had underestimated entrainment and impingement loss rates, underestimated the impact of impinging age-1 and older fish, used inappropriately low survival rates to scale entrainment losses to age-1 equivalent losses, and underestimated the economic value of entrained and impinged fish. In addition, PISCES argued that habitat restoration and replacement projects should not be used to satisfy the requirements of Section 316(b) because in-kind replacement of entrained and impinged fish cannot be guaranteed.

In reviewing Pisces' comments, I identified several significant errors and misinterpretations, and I find that most of Pisces' major conclusions are incorrect.

- In its analysis of entrainment and impingement loss rates, Pisces used an inapplicable data set and misinterpreted cooling water withdrawal data for the Salem Generating Station. I found no evidence that EPA had underestimated these losses.
- Pisces' assertion concerning the impact of impinging fish older than one year-of-age is partially correct. However the conclusion that impingement loss rates, when expressed as age-1 equivalent losses, are nearly as large as entrainment loss rates is based on invalid reasoning and is incorrect.

- Pisces' analysis of variability and bias in estimates of natural survival rates for early life stages of fish is invalid and the conclusion that survival rates for all species should be increased by 25% is incorrect.
- Pisces' "reproductive value" approach to estimating the economic value of fish that die of natural causes ignores density-dependence and would be expected to greatly overstate the actual economic value of unharvested fish.
- Pisces' critique of habitat restoration and replacement projects is one-sided and substantially understates the potential environmental benefits of these projects.

In the NODA, EPA announced a change in the assumptions made concerning the age distribution of impinged fish and concerning the fraction of forage fish biomass that is converted to harvestable predator biomass (termed "trophic transfer efficiency"). In reviewing the NODA itself and the supporting information provided in the docket, I found significant problems with both of these methodological changes.

- The new assumption concerning impingement age distributions is clearly wrong, is contradicted by data already in the docket, and would greatly overstate the benefits of reducing impingement.
- The new assumption concerning trophic transfer efficiency is inconsistent with the most recent scientific literature and would overstate the benefits of reducing entrainment and impingement of forage fish.

I was greatly impeded in my review by the poor documentation provided in the NODA and in the docket. I was unable to reproduce any of the age-1 equivalent loss, foregone yield, or production foregone estimates provided in the North Atlantic and Northern California case studies. For this reason, I cannot evaluate the quantitative importance of the errors and overly conservative assumptions I identified. In addition to the problems I found in my review, some of the values provided in the benefits tables for these new case studies appear suspicious, (e.g., the extremely high value of tautog production foregone in Table X-7, FR page 13553), however, the information needed to confirm whether errors have been made is unavailable.

Please contact me if you have any questions concerning this review.

Sincerely,

Lawrence W. Barnthouse
President

105 Wesley Lane
Oak Ridge, Tennessee
Phone: (865) 483-0100; Fax: (865) 483-0115; Email: lwb.env@attglobal.net

APPENDIX A
EPRI COMMENTS ON THE EPA PHASE II PROPOSED 316(b) RULE AND NOTICE OF DATA AVAILABILITY

ATTACHMENT

Review of Comments by Pisces, Ltd. On EPA' 316(b) Case Study Analysis
Review of Modifications to the Case Study Methodology Documented in EPA's Notice of Data Availability

Lawrence W. Barnthouse, Ph. D.
LWB Environmental Services, Inc.
Oak Ridge, Tennessee

Prepared for the Electric Power Research Institute under

Agreement No. EP-P8210/C4139

May 2003

On behalf of the Riverkeeper Organization, Pisces, Ltd. prepared an analysis of biological issues relating to the benefits case study EPA performed to support the proposed 316(b) rule for existing facilities. Pisces' analysis was submitted to EPA during the comment period for the proposed rule. In March 2003 EPA issued a Notice of Data Availability (NODA) related to the rule. The NODA provided documentation of several significant changes in the case study methodology. In this report I review both Pisces' analysis and the changes announced in the NODA.

Review of Comments by Pisces, Ltd. on EPA's 316(b) Case Study Methodology

I have reviewed Pisces' analysis with respect to technical accuracy and relevance to the rulemaking process. My comments are organized around five major issues raised by Pisces that directly challenge the basis for EPA's proposed rule. Brief comments are also provided concerning some secondary issues that do not directly challenge EPA's analyses or conclusions but that support the Riverkeeper Organization's contentions concerning the importance of minimizing entrainment and impingement losses.

Major Issue 1: Underestimation of losses due to underestimation of expected future water withdrawal rates.

Pisces argued (Section 1.4) that impingement and entrainment vary nonlinearly with flow, and that, because of recent increases in flow rates, EPA's estimates of future entrainment and impingement loss rates are biased low.

Pisces cited a paper by Kelso and Milburn (1979) as support for the proposition that impingement and entrainment are nonlinearly related to flow. These authors examined entrainment and impingement data for 37 power plants located on the Great Lakes, and developed empirical equations relating flow rate, entrainment, and impingement. Both equations are both power functions, implying that entrainment and impingement increase exponentially with flow. Clearly, since the data relate to the Great Lakes and are more than 25 years old, the equations themselves are irrelevant to evaluation of EPA's analysis. Moreover, as is shown in Figure 1, the quantitative significance of the non-linearity described by Kelso and Milburn (1979) is trivial. Figures 1a and 1b plot entrainment and impingement rates calculated from the equations provided in the Pisces analysis over an arbitrary range of 500 to 1600 gallons per second (the actual range of flows used is irrelevant – the plots would look the same over any range of flows). Along with curves calculated using the power functions from Kelso and Milburn (1979), each plot shows a linear approximation calculated by drawing a straight line connecting the two ends of the curves. It is clear that the degree of non-linearity is small. For any given flow rate, the differences between the curves and the lines are probably much smaller than the uncertainty (not discussed by Pisces) in the impingement or entrainment rate expected at any given flow. Moreover, at every flow rate, the linear approximations *overestimate* the loss estimates obtained using the power functions.

