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**ALTERNATIVE MITIGATION MEASURES  
CLOSED COOLING SYSTEMS ALTERNATIVE**

**BOWLINE ELECTRIC GENERATING STATION  
ORANGE & ROCKLAND UTILITIES, INC.  
AND  
ROSETON GENERATING STATION  
CENTRAL HUDSON GAS & ELECTRIC CORP.  
AND  
INDIAN POINT 2 NUCLEAR GENERATING STATION  
CONSOLIDATED EDISON COMPANY OF NEW YORK  
AND  
INDIAN POINT 3 NUCLEAR GENERATING STATION  
NEW YORK POWER AUTHORITY**

**MAY 1993**

**STONE & WEBSTER ENGINEERING CORPORATION**

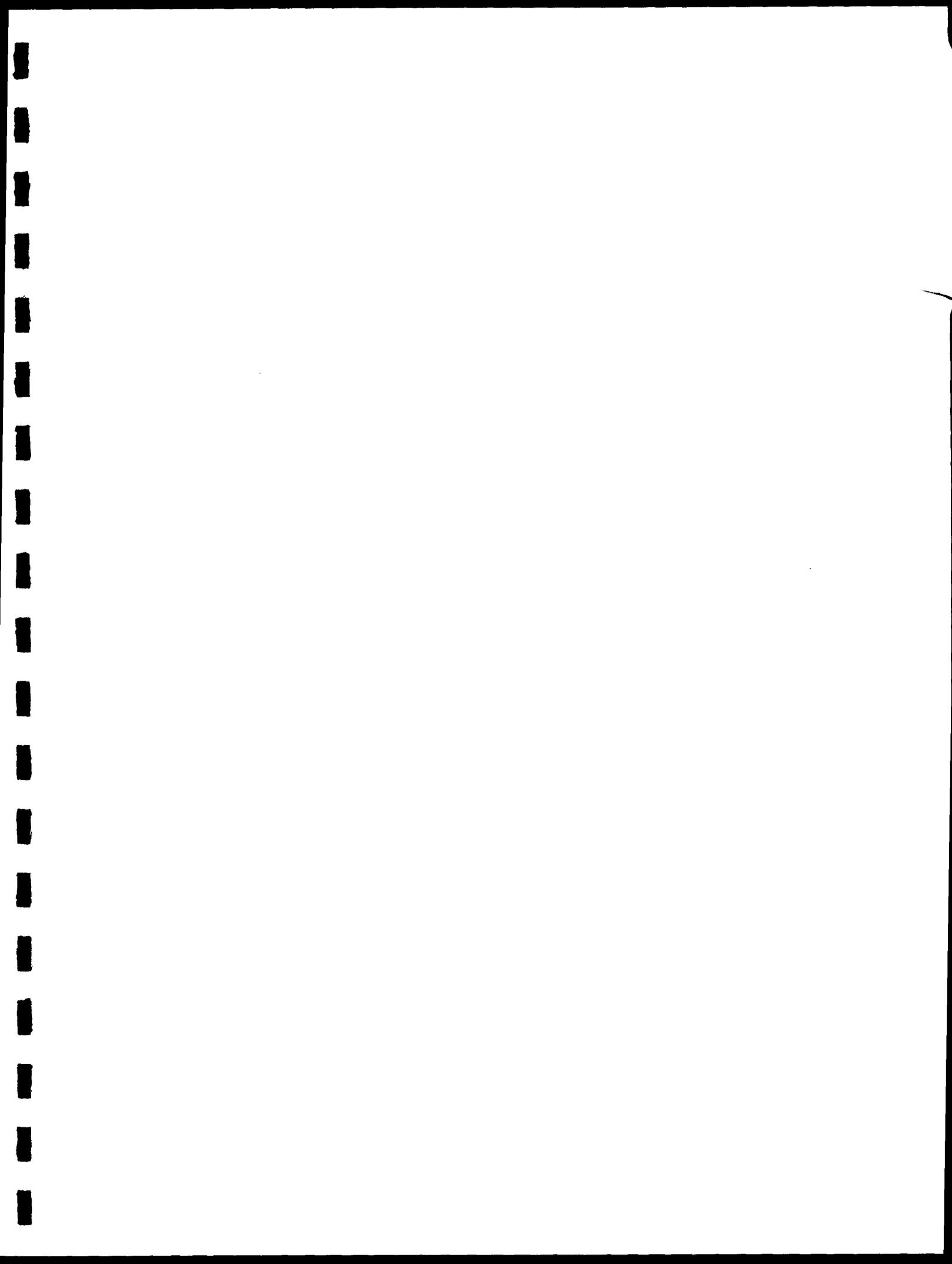
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## 1.0 INTRODUCTION

### 1.1 Objectives

Plant cooling requirements at Indian Point, Bowline and Roseton are currently supplied by large quantities of Hudson River water. The purpose of this study is to discuss the engineering, environmental and cost aspects of all commercially viable alternative closed-cycle systems that employ significantly less quantities of river water. The report also updates the information and conclusions of the various previous studies (Reference 1 to 5) as is necessary and appropriate.

### 1.2 Current Cooling System Technologies

The boilers of the Bowline and Roseton Stations and the steam generators at Indian Point produce steam at high pressure and temperature. That energy is converted to mechanical energy in the steam turbines which turn the electrical generators of the respective plants. The steam exhausting from the last stages of the low pressure turbine is indirectly cooled by water in a heat exchanger termed a condenser. The condenser recovers the pure condensate which is recycled back to the boiler or steam generator for continued power production. The quantity of water used by the condenser in this cooling process is immense and in the existing once-through design it releases its heat to the environment by mixing with the Hudson River.

This study evaluated wet, dry and wet/dry alternative cooling systems for the Hudson River plants. These alternatives are briefly described in the following paragraphs.

Wet cooling towers cause the warm water from the condenser to reject its heat to the surrounding air directly. The cooling effect is enhanced by the concurrent evaporation of a small fraction of that water. By breaking the flow of warm water from the condenser into small droplets and causing ambient air to contact the resulting exposed surfaces, efficient cooling occurs. To be more specific, in a modern

counterflow tower of the type likely to be utilized in New York the warm water from the condenser is distributed across the top of the cooling sections, i.e., the fill, by spray nozzles. (Note that only the mechanics of counterflow towers are described. These towers have been exclusively utilized during the past decade by new utility projects because they are energy efficient and can achieve a close approach. Only in special circumstances of significant silt or macro-fouling would the open crossflow tower be specified and installed. Thus, the discussion of towers in the case of the New York utilities can be limited to counterflow designs; all other discussions are really irrelevant.) As the warm water falls through the fill in sheets and droplets, it is constrained to contact ambient air flowing upward that has entered the tower through wide inlets below the fill. A small portion of the water evaporates in the process. The cooled water rains into and collects in the basin directly below the fill. At the same time, the heated ambient air which has been totally saturated with water vapor, exhausts from the top of the tower and is termed the plume. When this process is accomplished and the fresh air flow is created by a multiplicity of fans it is called a mechanical draft design. When the air flow is generated by the chimney effect of the huge hyperbolic shells, it is termed a natural draft design. In either case, the cooled water is then recirculated back to the condenser with a relatively small quantity of water added from the river to make up for the evaporative losses associated with the cooling process and for the loss due to blowdown which is required to prevent any heavy concentration of the salts that were originally in the Hudson river.

The heat transfer process of wet cooling towers, as indicated, involves significant evaporation. This causes the cooled water temperature achieved by the tower to be higher than the ambient wet bulb temperature. The latter, an atmospheric temperature equivalent to the dew point at 100% relative humidity, is almost always higher than the river temperature at the same season of the year. The difference between the cooled water temperature and the wet bulb temperature is termed the approach. The approach will be at least 10°F during warm weather to as much as

40°F during cold weather seasons. That inherent approach combined with the lesser quantity of circulating water used when retrofitting a cooling tower, causes a higher turbine exhaust pressure. The higher pressure reduces turbine generation. Further, extra auxiliary power is needed to operate all the numerous, giant fans (if applicable) and to convey the recirculated water through the condenser to the top of the fill section of the tower, and back again. This total loss of station capability is called derate.