Pisces also stated that EPA underestimated losses by failing to account for increases in the mean flow at many plants. The report illustrates this contention through an analysis of withdrawal rate data for Salem. According to Pisces, EPA should have used the estimated withdrawal rate for 1998, which was very high, rather than a long-term average withdrawal rate. This particular example is clearly inappropriate, because the principal use for these data in EPA's analysis was to calculate entrainment and impingement loss rates, measured as fish per unit flow. The loss rates were then scaled to other facilities using estimated withdrawal rates for those facilities. In any case, 1998 withdrawals are probably not representative of future operations at Salem, because this particular year immediately followed a two-year shutdown of both units for a major facility upgrade. Representative future withdrawal rates would have to be adjusted for periodic refueling and maintenance shutdowns.

Major Issue No. 2: Underestimation of impingement due to age 1 assumptions

Pisces claimed that EPA underestimated effects of impingement losses "both in terms of their impact on the populations and relative to entrainment" by assuming that all impinged fish are age 1. Pisces supported this argument with a series of calculations (sections 1.5 and 1.6) in which, instead of assuming that all impinged fish are age 1, actual age distribution of the impingement counts provided in the Salem filing (as reproduced in EPA's input spreadsheet for Salem) were used to convert age 1 and older impingement losses into age-1 equivalents. The results of these calculations were especially dramatic for white perch, because white perch up to eight years old are impinged at Salem.

Pisces is correct in stating that the appropriate approach for calculating age-1 equivalents is to scale all of the age groups to age 1 using estimates of the fraction of fish expected to survive from age 1 to the age at which impingement occurred (e.g., for eight-year-old fish, the fraction expected to survive from age 1 to age 8). I was able to reproduce Pisces's calculations, and in doing so I found that Pisces's survival rates for age groups 1 through 7 are incorrect. These values were apparently taken from EPA's input data spreadsheet for Salem. This spreadsheet contains an erroneous formula for calculating total mortality rates from estimates of age-specific natural mortality and fishing mortality rates. The error inflates the total mortality rate estimates for adult fish, and consequently inflates the estimated numbers of age-1 equivalents for fish that are age 1 and older.

Although the calculations presented in Pisces's analysis are correct in principle, the interpretation of the results by Pisces is incorrect. For the purpose of impact assessment and benefits analysis, scaling older fish backwards to age 1 is not equivalent to scaling younger fish forward to age 1. For age 0 fish, the scaling adjusts losses of eggs, larvae, and juveniles to a common future age, prior to the age at which the fish may be expected to reproduce or to be harvested. These estimates can then be used to calculate expected harvest or reproduction that would have occurred at future ages, had these fish not been entrained or impinged. The same procedure cannot be applied to age-1-equivalent estimates derived from backward-scaling of losses that occur at older ages. The reason for this is that no reproductive potential or opportunity for harvest is lost prior to the age at which a fish is actually impinged. For example, each eight-year-old white perch is, according to Pisces, equivalent to 13,572 age-1 white perch (the correct value is 5,961 age-1 equivalents). This means that, for every 13,572 (5,961) white perch alive on their first birthday, only 1 would be expected to survive to age 8 years. The 13,571 (5,960) fish that did *not* survive would have died of natural causes (most likely due to consumption by predators) or would have been harvested prior to the age at which the impinged fish was lost. No foregone yield or reproduction would accrue due to the deaths of these fish.

In Table 6 of its analysis, Pisces calculated numbers of equivalent 1-year olds, by age group, for the total numbers of all RIS fish species collected over all available years of sampling at Salem. Figure 2 of my comments compares estimates of equivalent 1-year-olds (recalculated using correct mortality rates), yield foregone, and production foregone for each white perch age group. The following equations were used to make these calculations:

$$Y_i = L_i \sum_{j=i}^{n} S_{ij} W_j \left[\frac{F_j \left(1 - e^{-Z_j}\right)}{Z_j} \right] \qquad \text{(Eq. 1)}$$

Where

Y_i = yield foregone for fish impinged at age i
L_i = number of fish impinged at age i
S_{ij} = fraction of fish surviving from age i to age j ($S_{ii} = 1$)
W_j = average weight of fish harvested at age j
F_j = instantaneous rate of fishing mortality at age j
Z_j = instantaneous rate of total mortality at age j

$$R_i = L_i S_0 \sum_{j=i}^{n} S_{ij} E_j \qquad \text{(Eq. 2)}$$

Where

R_i = reproduction foregone, expressed as age-1 equivalents
E_j = expected egg production at age j, adjusted for sex ratio and % mature
S_0 = probability of survival from egg to age 1

All parameters in the above equations except for the loss rates were taken from Appendix L, Tab 18 of the Salem filing (PSEG 1999). Figure 2 shows that white perch impinged at an age of eight years, which account more than 75% of the age-1 equivalent losses as calculated using Pisces' method, account for only 7% of the total yield foregone and for less than 2% of the total reproduction foregone. It is true that assuming that age 1 and older fish are impinged at age 1 underestimates the yield and reproduction foregone due to impingement, however, the magnitude of the bias is much smaller than is implied in Pisces' analysis. Pisces's assertion that impingement losses – when measured in terms that are relevant for impact assessment and benefits analysis – are similar in magnitude to entrainment losses is erroneous.

Major Issue No. 3: Effect of survival rate on age 1 equivalent calculations

In Section 1.6.2, Pisces argued that survival rates used by EPA in scaling losses of early life stages to age-1 equivalents were too low. Support for this argument included (1) a comparison of striped bass and cunner survival rates used by EPA to other published values (2) a sensitivity analysis demonstrating that increasing the assumed survival fractions increases the estimates numbers of age-1 equivalents, (3) an assertion that EPA's survivorship estimates already include effects of power stations and therefore are probably biased high, and (4) an assertion that for this reason all of the survivorship estimates should be increased by 25%.

The fact that survival rates of early life stages are highly variable and difficult to measure is well known. Table 1 compares empirical estimates of survival rates from five different studies of bay anchovy, one of the most frequently studied of all fish species vulnerable to entrainment and impingement. This table shows that the actual range of variation in estimated values is much greater even than is suggested in Pisces's comments. This variability does not imply, however, that EPA systematically underestimated or overestimated survival rates as compared to published studies.

It is definitely not true that the estimates used by EPA in general include station mortality. This could be the case only for survival estimates derived from site-specific studies of populations susceptible to station impacts. However, survival estimates used in 316(b) demonstrations are only rarely based on site-specific data. In the great majority of studies, survival estimates are derived from available scientific literature.

Even if the estimated survival rates did include station mortality, it is not true that conditional mortality rates are "often in the 10%-25% range." Such rates have been observed only at a few sites (most notoriously, the Delaware Estuary and the Hudson River) and for only the most susceptible species at those sites. Values this high are not representative of all sites or species nationwide. Hence, there is no justification for increasing the survival rate estimates used in the age-1 equivalent calculations.

Major Issue No. 4: Calculating the worth of commercial species impinged and entrained

Pisces asserted that EPA had underestimated the economic value of entrained and impinged fish species by neglecting to value those fish that would have died of natural causes rather than being harvested. Pisces's argument is based on "reproductive value," defined as the expected contribution of a fish at any given age to future generations of fish. If the reproductive value of each egg is defined to be 1.0 (since at equilibrium one egg will be produced in each generation for each egg produced in the previous generation), then the reproductive value of a fish at any given age is given by:

$$V(a)=\sum_{x=a}^{a_{max}}\frac{l(x)}{l(a)}m(x) \qquad \text{(Eq. 3)}$$

Where:

> *l(a)* = fraction of eggs expected to survive to age *a*
> *l(x)* = fraction of eggs expected to survive to age *x* ($x \geq a$)
> *m(x)* = fecundity of a fish at age *x*

For age *a* = 1 year, Equation (3) calculates the number of eggs expected to be produced over the lifetime of each age-1 equivalent fish. When applied to age-1 equivalent fish, reproductive value as defined in Equation (3) is identical to reproductive potential as defined in Equation (2) divided by the age 0 survival rate.

As noted by Pisces, the economic benefits model used by EPA calculates the expected lifetime yield from each 1-year-old equivalent fish, and then assigns an economic value to that yield. Pisces used Equation (3) to assign values for those fish that die of natural causes rather than being harvested. Pisces calculated the number of eggs that would have been produced by the unharvested fish, multiplied this value by the fraction of eggs expected to survive to age 1, and then calculated the value of these second-generation 1-year-olds using EPA's model.

Pisces provided numerical calculations for striped bass and for 11 species entrained and impinged at Pilgrim. The survival and fecundity values for striped bass appear to have been taken from Setzler-Hamilton et al. (1980) and do not match values used by EPA or by PSEG (1999). It is not clear whether the age-specific fecundity values were adjusted to account for sex ratio (which they should have been). However, the principal problem with the approach is with the validity of the multi-generational extrapolation. Pisces's approach assumes that recruitment is directly proportional to egg production, i.e., there is no density-dependence. Numerous recent studies, as reviewed by Rose et al. (2001), have shown that density-dependent recruitment in marine fish species is the rule rather than the exception; evidence for density-dependence in striped bass is especially strong. For this reason, Pisces's approach should overestimate next-generation reductions in harvest. In a population that is relatively stable from generation to generation, there would be little or no net loss to the next generation because reduced egg production due to the losses would be balanced by improved reproduction or survival of those fish that were not entrained or impinged. Even if a reduction in recruit production due to entrainment and impingement did occur, any reduction in value assigned to these foregone future fish would have to be converted to net present value using an appropriate discount rate.

Pisces's comment simply reflects an alternative and highly conservative assessment approach, not an error on the part of EPA.

Major Issue No. 5: Biological issues implicit in habitat replacement

Section 5.1 of Pisces' comments consists primarily of a one-sided value-based argument (non-scientific) against PSEG's Estuary Enhancement Program and other similar restoration projects. However, three important and arguably valid points are raised on page 55:

1. Habitat equivalency analysis is primarily aimed at offsetting past losses or damage, rather than continuing loss

2. Considerable uncertainty exists as to whether equivalence can be focused on actual species harmed; and

3. Sufficient habitat to offset losses or damage may often be unavailable.

The first two points raised by Pisces are technically correct but irrelevant. Since cooling water withdrawals do not affect the ability of habitat to perform its normal ecological function, the concept of habitat equivalency is inapplicable to 316(b) issues, regardless of whether the damage is past or continuing. In addition, it should be obvious that habitat restoration projects cannot possibly be designed to provide specific numbers of specific fish species. Successful projects can enhance the productivity and diversity of entire ecosystems, however, the numbers or biomass of individual species that will be produced by any given project cannot be confidently predicted. Even the most aggressive proponents of habitat restoration make no such claims. In raising the issue of in-kind replacement as a defect in EPA's proposed rule, Pisces has simply erected a convenient straw man to knock down.