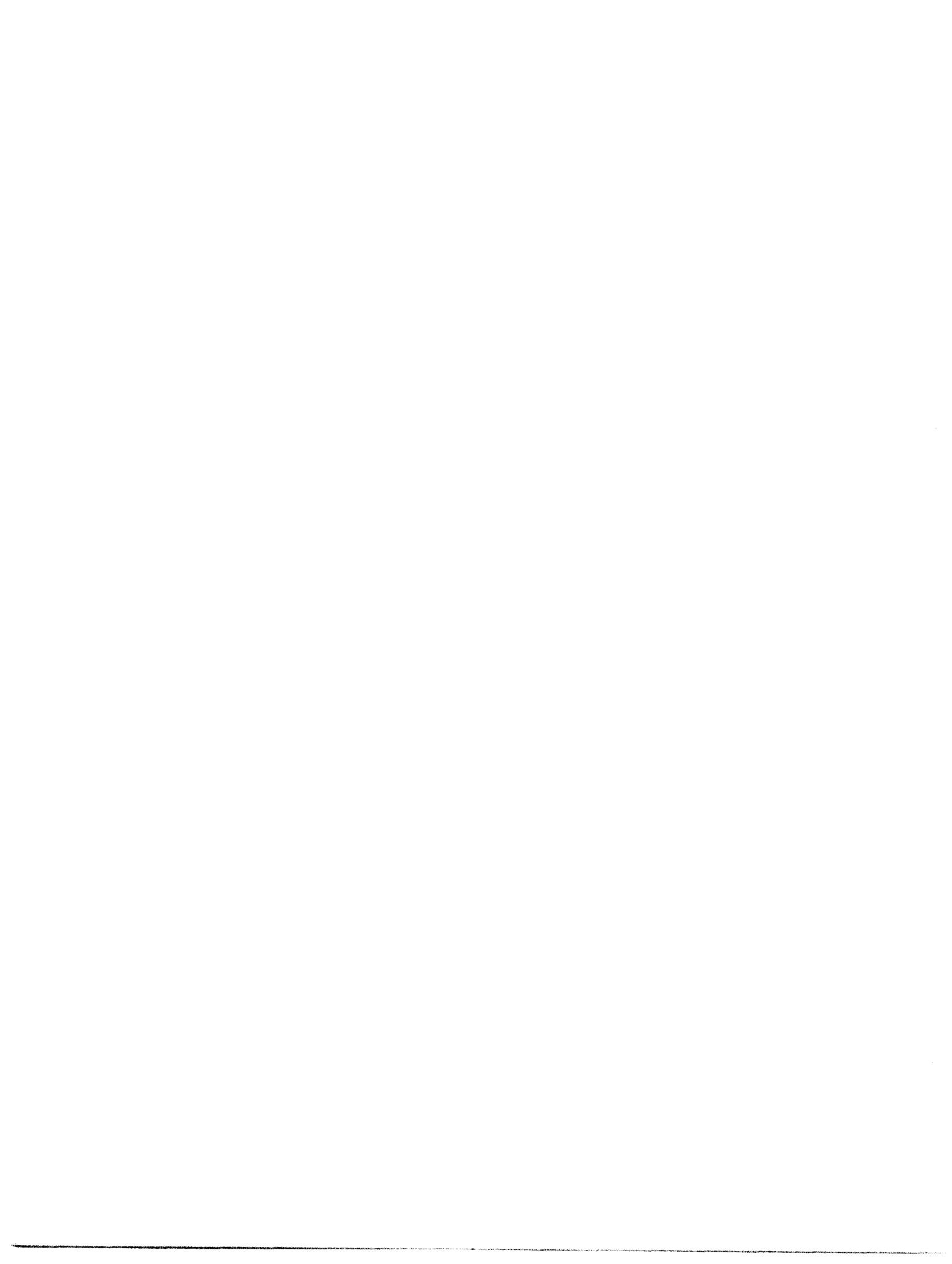
The mechanism of heat release to the air from the recirculated warm condenser cooling water stream, as outlined in the above paragraphs, is essentially descriptive of other methods of evaporative cooling such as spray ponds, spray cooling canals or cooling lakes. However these latter methods are much less effective at producing the large water surface required for the evaporation and/or in providing the enormous fresh air quantities necessary to cool those surfaces. Since their application requires the dedication of land areas that exceed site availability at Indian Point, Bowline and Roseton, they were not considered further.

In another closed-cycle cooling alternative, the water which cools the turbine exhaust steam in the condenser releases its heat directly to the environment by passing through a heat exchanger called a dry cooling tower. It is comprised of finned tubes and components similar to that of an automobile radiator but enormous in all respects. Heat transferred to the surrounding air occurs by the difference in temperature between the warm condenser water and the relatively cooler ambient air which is caused to flow over the fins. Because this process does not employ any evaporative cooling, river water makeup is not required and a SPDES permit is not necessary. While many environmental effects normally associated with wet cooling towers are not present, these dry towers are significantly larger and cannot achieve the same degree of cooling as wet systems. This causes important annual reductions in plant generation and increases operating and capital costs.

The closed-cycle dry cooling tower option for Indian Point, Roseton and Bowline stations is discussed more completely in Section 6.

One other closed cycle-cooling option which was considered in the referenced reports was the hybrid wet-dry cooling system. As its name indicates, it combines the features of wet and dry cooling towers. It was evaluated in this 1993 report update for the Indian Point, Bowline and Roseton sites with the following considerations in mind:

- It typically withdraws river makeup and rejects blowdown quantities that are only slightly lower than those of entirely wet systems.
- The plume incidence from these towers is only marginally less than that for entirely wet cooling system.
- The size is much larger than a comparable wet cooling system and it requires more auxiliary power to operate the fans and pump cooling water so plant generation is reduced.
- The maintenance and capital costs are greater than a wet cooling system. There are more fans and the fin tube dry section tends to foul and corrode.
- The design reduces station output during portions of the year due to the existence of the dry section which reduces the effectiveness of the cooling process.
- The experience in the industry is very limited with wet-dry systems. In the late 70's there were expectations this equipment would be commonplace for application to plants like Indian Point, Bowline or Roseton. However, that has not occurred in the U.S. Only one wet-dry tower of comparable size has



been applied and that to a plant in the southwest with markedly different weather and application circumstances.

Since it has no significant actual advantages, limited actual usage, and since it exhibits all the worst characteristics of the wet and dry cooling systems, it was not evaluated further as an alternative closed-cycle cooling system in this study.

To summarize, only wet natural draft and wet mechanical draft towers were considered for those cooling alternatives that requires a SPDES permit since they represent mature and commercially, reliable large-scale designs.

### 1.3 Environmental Aspects - An Overview

Wet cooling towers will reduce the usage of Hudson River water by the plants to 2 or 3% of the current values and thereby reduce aquatic entrainment by a similar amount. However, towers do have strong inherent environmental impacts. Some of these effects can be generalized or quantitatively characterized; others are related to the specific kind of cooling tower and the specific local site conditions and environs. The former will be addressed in this section while the latter will be reserved for the particular plant discussions.

One general effect is the aggregate vaporization of the fresh water component of the Hudson River. On an annual basis from all the towers it amounts to over 13 billion gallons, a value also equal to 40,000 acre-feet. Though this quantity of water is immense, it is not likely to change the weather or move the Hudson River salt front measurably. To provide a perspective however, the above evaporative consumption is as much as the supply needs for a city of 300,000 people and it also equals covering the entire state of Rhode Island with one-half inch of water.

In addition drift, small airborne droplets of cooling tower basin water, is continually emitted in the wet cooling tower exit air plumes. The drift droplets are comprised of concentrated chemicals, salts and solids that are constituents in the original makeup water. Potentially, PCB's and other pollutants may also be present if they were present in the makeup water at the time it is pumped into the plant. These drift particles eventually reach the ground and also contribute to airborne concentrations at ground level. Natural draft towers tend to disperse this drift widely because of their elevated exit while mechanical draft towers have more intense local effects. In some instances, it can cause harm to certain sensitive trees and foliage. The drift is a small value, a total of less than 50 GPM for all the plants, but it is persistent and not likely to be eliminated by future technical improvements. Its long term consideration is an important matter.

Icing and fogging from the plume often recur downwind of towers during certain atmospheric conditions that are conducive to these formations. This occurs frequently during cool or cold weather seasons. Natural draft towers seldom are responsible for ground level incidents; but mechanical draft towers with their lower, extensively spaced discharges have significantly more troublesome visibility, icing and fogging events depending on the wind direction and location of the tower.

Noise is another effect that is a pervasive and persistent emittant from both types of towers. At property lines the sound equivalent of natural and mechanical draft towers may be 60 dBA, the sound equivalent to that of working inside the average factory or hearing an approaching subway train, 24 hours a day, 7 days a week. It is slightly higher from mechanical draft towers because of their fan noise. Though much discussion is made of attenuating that noise most reduction techniques involve some form of large scale blockage barriers. This however runs counter to the tower design performance requirements of providing an easy entry and exit for the admission of cooling air. Hence real costs are very high for noise treatments because they must account for both the hardware and the compromised cooling performance.



Blowdown discharge contains concentrated levels of salts and chemicals present in the makeup water. Its temperature will generally be elevated above the river temperature by 10 to 30°F reflecting that of the cooling tower basin at the particular moment. In terms of quantity it will be about 1 to 2% of the flow circulated in the plant.

Sludge develops in the basin from silt and heavier suspended solids in the makeup. It will reflect the character of the Hudson River at the location and particular time of the makeup. Silting will vary seasonally and is generally very site specific. If severe, it could interfere with operations and cause reduced thermal performance. As had been indicated for drift, it potentially could contain PCB's and other pollutants. The sludge must be properly managed and in the future, the costs for testing, removal and proper landfill disposal could be substantial.

Visually, the natural draft towers will be taller than other current station structures. Along with its associated plume it will be very evident. The mechanical draft towers have a lower profile, similar to that of other plant buildings, but are much more extensive than the natural draft designs. The visual impact of the mechanical draft tower however should be evaluated with their frequent lengthy plumes when comparing them with the effects of the natural draft design.

Both types of towers use significant amounts of land and thus preclude any future necessary alternate plans.

To compensate for the reduced performance, resulting from retrofitting cooling towers, replacement generation must be created. The replacement plant generation will likely utilize fossil fuels and thereby cause added airborne emissions of CO<sub>2</sub>, SO<sub>2</sub> and NO<sub>x</sub>. This would also produce environmental impacts similar to those associated now with the fossil Hudson River plants.