With regard to point no. 3, Pisces is probably correct that lack of suitable quantities of habitat will often prevent companies from using restoration as a means of satisfying the rule. However, this does not mean that these activities should not be pursued where feasible.

Regardless of the objections raised by Pisces, habitat restoration is a worthwhile activity that can provide a wide variety of tangible environmental benefits. The most obvious of these benefits include enhanced production of all types of aquatic biota, provision of habitat for wildlife, and increased opportunities for aesthetic enjoyment and education. Benefits of restoration can be expected to continue long after the retirement of all of the facilities subject to the proposed rule. In contrast, long-term monitoring studies have provided at best equivocal evidence that fish populations have been adversely affected by entrainment and impingement losses. Reducing those losses may produce no measurable environmental benefits.

Other issues

Impacts on threatened and endangered species. The need for additional reductions in losses to protect T&E species is raised in Section 7.1 of Pisces' comments. However, these species are already protected by the Endangered Species Act. Operators whose facilities have the potential to entrain or impinge T&E species are already required to have consultations with the appropriate agencies, and to obtain certification that they are not harming these species.

Problems in calculating age-1 equivalents. Section 1.5 (page 8) correctly notes that the validity of the calculations is limited by the quality of available data on stage-specific losses and survival rates. As noted above, there is no indication that EPA systematically overestimated or underestimated these values.

Trends in abundance of fishes. Section 1.7 (pages 22-26) notes that, because of improved water quality and in some cases improved fisheries management, the abundance of some fish species has increased. Any such increases would likely result in increased entrainment and impingement losses. This, according to Pisces, means that the economic benefits of reducing the losses might have been underestimated if the available data were collected in the 1970s. On the other hand, some species have declined since the 1970s (Atlantic tomcod in the Hudson River is cited as an example). In these cases, using older data means that the potential impacts of entrainment and impingement on the declining species may have been underestimated. This comment is clearly an example of "spin" and not a technical comment on the benefits analysis. Since the benefits analysis is a national aggregate, as long as the increases and decreases are roughly balanced there would be minimal effects on the net results.

Increased species richness and fish/crustacean abundance following plant closure. I have previously read through the Henderson et al. Hinkley Point monitoring report that is cited in Section 1.7.2 as support for the proposition that reducing losses through plant closures is beneficial to fish populations. Effects of reduced withdrawal of cooling water from the Bristol Channel are confounded with effects of improved water quality and regional oceanic temperature increases that have occurred over the same time period. Although the authors claim that within two more years they will be able to test whether station closures have contributed to the observed increases, they provide no indication of how they will perform the test.

Comment on EPA's Notice of Data Availability for the 316(b) Phase II Existing Facilities Rule

My comments on the NODA address changes in the benefits assessment methodology that were announced in Section X. Both of these changes would significantly increase the estimates of benefits resulting from reductions in impingement and entrainment losses. One of these changes, a revision in the assumption made concerning the age distribution of impinged fish, is consistent with a recommendation made by Pisces (reviewed above). The new assumption clearly conflicts with readily available data concerning the typical ages of impinged fish and results in greatly inflated estimates of the economic impact of impingement losses. The second change, a revision in the assumption made concerning trophic transfer efficiency (i.e., the fraction of forage fish biomass that is converted to harvestable predator biomass) likely also leads to an overestimation of benefits, but the quantitative importance of the change is impossible to calculate from the information provided by EPA.

Inflated estimates of foregone yield due to impingement

In the NODA, EPA states that:

> In the case studies prepared for proposal, EPA determined that all impinged fish are age 1 because of a lack of data on the actual ages of impinged fish. As several commenters pointed out, this biases estimates low because impinged fish may include older individuals that are closer to harvestable age. This is confirmed by data on the ages of impinged fish presented in studies conducted at Salem (PSEG, 1999) and Millstone (Northeast Utilities Environmental Laboratory, 1992). To address this concern, the current studies relax the assumption that all impinged fish are age 1, and assume instead that the ages of impinged fish are 1 and older, and follow an age distribution that is implied by the associated survival rates. This approach takes into consideration the common observation that relatively few older, larger fish are impinged. The effect of this adjustment is that a higher proportion of impinged fish are assumed to survive until harvest. As a result of this adjustment, the estimate of foregone yield associated with impingement increases by a factor ranging from about three to ten, depending on a species' age-specific survival rates. [NODA, section X.B.3.b(4), 68 Fed. Reg. 13,546, col. 2]

The adjustment made by EPA is based on erroneous assumptions concerning the age composition of typical impingement collections. Because of these errors, EPA's adjusted estimates greatly overstate the expected foregone yield due to impingement. Studies of the ages of impinged fish have consistently shown that:

1) Most impinged fish are <u>younger</u> than one year of age, and not one year old or older as assumed by EPA.

2) The vulnerability of most species to impingement decreases with age, so that EPA's use of survival rates to estimate the age composition of impinged fish usually overstates the relative contributions of older fish to impingement losses.

The importance of these errors is demonstrated below, using the data for Salem provided in Docket No. 4-2051 to the Proposed Rule. The Salem data are used for this demonstration because, unlike the other input data files provided by EPA, the impingement data for Salem include a breakdown by life stage and age class. Figure 3 plots age distributions of fish impinged at Salem from 1990 through 1998 for three representative species: weakfish, striped bass, and white perch. The actual age distributions are compared to the distributions implied by EPA's original (proposed rule) and revised (NODA) assumptions concerning the age distributions of impinged fish. For all three species, the impingement totals are comprised primarily of fish in the juvenile 1 and juvenile 2 life stages. Only 0.2% of weakfish, 15% of striped bass, and 26 % of white perch were age 1 or older. For only one of the species listed in the Salem input file, bay anchovy, do age 1 and older fish make up more than half of the total impingement losses. Figure 3 shows that, contrary to EPA's assumption in the NODA, no weakfish older than age 1 and no striped bass older than age 2 were reported in impingement collections at Salem from 1990 through 1998. White perch up to age 8 are impinged at Salem, however, even for this species EPA's assumed age distribution greatly overstates the proportion of fish impinged at ages older than age 1.

As in my comments on the Pisces report (above), I used Equation (1) to calculate the foregone yield due to fish impinged at each age or stage. This is the same yield equation used by EPA [see NODA, section X.B.3.b(1), 68 Fed. Reg. 13,545-46]. The total foregone yield due to impingement is obtained by summing the stage and age-specific values over all stages and age classes. Results of these calculations are shown in Figure 4. For all three species, foregone yield estimates calculated using the NODA assumptions are inflated compared to estimates calculated using actual age distributions. For weakfish, the NODA value is inflated by a factor of 70 times over the value calculated using actual age distributions. For striped bass and weakfish, assuming that all impinged fish are age 1 also greatly overestimates foregone yield. Only for white perch does the age-1 assumption underestimate foregone yield, and the difference in this case is only about 20%.

The age distribution of fish impinged at Salem is probably typical of estuarine facilities, and perhaps most facilities. Very small fish have lower swim speeds and smaller energy reserves than larger fish, and are therefore more vulnerable to being trapped and impinged. Moreover, a large fraction of the species impinged in high numbers at Salem and other estuarine facilities spend most of their life cycles at sea. These species include anadromous species such as striped bass, American shad, alewife, and blueback herring; and estuarine-dependent species such as weakfish, spot, Atlantic croaker, and Atlantic menhaden. For all of these species, EPA's original approach to calculating foregone yield due to impingement almost certainly would have overstated the potential reduction in harvest; EPA's revised approach greatly overstates this reduction.

Relatively few of the species addressed in the case studies (e.g., white perch and bay anchovy) are estuarine-resident throughout their life cycles and, therefore, vulnerable to impingement at all ages. However, the revised approach still would overestimate foregone yield because it assumes that all impinged fish are at least one year old.

The extent to which the above comments apply to EPA's estimates of production foregone due to impingement is unknown, because EPA has provided no documentation of the method used to calculate production foregone for harvested species. Although Chapter 5 of the original case study report states that the production foregone model was applied only to forage species, both the original case study and the new regional case studies documented in the NODA (Tables X-6, X-8, X-20, and X-22, 68 Fed. Reg.

135,52-53, 13561-62) include estimates of production foregone for impinged fish belonging to harvested species. If the same assumptions used to calculate yield foregone for these species were also used to calculate production foregone, then the production foregone estimates would be similarly biased.

Estimation of Trophic Transfer Efficiency

In the case study performed to support the proposed 316(b) Phase II Existing Facilities Rule, EPA used a trophic transfer model to estimate the yield of harvested species foregone due to entrainment and impingement of forage species. EPA's model assumed that 20% of forage species biomass is directly consumed by harvested species. EPA assumed that the remaining 80% is consumed by intermediate predators, which are then consumed by harvested species. EPA assumed that the trophic transfer efficiency for the direct pathway is 9%, and that the transfer efficiency for the indirect pathway is 0.9%. These values imply a net transfer efficiency, considering both direct and indirect pathways, of 2.5%.

In the NODA (section X.B.3.b(2), 68 Fed. Reg. 13,546, col. 1), EPA stated that it had revised the trophic transfer model and was now assuming a net trophic transfer efficiency of 20%. The change, according to the NODA, was "based on an additional review of the scientific literature." The change reflects a questionable review of the scientific literature and could lead to overestimation of the estimated benefits of reducing entrainment and impingement of forage fish.

The following issues are relevant:

3) The trophic transfer efficiency is derived from an unrefereed source and is at the high end of the range of accepted values.

4) The modified approach assumes that 100% of forage fish biomass is consumed by economically valuable species.

With regard to the first issue, the only citation provided in the NODA to support the new value is to "Reed *et al.* (1994)." The reference cited by EPA is the documentation report for NOAA's Type A Natural Resource Damage Assessment model for the Great Lakes. This model was developed to facilitate calculation of natural resource injuries and service losses cause by spills of oil or hazardous substances. Rather than providing a detailed review of the literature on trophic transfer efficiency, the NOAA report simply states that a range of values between 10% and 30% has been estimated by various authors, provides a brief list of citations, and states that the "preferred" value is 20%. The most recent of the papers cited in the report was published in 1987. Pauly et al. (1995) published a more recent and more thorough review of the literature on trophic transfer efficiency. These authors compiled 140 estimates of trophic transfer efficiency from 48 trophic models of aquatic ecosystems. They found that, although the range of values was very wide, the mean value was 10% and only a few of the values were 20% or higher. It appears from Pauly and Christensen's study that the value chosen by EPA is at the upper end of the range of accepted values and probably overstates the average trophic efficiency across all aquatic ecosystems.