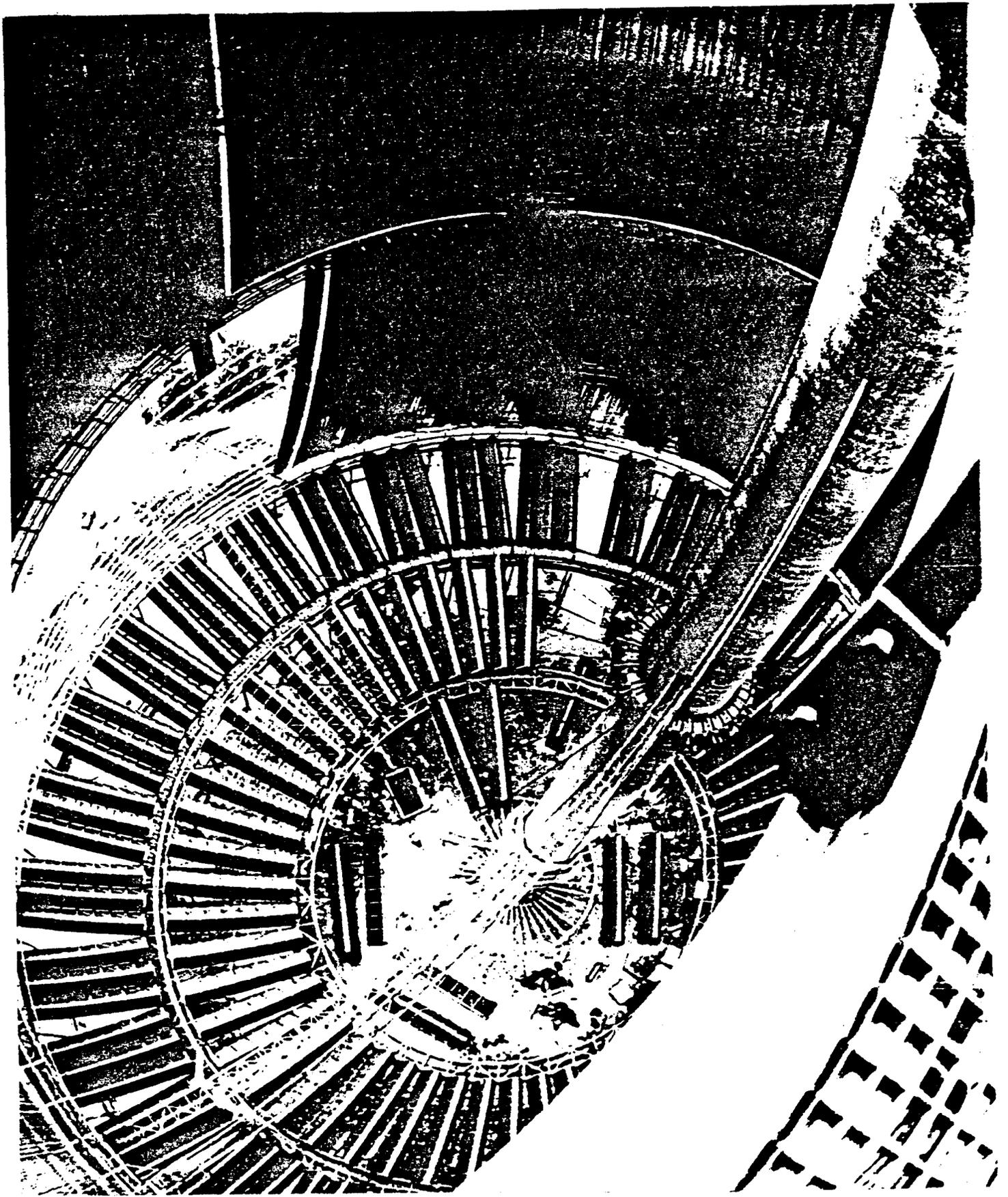
#### 1.4 Study Approach

Wet cooling tower engineering design was originally defined early in this century by mathematical formulations of the physics of the simultaneous sensible and evaporative heat transfer processes which occur. Those theories remain valid to this day. Though there have been some technology improvements - notably to fill, fans, drift eliminators, and materials of construction - a large cooling tower purchased today would be very similar to one of 20 years ago.

Thus this study was able to take advantage of the extensive past work efforts on previous conceptual closed cycle plants, their engineering and estimates. In fact this allowed the study to effectively focus on past results, planned schedule and cost estimates and only revise and update to 1993 any outmoded data. These past costs were then escalated using accepted methods that are appropriate for the industry. They are listed in the appropriate sections for each site.

In addition, though there have been advances in materials, wet natural draft cooling towers still rely on concrete shell construction. That material is strong, permanent, relative easy to form, and impervious to the corrosive effects of the warm, and sometimes saline, circulated water it contains. Fabric cooling tower shell materials can reduce the costs of this expensive component but are only applicable and have only been used for dry cooling towers since they do not operate in the same potentially corrosive environment. One application shown in Figure 1 is a photograph of the inside of a fabric tower in construction at the Schmehauser Site, a 300 MW fossil plant in Germany. The dry tower "A" frame finned tube headers are clearly shown in the photograph. As indicated, the fabric cooling tower is not a design that is relevant or applicable to the wet cooling towers of the Hudson River plants.

Retrofitting closed-cycle cooling system may appear to be a simple task, in actuality, the fundamental differences between the once-through and closed-cycle cooling system, the permanence of the construction of the existing massive structures, the optimum economic



**Figure 1**

**VIEW INSIDE 'FABRIC' DRY COOLING TOWER  
300 Mega Watt Fossil Plant-Schmehausen Site**

balances originally struck between the cooling system requirements and the overall station generation, make retrofitting a closed-cycle cooling system a major engineering, design and construction task.

It should be recognized that since a cooling tower recirculates less water and also produces warmer water than the current once-through cooling, the steam turbine exhaust pressure increases, lowering plant output. In addition, the auxiliary load to operate the station increases: first, due to the higher pumping power necessary to convey the closed-cycle water a greater distance to the towers, to the top of the fill and back to the station; and second, in the case of mechanical draft systems, to operate the many fans that are needed to provide the cooling air supply of the tower. So besides the planning, design and construction of the tower and the permitting process (which could be a very long and costly process when trying to permit a cooling tower along the Hudson River, especially for a nuclear power plant with its own extensive licensing requirements) the major items that complicate and make retrofitting costly which are addressed by this study include:

1. Station performance penalties.
2. Demolition of existing circulating water intakes, discharges and obsolete runs of piping.
3. Replacement of circulating water pumps and construction of new pump houses and intake bays to accommodate the higher energy demands.
4. Condenser modifications including on-line cleaning systems and possible reinforcement to withstand higher hydraulic pressures and reduced flows.
5. Fabrication of new high pressure piping to the cooling tower.

6. Installation of new electrical services, motor control facilities, chemical treatment, new makeup and blowdown systems.
7. Scheduling cut-in construction, testing and start-up of the closed-cycle system to have only a relatively short term disruption of station generation.

The last paragraph noted that retrofitting a cooling tower will cause a loss of net station generation. There will be a reduction in turbine performance due to the warmer level of cooling water (compared to Hudson River water temperatures at the same time) and due to the extra power needed (for fans and/or conveying cooling water a larger distance and up to the hot water distributor of the tower). The total loss is termed a derate. The following paragraphs discuss specifically how the derate was estimated in this study.

First, the results of the original studies were reviewed to ensure that estimates of the extra auxiliary power identified above were reasonable and still appropriate for 1993. Since the cooling towers, quantity of water and locations of equipment were essentially the same for the original and present study; and because the state-of-the-art has not improved measurably with regard to auxiliary power requirements in the intervening years, these former values were determined to be still relevant.

Second, the original studies indicated the wet towers use of asbestos fill had lifted the performance levels and included several depictions of the cooling towers. Based on that information, and a recollection of cooling tower manufacturers offerings for large utility applications conclusively ascertained that these early studies employed data for film fill, counterflow, natural draft cooling towers and crossflow mechanical draft towers. In fact, today's natural draft tower study candidates would be basically the same as those of the mid 70's. The mechanical draft tower for utility application today would however more typically be counterflow using a PVC film fill that had provisions to prevent fouling of the natural surfaces which causes the fill to be deeper.

Because the total pumping head for the system is comprised of that through the condenser, valving, lines to and from the tower, over the air inlets up above the fill and across the low head distributor nozzles, a change in auxiliary pump power between the old and present studies due to an improved fill is minimal. Thus, between the old and present studies the pump power estimate is within the accuracy of this study. Accordingly, the past study pump power was used and when applicable, the significant fan power requirement, which has not changed since the mid 70's, was added.

Due basically to a shift to a counterflow design, the performance of a mechanical draft tower has improved since the original studies. Though the size of the tower has remained about the same, the close up achievable approach of the tower to the design wet bulb temperature has been importantly reduced. This effect was recognized in the current study.

With the above information, the performance loss of the turbine was determined by computing the corresponding changes in turbine exhaust saturation temperature and then the pressure. The incremental heat rate curves estimated for that turbine were then examined to determine the impact on station generation. The original capability loss figure was then modified accordingly. As stated, since the natural draft tower performance is expected to be the same, no change in capability losses was necessary. If a power level increase had recurred at the station, this loss was included. The auxiliary power requirements due to the retrofit were then added to obtain the total station derate.