With regard to the second issue, although the actual percentage of forage species that are consumed by harvested species is unknown, it is certain that a large fraction of forage species production goes to unharvested species, including invertebrates such as jellyfish. In the Lake Turkana food web described by Pauly and Christensen (1995), for example, approximately 60% of pelagic forage fish biomass is directly consumed by the top-level predators (tigerfish and Nile perch). The remaining forage biomass is consumed by catfish, which are then consumed by Nile perch. EPA's original assumption concerning the fraction of forage fish biomass directly consumed by economically valuable species may be either an underestimate or an overestimate of the actual average value; the new assumption clearly is an overestimate.

EPA claimed in the NODA that the effect of the change in trophic transfer assumptions insignificant because foregone yield attributable to losses of forage fish is only a small component of the total foregone yield due to entrainment and impingement. EPA has not provided supporting analyses to verify this claim. However, the change in assumptions results in a factor-of-eight increase in all estimates of yield foregone due to losses of forage species. The difference might well be significant for facilities at which entrainment and impingement losses consist primarily of forage species. BFN

References

Kelso, J. R. M., and G. S. Milburn. 1979. Entrainment and impingement of fish by power plants in the Great Lakes which use the once-through cooling system. *Journal of Great Lakes Research* **5**:182-194

Pauly, D., and V. Christensen. 1995. Primary production required to sustain global fisheries. *Nature* **374**:255-257.

PSEG 1999. Permit Renewal Application, NJDPES Permit No. NJ0005622, Salem Generating Station, March 4, 1999.

Rose, K. A., J. H. Cowan, Jr., K. O. Winemiller, R. A. Myers, and R. Hilborn. 2001. Compensatory density-dependence in fish populations: importance, controversy, understanding, and prognosis. *Fish and Fisheries* **2**:293-327.

Setzler-Hamilton, E. M., W. R. Boynton, K. V. Wood, H. H. Zion, L. Lubbers, N. K. Mountford, P. Frere, L. Tucker, and J. A. Mihursky. 1980. Synopsis of biological data on striped bass, *Morone saxatilis* (Walbaum). NOAA Technical Report Circular 433, FAO Synopsis 121.

Table 1. Published mortality rates and survival fractions for bay anchovy post yolk-sac larvae (From Table 10-10 of PSEG 1999, Appendix III, Attachment C-5)

Study	System		M (1/day)	Fraction surviving[a]
Castro and Cowen, 1991	Great South Bay, NY	Min	0.20	0.25%
		Max	0.48	<0.001%
Purcell et al., 1994	Chesapeake Bay	Min	0.41	<0.001%
		Max	4.25	<0.001%
Cowan and Houde, 1989	Chesapeake Bay	Min	0.08	9.07%
		Max	0.23	0.10%
Houde, 1989	Biscayne Bay, FL	Min	0.30	0.01%
		Max	0.45	<0.001%
PSE&G, 1984	Delaware River	Min	0.07	12.25%
		Max	0.10	4.98%

[a]assuming a stage duration of 30 days.

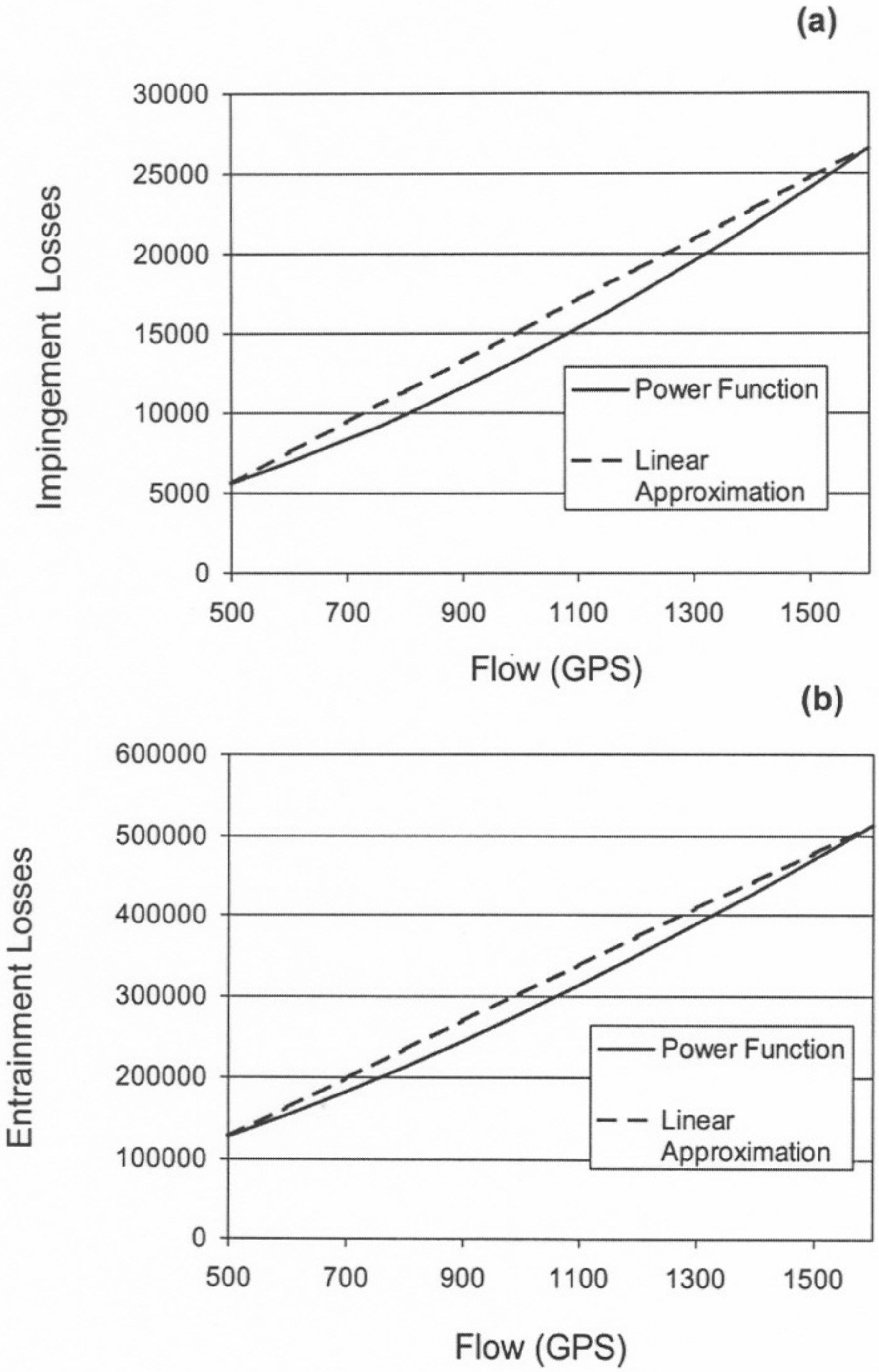

Figure 1. Power functions fitted to Great Lakes entrainment and impingement loss data (from Kelso 1979), compared to linear approximations over the same flow range.

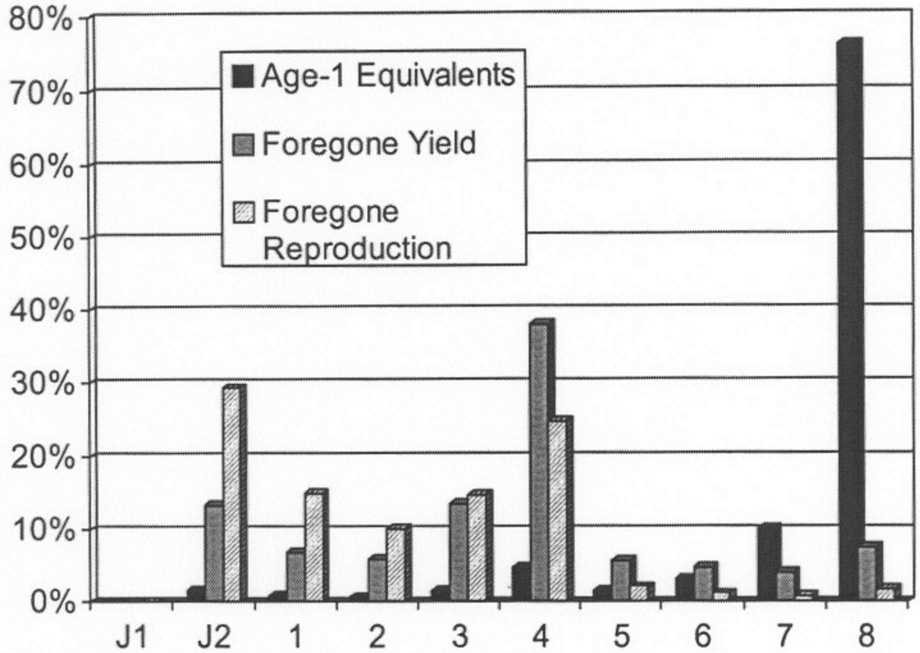

Figure 2. Relative contribution of different age groups to total losses of white perch at Salem, over all available years, expressed as age-1 equivalents, foregone yield, and foregone reproductive potential.