It is also important to recognize that although the construction of the cooling tower will be basically independent of the plant operations, timing the tie-in to coincide with a moderate load period, weather, etc. will require a complex, sensitive plan. The tie-in activities are many and will take an appreciable time to complete. These activities include, excavations, installation of a security fence, disconnection and hook-in of buried and overhead electrical services, installation of control and monitoring systems, demolition of affected existing circulating water lines, construction of thrust blocks, construction of foundations in the area of tie-ins, modification of the inlet for makeup, modifications for a blowdown facility, tests

and modifications to the condenser for a tube ball cleaning system and to accommodate the potential for higher hydraulic pressures, innumerable tests and start-ups for valves, pumps and components of the circulating water system, pump performance capacity tests, and finally tests of the detailed components of the cooling tower and its physical and hydraulic compatibility with the entire system.

Two closed-cycle cooling system designs not considered by this study are the round wet mechanical draft design and the dry natural draft cooling tower. The round wet tower is significantly more expensive than the wood design and has a limited size range. While this tower has some minor engineering advantages, the rectangular tower sufficiently characterizes the wet mechanical design. The dry natural draft tower is not a commercially viable, tested design in the U.S. It has also not been considered due to its immense size such that it has no comparable construction history in this country.

To put this study in perspective, it should be understood that retrofitting closed cycle cooling towers has only happened a few times. The plants where this occurred were much smaller. A project of this magnitude at any one of the large plants of this study is unprecedented. To retrofit all six Hudson River plants will be an enormous and perhaps chaotic construction effort.

Modern day cooling tower technology compared to the 1970's cannot be portrayed as one which has significantly reduced sizing, costs and energy usage. That simply hasn't happened. Budget estimates of costs and sizing received from three cooling tower manufacturers for the Hudson River plants were utilized in this study. These costs were similar to ones which could have been developed by escalating, the original mid 70's costs at 5.5% inflation. This indicates there was no decrease in basic pricing. Further, the tower plot size was about the same as that of the early studies.

As far as energy is concerned, a comparison of the fill heights of difficult (close approach) thermal duty crossflow towers of the 70's would show them to be much higher than the



equivalent approach modern counterflow tower. But the original 17°F approach towers of the earlier study are not difficult duties. Besides, the fill height is only one component of the total hydraulic pumping head. Sufficient pump power is needed to force the circulating water through the condenser tubes, the valves, lengthy piping to and from the tower, up over the counter flow air inlets, above the required modern non-fouling film fill and through the low head distribution nozzles. Thus, the pump power can essentially be considered to be the same. In addition, the fan power in the experience of those in the cooling system field still is about the same. Improved heat transfer from modern film fills occurs due to better air-water interaction. This same mechanism actually increases unit pressure drop that the cooling tower fans must overcome.

With regard to other types of tower designs, the former USSR research idea of a trifolium tower design and fabric shells are not likely to be of use for the Hudson River plants. This tower is still only a concept and its advantages and disadvantages are likely still under debate in Russia. It is a long distance between that and a viable, commercial U.S. design. The fabric tower has been used for some Eastern Europe plants but its cable support system is subject to the long term corrosion effects of the salt and chemicals in wet cooling plumes and drift. Thus, the application of fabric shells for some time will be confined - as it has been - to dry natural draft cooling towers where no extraordinary drift problems recur.

The use of wet/dry towers is rare. In fact, the latter are so rare that there is only one such tower at a plant that is slightly smaller in size to the smallest of the Hudson River plants. No manufacturers have made any recently. It is installed in the southwest in a climate much different than New York. Due to this rarity it is unlikely that the capital or operating costs could be even remotely estimated. The degree of plume avoidance ability is proportional to the dry finned tube surface. This could significantly impact the plant performance, increase cost and be a costly maintenance item due to the corrosion and side effects of the wet plume on the fins.



## 2.0 INDIAN POINT 2 ALTERNATIVE CLOSED-CYCLE COOLING SYSTEM

### 2.1 Summary Description of Existing Plant

The Indian Point Power Station contains two units - Unit No. 2 is a 1000 MW commercially operating nuclear power plant (pressurized water reactor) owned and operated by Consolidated Edison Company of New York; Unit No. 3 is a 1000 MW commercially operating nuclear power plant (pressurized center reactor) owned and operated by the New York Power Authority. The site is located in Buchanan, New York, approximately 35 miles north of New York City on the east side of the Hudson River. Its features are described in Reference (2) and it began commercial operation in 1975.

The existing cooling system is classified as a once-through system. The Indian Point No. 2 turbine generator converts the thermal energy of steam to electric energy. After all recoverable energy has been extracted from the steam, the residual thermal energy is transferred to the condensers and discharged via the condenser cooling water to the discharge canal.

The discharge structure at the end of the canal is designed to create mixing of the warm condenser water in such a way as to minimize water temperature differences in the Hudson River. It will accommodate the combined cooling water flow from both active Indian Point Units and at full flow amounts to about 850,000 gpm including service water from each station. This outfall structure is 270 ft. long and contains a series of submerged ports.

Both intake and discharge structures are located essentially on the river edge. Large pumps within the intake structure pump the water to the condenser in the main plant area via buried pipes and that water is conveyed back to the river edge discharge structure in a below-ground canal.

The current Consent Order from the Supreme Court of the State of New York mandates the station to operate at the minimum condenser cooling water volume required for efficient operation taking into account ambient river water temperatures, operating status and the need to meet all water quality and permitting conditions. These flows are defined in Figure 2, taken from Table B-1 of the Consent Order. Other stipulations of this consent order can be found in Reference (6), the Order itself.

## **2.2 Natural-Draft Cooling Towers**

### **2.2.1 Engineering Aspects**

The natural draft tower design for the Indian Point 2 Station is the same as that determined in Reference 2. One tower is provided (Figure 3). It has a reinforced concrete shell of hyperbolic shape. The base diameter is 462 feet and the overall height 565 feet. The tower design point is for a circulating water flow rate of 600,000 gpm, a range of 25 °F and an approach of 18°F at a 79° wet bulb and 50% relative humidity.

To maintain the concentration of the circulated station water at reasonable levels, approximately 8,000 gpm of blowdown water per unit would be discharged to the Hudson River. At the design point, about 12,000 gpm is lost through evaporation, as the thermally induced air flow comes in contact with the warm circulating water sprayed in the fill. Approximately 12 gpm of water would be lost through drift. The latter is the classification of water droplets which become suspended in the air flowing through the tower and which are deposited on the surrounding land. All categories of water usage are replaced by makeup water drawn from the river.

### **2.2.2 Cost Estimates**

Section 1.4 had indicated why the cost of the cooling towers is only a small fraction of the overall cost of retrofitting. The capital costs of the system using the wet natural draft cooling tower are listed in 1993 dollars as follows:

**Figure 2 PREDICTED CONDENSER COOLING WATER FLOW RATE SCHEDULES TO ACHIEVE EFFICIENT OPERATION OF INDIAN POINT UNIT 2**

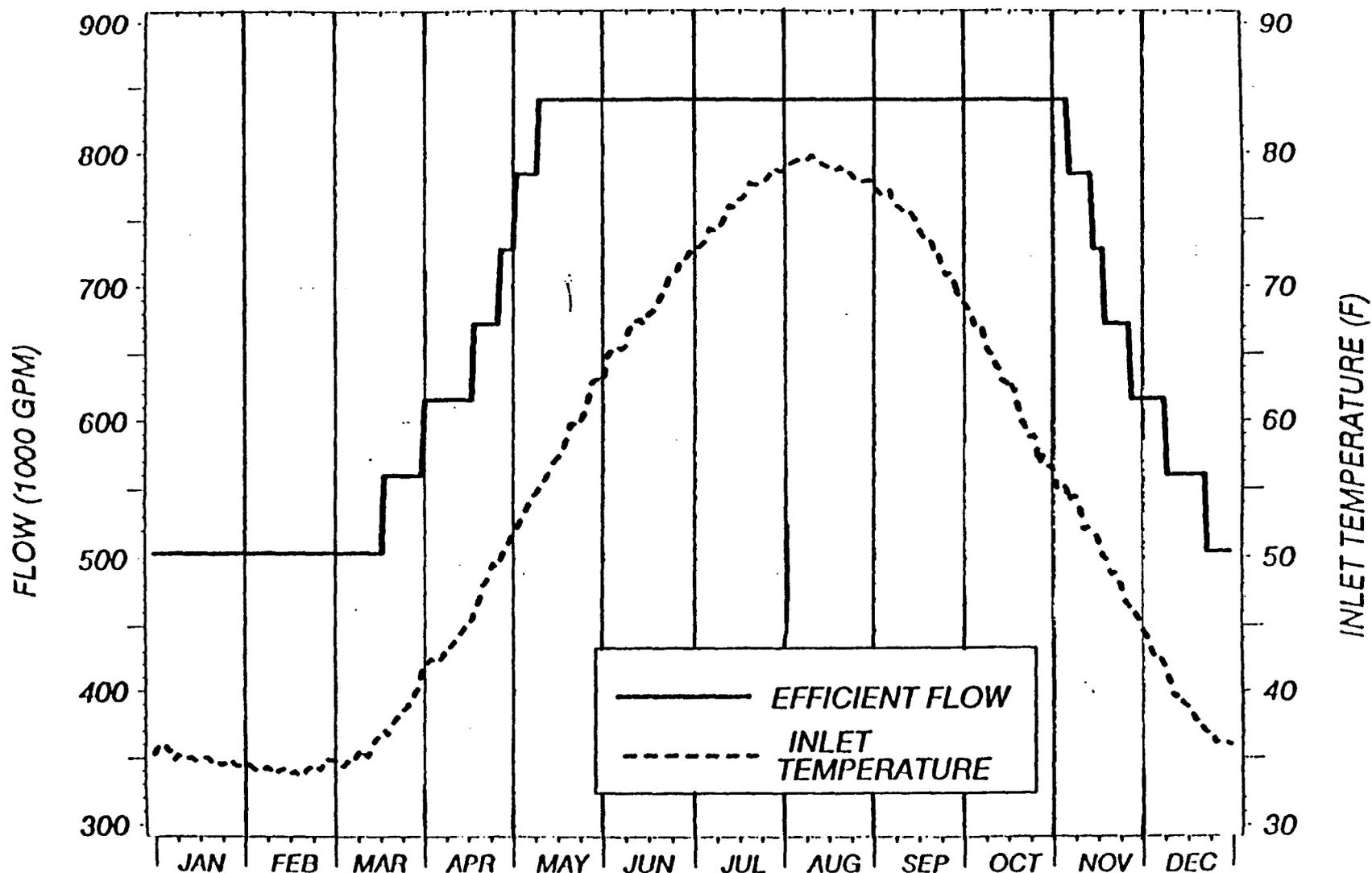
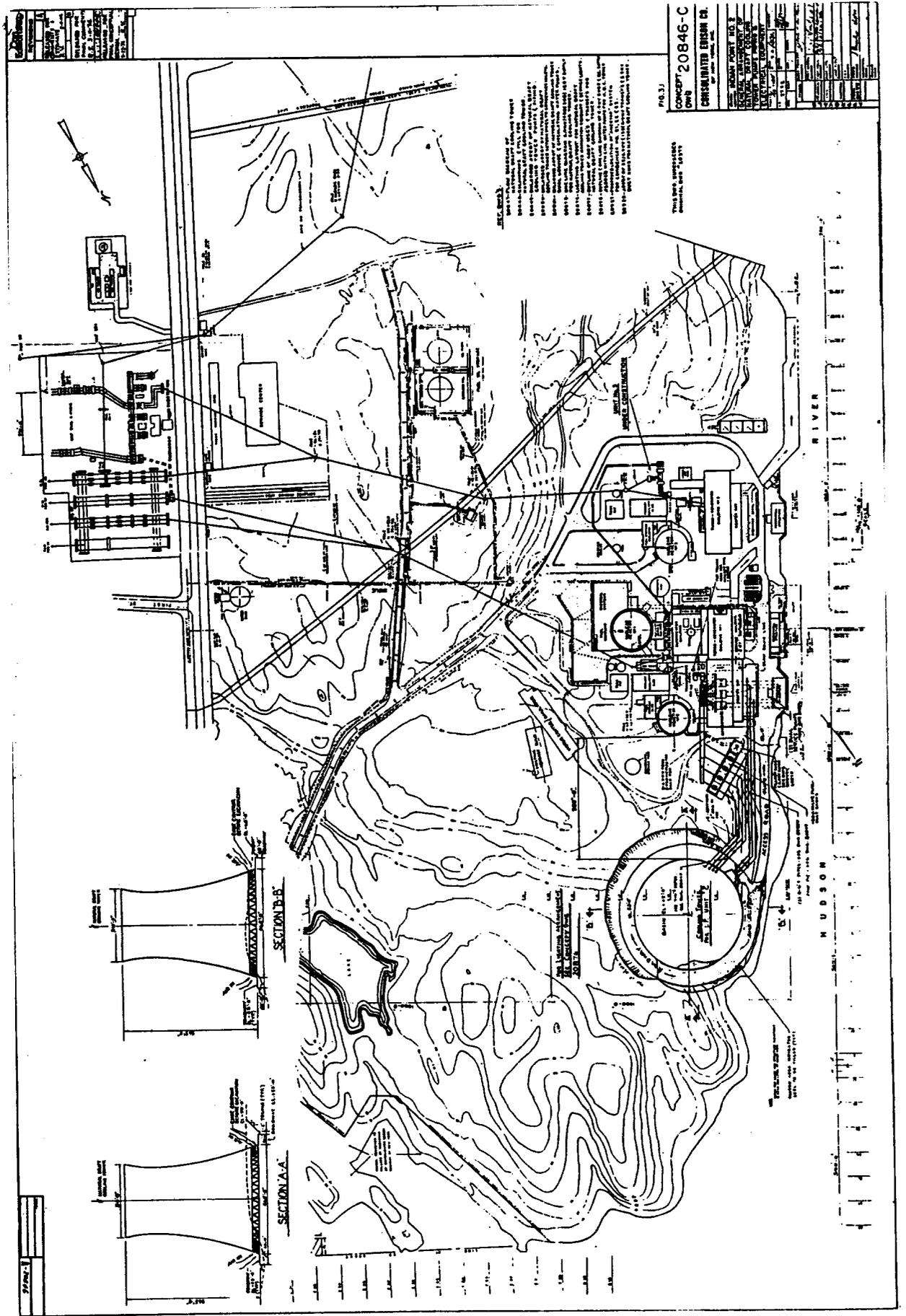


Table B-1

**PRIVILEGED AND CONFIDENTIAL  
FOR SETTLEMENT PURPOSES ONLY**

Figure 3 INDIAN POINT 2 SITE PLAN - NATURAL DRAFT TOWER



Cooling tower - erection, labor and materials	\$ 26,100,000
Amertap	\$ 8,651,000
Piping	20,036,000
Structural	36,174,000
Electrical	5,683,000
Project Management	2,979,000
Indirects and Professional Services	<u>11,900,000</u>
<b>TOTAL</b>	<b>\$111,500,000</b>

These costs are consistent with previous costs of Table 5-1, Ref. 2. Cooling tower costs shown however are based on 1993 data supplied by cooling tower vendors. All other inherent construction and installation costs of the 1977 studies and including current site drawings were reviewed and found to be appropriate. They were escalated and updated from the 1977 studies to reflect the 1993 material and labor costs. An average escalation rate of 5.5% was assumed from 1977 through 1993. The construction schedule will be approximately 3½ years, as indicated in Ref. 2.

Section 1.4 also discusses the kinds of new added operating costs and the related loss in station efficiency when retrofitting cooling towers. The capability losses of this alternative for Indian Point 2 follow. They are based on Table 3-3 and Table 5-5 of Ref. 2.

#### Plant Capability Losses

Loss of turbine capability - MW	15
Cooling System Auxiliaries - MW	12
Total Derating - MW	27
Max. Peak Temperature Derate - MW	68

### 2.2.3 Environmental Aspects

A natural draft cooling tower has effects on air quality, ambient noise, terrestrial ecology, water quality, aquatic ecology and aesthetics. The major effects are discussed in Reference 2 and summarized below.

- A) Ground fog incidence is only one event in 11 months; this is inconsequential in comparison to the natural fog incidence of 2%. No icing incidence will occur.
- B) Drift dispersion from a natural draft tower is very wide spread. It results in salt deposition and saline air concentrations that are relatively small. Those studies also indicate most plants in the Indian Point area will not be injured with the possible exception of hemlock, flowering dogwood and white ash. As reported in these very extensive studies, the potential for biological damage is improbable unless the ambient conditions described in Reference 2 persist.
- C) Blowdown chemistry could be diluted in the discharge canal before discharge to the river. All discharge water chemistry, including intermittent chlorine, will be within EPA mandated guidelines. Extensive bioassay data on the aquatic populations is included in the referenced 1977 studies.
- D) Noise input studies during construction and operation is expected to be lower for natural draft towers and less localized.
- E) Due to its size, a natural draft tower will have a major visual and aesthetic impact on surrounding areas. It will render ineffective Indian Point efforts at maintaining a low profile.

- F) The makeup of a closed cooling system is about 2% of the existing once-through flow. This reduces the entrainment of aquatic life appreciably as Reference 2 shows. Thermal heat to the Hudson River is also reduced proportionally.
  
- G) Installation of a closed-cycle cooling system necessitates replacement of the fossil power generation due to the amount of the Indian Point 2 derate and the plant downtime during the cut-in of the cooling tower. This will result in added atmospheric discharges of fuel based oxides and particulates.

## 2.3 Mechanical Draft Cooling Towers

### 2.3.1 Engineering Aspects

The mechanical draft tower design for the Indian Point 2 Station is the same as that determined in Reference 2. Four towers are provided. It is a concrete rectangular cooling tower . The size of each tower is 54' x 486' and it is 33 feet to the fan deck. The tower design point is for a circulating water flow rate of 600,000 gpm, a range of 25 °F and an approach of 10 °F at a 79 °F wet bulb and 50% relative humidity.

To maintain the concentration of the circulated station water at reasonable levels, approximately 8,000 gpm of blowdown water per unit would be discharged to the Hudson River. At the design point, about 12,000 gpm is lost through evaporation, as the thermally induced air flow comes in contact with the warm circulating water sprayed in the fill. Approximately 12 gpm of water would be lost through drift. The latter is the classification of water droplets which become suspended in the air flowing through the tower and which are deposited on the surrounding land. All categories of water usage are replaced by makeup water drawn from the river.