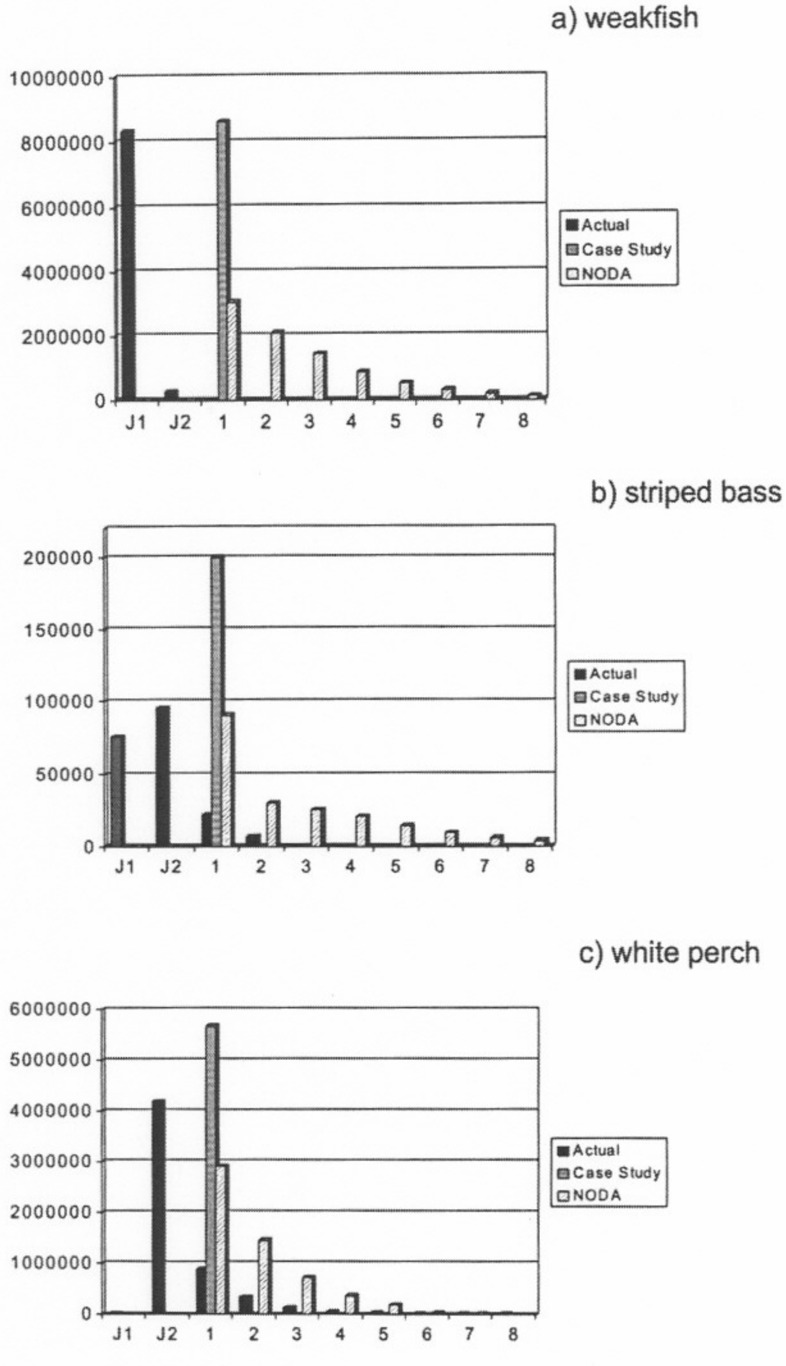

Figure 3. Comparison of actual age distribution of impingement losses to distributions assumed by EPA (1) in the original case study report and (2) in the new case studies documented in the NODA. This graph was constructed using estimates of total impingement losses at Salem, by stage and age, from 1990 through 1998.

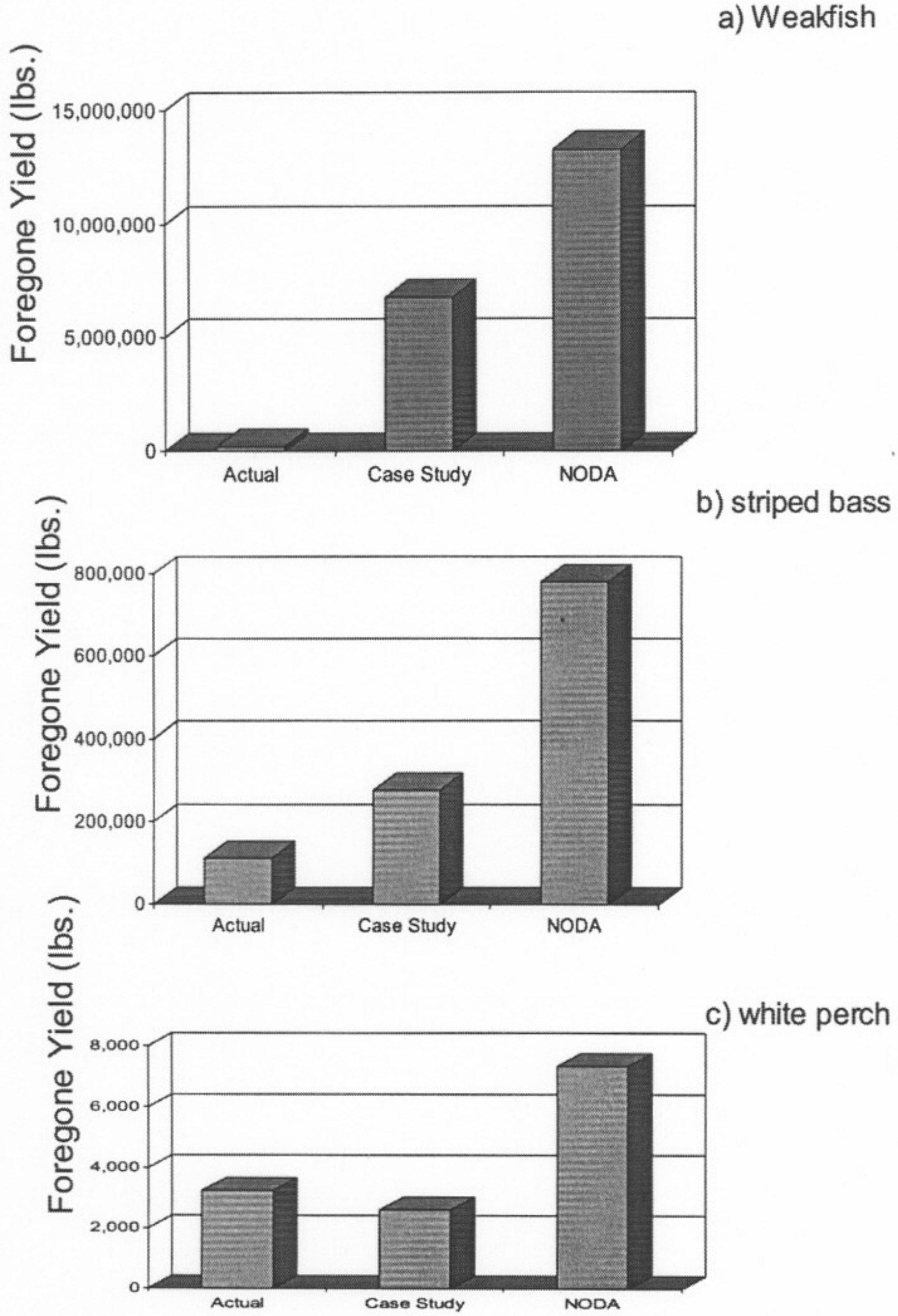

Figure 4. Comparison of foregone yield estimates calculated using the impingement loss estimates from Figure 1.