### 2.3.2 Cost Estimates

Section 1.4 had indicated why the cost of the cooling towers is only a small fraction of the overall cost of retrofitting. The capital costs of the system using the wet mechanical draft cooling tower are listed in 1993 dollars as follows:

Cooling tower - erection, labor and materials	\$ 18,000,000
Amertap	\$ 8,651,000
Piping	20,036,000
Structural	39,490,000
Electrical	7,602,000
Project Management	2,556,000
Indirects and Professional Services	<u>11,560,000</u>
TOTAL	\$107,900,000

These costs are consistent with previous costs of Table 5-2, Ref. 2. The cooling tower costs shown are based on 1993 data supplied by cooling tower vendors.

All other inherent construction and installation costs of the 1977 studies were reviewed and including current site drawings, found to be appropriate. They are escalated and updated from the 1977 studies to reflect the 1993 material and labor costs based on an annual rate of 5.5%

Section 1.4 also discusses the kinds of new added operating costs and the related loss in station efficiency when retrofitting cooling towers. The capability losses of this alternative for Indian Point 2 follow. They are based on Table 3-2 and Table 5-6 of Ref. 2.

### Plant Capability Losses

Loss of turbine capability - MW	22
Cooling System Auxiliaries - MW	14
Total Derating - MW	36
Max. Peak Temperature Derate - MW	55

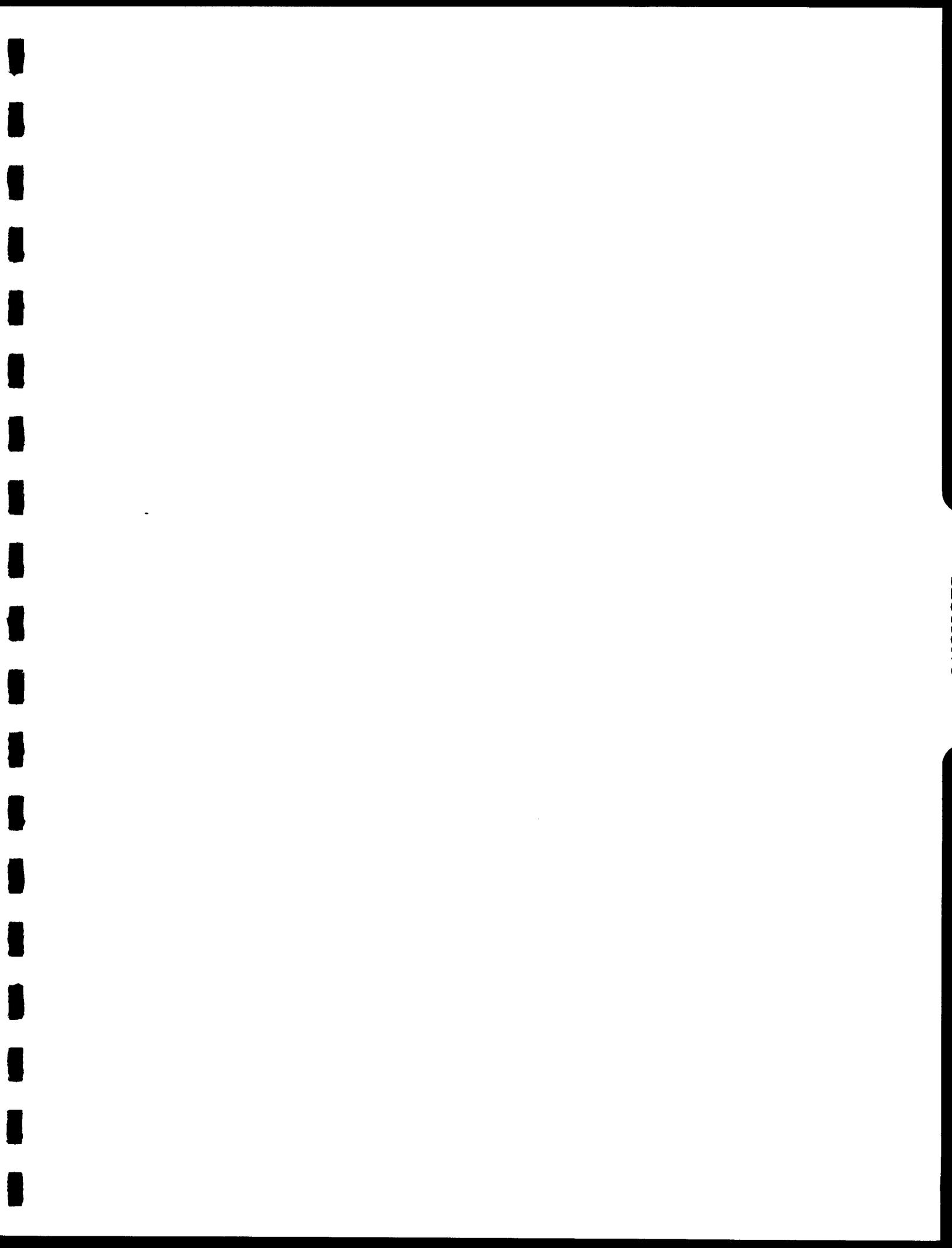
### 2.3.3 Environmental Aspects

A mechanical draft cooling tower has effects on air quality, ambient noise, terrestrial ecology, water quality, aquatic ecology and aesthetics. The major effects are discussed in Reference 2 and summarized below.

- A) Ground fog (plume) incidence are more frequent than in natural draft cooling towers. Icing incidence will occur.
- B) Drift dispersion from a mechanical draft tower is very localized. It results in salt deposition and saline air concentrations that are higher than natural draft. Those studies also indicate most plants in the Indian Point area will not be injured with the possible exception of hemlock, flowering dogwood and white ash. As reported in these very extensive studies, the potential for biological damage is improbable unless the ambient conditions described in Reference 2 persist.
- C) Blowdown chemistry could be diluted in the discharge canal before discharge to the river. All discharge water chemistry, including intermittent chlorine, will be within EPA mandated guidelines. Extensive bioassay data on the aquatic populations is included in the referenced 1977 studies.
- D) Noise during construction and operation is expected to be lower for natural draft towers and less localized.

- E) The makeup of a closed cooling system is about 2% of the existing once-through flow. This reduces the entrainment of aquatic life appreciably as Reference 2 shows. Thermal heat to the Hudson River is also reduced proportionally.
  
- F) Installation of a closed-cycle cooling system necessitates replacement of the fossil power generation due to the amount of the Indian Point 2 derate and the plant downtime during the cut-in of the cooling tower. This will result in added atmospheric discharges of fuel based oxides and particulates.

It should also be noted that the mechanical draft towers have a lower profile, similar to that of other plant buildings, but are much more extensive than the natural draft designs. The visual impact of the mechanical draft tower should be evaluated with their frequent lengthy plumes when comparing them with the effects of the natural draft design.



### **3.0 INDIAN POINT 3 ALTERNATIVE CLOSED-CYCLE COOLING SYSTEMS**

#### **3.1 Summary Description of Existing Plant**

The Indian Point Power Station - Unit No. 3 is a 1000 MW commercially operating nuclear power plant (pressurized water reactor). The site is located in Buchanan, New York, approximately 35 miles north of New York City on the east side of the Hudson River. Its features are described in Reference (3) and it began commercial operation in 1977.

The existing cooling system is classified as a once-through system. The Indian Point No. 3 turbine generator converts the thermal energy of steam to electric energy. After all recoverable energy has been extracted from the steam, the residual thermal energy is transferred to the condensers and discharged via the condenser cooling water to the discharge canal.

The discharge structure at the end of the canal is designed to create mixing of the warm condenser water in such a way as to minimize water temperature differences in the Hudson River. It will accommodate the combined cooling water flow from both active Indian Point Units and at full flow amounts to about 850,000 gpm including service water from each station. This outfall structure is 270 ft. long and contains a series of submerged ports.

Both intake and discharge structures are located essentially on the river edge. Large pumps within the intake structure pump to the condenser in the main plant area via buried pipes and that water is conveyed back to the river edge discharge structure in a below-ground canal.

The current Consent Order from the Supreme Court of the State of New York mandates the station to operate at the minimum condenser cooling water volume required for efficient operation with due regard to ambient river water temperatures, operating status and the need to meet all water quality and permitting conditions. These flows are defined

in Figure 4, taken from Table B-2 of the Consent Order. Other stipulations of this consent order can be found in Reference (6), the Order itself.

## **3.2 Natural-Draft Cooling Towers**

### **3.2.1 Engineering Aspects**

The natural draft tower design for the Indian Point 3 Station is the same as that determined in Reference 3. One tower is provided (Figure 5). It has a reinforced concrete shell of hyperbolic shape. The base diameter is 462 feet and the overall height 565 feet. The tower design point is for a circulating water flow rate of 600,000 gpm, a range of 25°F and an approach of 18°F at a 79°F wet bulb and 50% relative humidity. A current site drawing was reviewed and it was found that the site would be able to accommodate cooling towers, though a future plan for an administrative building may require a slight alteration.

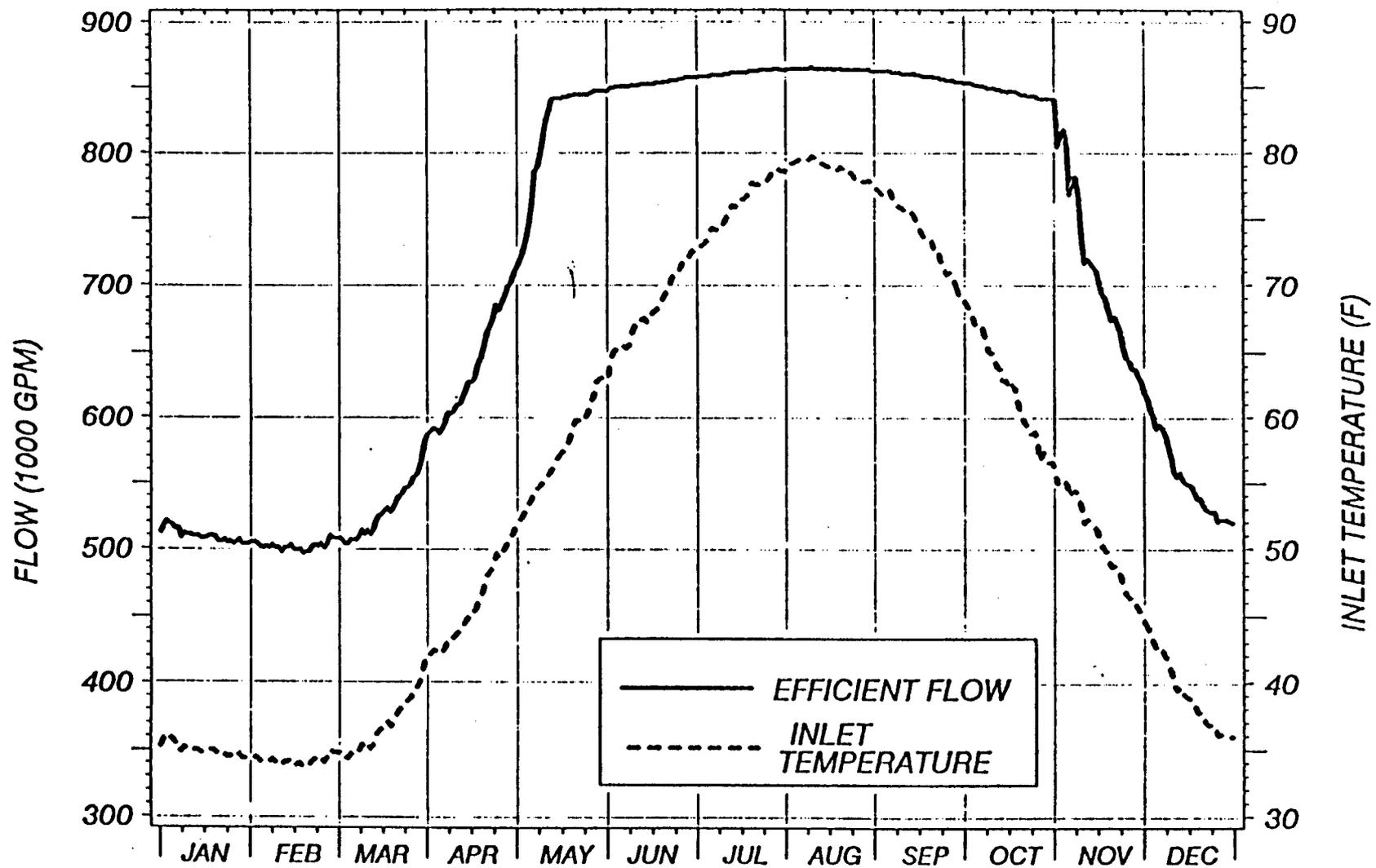
To maintain the concentration of the circulated station water at reasonable levels, approximately 8,000 gpm of blowdown water per unit would be discharged to the Hudson River. At the design point, about 12,000 gpm is lost through evaporation, as the thermally induced air flow comes in contact with the warm circulating water sprayed in the fill. Approximately 12 gpm of water would be lost through drift. The latter is the classification of water droplets which become suspended in the air flowing through the tower and which are deposited on the surrounding land. All categories of water usage are replaced by makeup water drawn from the river.

Construction times for the overall project are estimated to be the same as in 1977, 3½ years.

### **3.2.2 Cost Estimates**

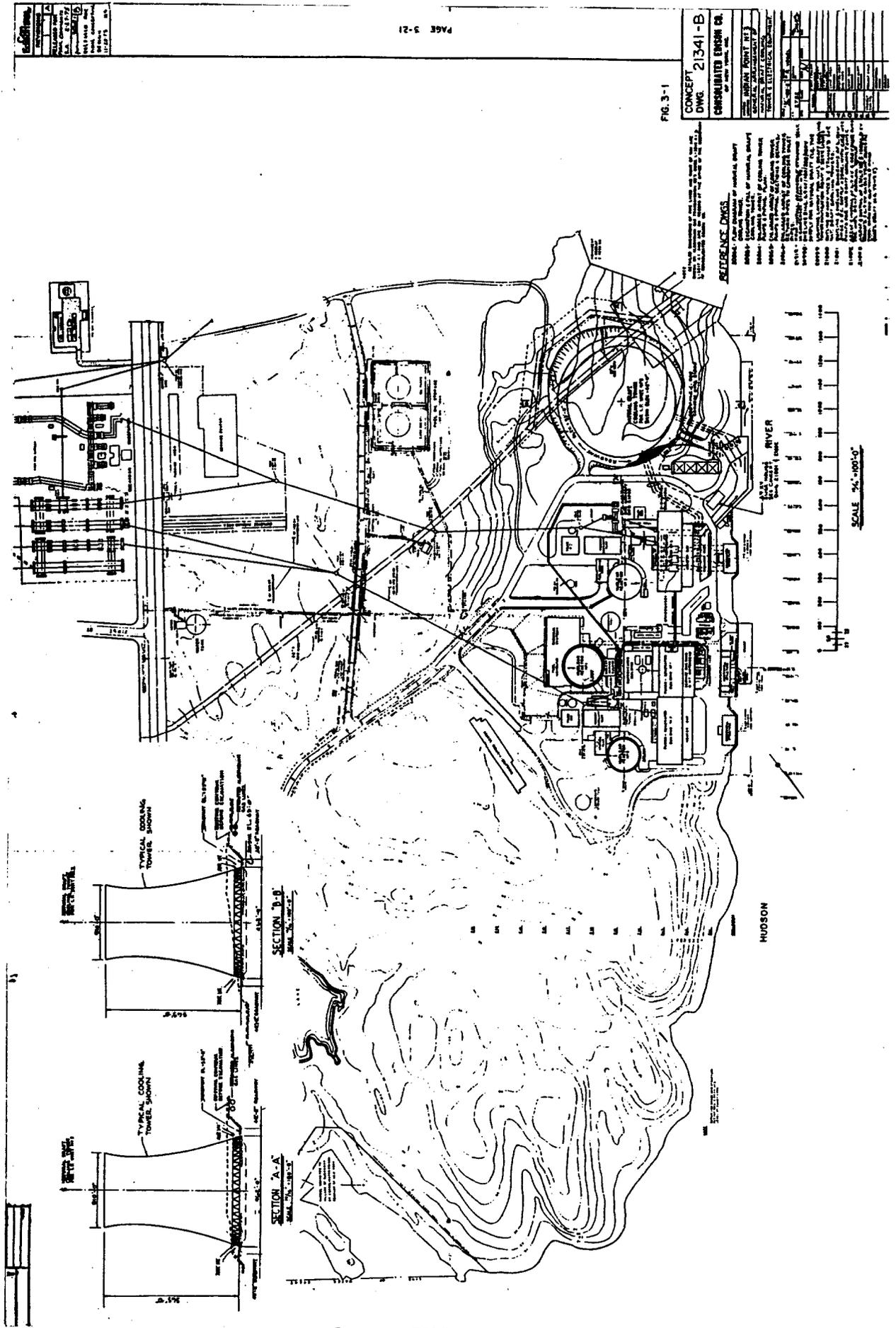
Section 1.4 indicated why the cost of the cooling towers is only a small fraction of the overall cost of retrofitting. An escalation rate of 5.5% was utilized to adjust costs to 1993.

**Figure 4 PREDICTED CONDENSER COOLING WATER FLOW RATE SCHEDULES TO ACHIEVE EFFICIENT OPERATION OF INDIAN POINT UNIT 3**



**PRIVILEGED AND CONFIDENTIAL  
FOR SETTLEMENT PURPOSES ONLY**

Figure 5 INDIAN POINT 3 SITE PLAN - NATURAL DRAFT TOWER



The capital costs of the system using the wet natural draft cooling tower are listed in 1993 dollars as follows:

Cooling tower - erection, labor and materials	\$ 26,100,000
Amertap	\$ 8,651,000
Piping	16,743,000
Structural	46,237,000
Electrical	6,947,000
Project Management	3,528,000
Indirects and Professional Services	<u>2,985,000</u>
TOTAL	\$121,190,000

Section 1.4 also discusses the kinds of new added operating costs and the related loss in station efficiency when retrofitting cooling towers. The operating expenses of this alternative for Indian Point 3 follow. They are based on Table 3-3 and Table 5-5 of Ref. 3.

#### Plant Capability Losses

Loss of turbine capability - MW	16
Cooling System Auxiliaries - MW	17
Total Derating - MW	33
Max. Peak Temperature Derate - MW	77

#### 3.2.3 Environmental Aspects

A natural draft cooling tower has effects on air quality, ambient noise, terrestrial ecology, water quality, aquatic ecology and aesthetics. The major effects are discussed in Reference 3 and summarized below.

- A) Ground fog incidence is only one event in 11 months; this is inconsequential in comparison to the natural fog incidence of 2%. No icing incidence will occur.
- B) Drift dispersion from a natural draft tower is very wide spread. It results in salt deposition and saline air concentrations that are relatively small. Those studies also indicate most plants in the Indian Point area will not be injured with the possible exception of hemlock, flowering dogwood and white ash. As reported in these very extensive studies, the potential for biological damage is improbable unless the ambient conditions described in Reference 3 persist.
- C) Blowdown chemistry could be diluted in the discharge canal before discharge to the river. All discharge water chemistry, including intermittent chlorine, will be within EPA mandated guidelines. Extensive bioassay data on the aquatic populations is included in the referenced 1977 studies.
- D) Noise during construction and operation is expected to be lower for natural draft towers and less localized.
- E) Due to its size, a natural draft tower will have a major visual and aesthetic impact on surrounding areas. It will render ineffective Indian Point efforts at maintaining a low profile.
- F) The makeup of a closed cooling system is about 2% of the existing once-through flow. This reduces the entrainment of aquatic life appreciably. Thermal heat to the Hudson River is also reduced proportionally.
- G) Installation of a closed-cycle cooling system necessitates replacement of the fossil power generation due to the amount of the Indian Point 3 derate and the plant downtime during the cut-in of the cooling tower. This will result in added atmospheric discharges of fuel based oxides and particulates.

### 3.3 Mechanical Draft Cooling Towers

#### 3.3.1 Engineering Aspects

The mechanical draft tower design for the Indian Point 3 Station is the same as that determined in Reference 3. Four towers are provided. It is concrete construction. The base diameter is 462 feet and size of each tower is 54' x 486' and it is 33 ft to the fan deck. The tower design point is for a circulating water flow rate of 600,000 gpm, a range of 25°F and an approach of 10°F at a 79°F wet bulb and 50% relative humidity. The review of a current site drawing indicates the site will accommodate mechanical draft towers.

To maintain the concentration of the circulated station water at reasonable levels, approximately 8,000 gpm of blowdown water per unit would be discharged to the Hudson River. At the design point, about 12,000 gpm is lost through evaporation, as the thermally induced air flow comes in contact with the warm circulating water sprayed in the fill. Approximately 12 gpm of water would be lost through drift. The latter is the classification of water droplets which become suspended in the air flowing through the tower and which are deposited on the surrounding land. All categories of water usage are replaced by makeup water drawn from the river.

#### 3.3.2 Cost Estimates

Section 1.4 had indicated why the cost of the cooling towers is only a small fraction of the overall cost of retrofitting. The capital costs of the system using the wet mechanical draft cooling tower are listed in 1993 dollars as follows:

Cooling tower - erection, labor and materials	\$ 18,000,000
Amertap	\$ 8,651,000
Piping	22,761,000
Structural	84,013,000
Electrical	20,180,000

Project Management	5,112,000
Indirects and Professional Services	<u>19,000,000</u>
TOTAL	\$177,700,000

These costs should be consistent with previous costs of Table 5-2, Ref. 2. Cooling tower costs shown are based on 1993 data supplied by cooling tower vendors. All other inherent construction and installation costs of 1977 studies were escalated as stated in Sub-section 2.2.2.

All other inherent construction and installation costs of the 1977 studies were reviewed and found to be appropriate. They were escalated and updated from the 1977 studies to reflect the 1993 material and labor costs at 5.5%.

Section 1.4 also discusses the kinds of new added operating costs and the related loss in station efficiency when retrofitting cooling towers. The capability losses of this alternative for Indian Point 3 follow. They are based on Table 3-2 and Table 5-6 of Ref. 3.

#### Plant Capability Losses

Loss of turbine capability - MW	24
Cooling System Auxiliaries - MW	21
Total Derating - MW	45
Max. Peak Temperature Derate - MW	73

### 3.3.3 Environmental Aspects

A mechanical draft cooling tower has effects on air quality, ambient noise, terrestrial ecology, water quality, aquatic ecology and aesthetics. The major effects are discussed in Reference 2 and summarized below.

- A) Ground fog (plume) incidence are much more frequent than in natural draft cooling towers. Icing incidence will occur.
- B) Drift dispersion from a mechanical draft tower is very localized. It results in salt deposition and saline air concentrations that are higher than mechanical draft. Those studies also indicate most plants in the Indian Point area will not be injured with the possible exception of hemlock, flowering dogwood and white ash. As reported in these very extensive studies, the potential for biological damage is improbable unless the ambient conditions described in Reference 2 persist.
- C) Blowdown chemistry could be diluted in the discharge canal before discharge to the river. All discharge water chemistry, including intermittent chlorine, will be within EPA mandated guidelines. Extensive bioassay data on the aquatic populations is included in the referenced 1977 studies.
- D) Noise during construction and operation is expected to be lower for mechanical draft towers and less localized.
- E) The makeup of a closed cooling system is about 2% of the existing once-through flow. This reduces the entrainment of aquatic life appreciably as Reference 2 shows. Thermal heat to the Hudson River is also reduced proportionally.
- F) Installation of a closed-cycle cooling system necessitates replacement of the fossil power generation due to the amount of the Indian Point 3 derate and the plant downtime during the cut-in of the cooling tower. This will result in added atmospheric discharges of fuel based oxides and particulates.

It should also be noted that the mechanical draft towers have a lower profile, similar to that of other plant buildings, but are much more extensive than the natural draft designs. The visual impact of the mechanical draft tower should be evaluated with their frequent lengthy plumes when comparing them with the effects of the natural draft design.



## **4.0 BOWLINE ALTERNATIVE CLOSED-CYCLE COOLING SYSTEMS**

### **4.1 Summary Descriptive of Existing Plant**

Bowline Point Station consists of two 600-MW oil-fired and natural gas-fired units, located in the town of Haverstraw, NY, on the west bank of the Hudson River at river mile 38. The present cooling system is a once-through design. The brackish circulating water is pumped from the Hudson River, passed through the condenser where it is heated, and released again to the river.

The design circulating water flow rate is 375,620 gpm per unit. However, the flow may occasionally be restricted as low as 257,000 gpm per unit under the terms of the Consent Order of 13 March 1992 (reference 6)

As stated in Section 1, above, a number of alternative cooling systems have been considered as replacements for the once-through system. Three offer mature designs and are evaluated carefully:

- Wet natural-draft cooling tower
- Wet mechanical-draft cooling tower
- Dry cooling tower

A 1977 study (reference 4) evaluated both types of wet cooling towers. The optimum natural-draft case considered circulating water flow rates, condenser performance, capital and operating costs. The optimum mechanical-draft case considered the same costs. Both cases included the effect of changed circulating water flow rate on costs of pumps, piping, and water-treatment systems.

The present study obtained new costs of the cooling tower for these optimum cases. The natural-draft design is quite similar to the 1977 study. However, the present study uses a wet mechanical-draft tower rather than the previous wet-dry design, as wet-dry tower

technology since 1977 has not matured into wide spread use, as indicated in Section 1. All wet cooling tower discharge a small stream called blowdown, requiring an SPDES permit. They are evaluated in this Section. The dry cooling tower discharges no water, requiring no SPDES permit, but presents special engineering challenges. Therefore, it is separately addressed in Section 6.

## 4.2 Natural Draft Cooling Tower

### 4.2.1 Engineering Aspects

Each of the two units at Bowline Station would have one natural-draft tower. The tower would have a reinforced concrete shell of hyperbolic cylindrical shape. The base diameter would be 331 feet and the overall height 454 feet. A review of the current site drawing indicates that the 1977 tower location (Figure 6) is still appropriate. The structure would require about 2.0 acres of land area and there is adequate ground space. The tower would have a circulating water flow rate of 295,130 gpm to disperse a heat load of  $2.795 \times 10^9$  BTU/hr, the same as in the 1977 study.

Construction of the cooling tower would require two construction seasons. Therefore, the tower could not be commissioned until at least two years after the decision to install it. The overall construction of the retrofit project is however estimated to require about 3 years.

Approximate  
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What?  
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1 yr to construct plant  
Do test, etc.  
3 yr Total to I/S  
(Both units)

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i be lost through evaporation, as the  
re circulating water sprayed on the fill.  
ating water flow) would be lost through  
air flowing through the tower and are  
s (8,000 gpm) are replaced by makeup



## 4.2.2 Cost Estimates

Cooling tower costs are based on data supplied  
All other costs are based on the 1977 study  
equipment and labor costs.

← 5.5% escalation

Section 1 had indicated why the cost of the cc  
overall retrofit costs. The direct, estimated ca  
units using the wet natural-draft cooling tower  
escalation rate of 55% is used to project the e:

Cooling Tower	\$ 44,580,000
On-Line/Cleaning	8,480,000
Piping	15,072,000
Structural	14,602,000
Electrical	6,550,000
Indirects and Professional Services	<u>25,890,000</u>
TOTALS	\$115,200,000

The annual capability loss of the tower is determined in the same manner as in the 1977 study. The duty and circulating water flow would be the same, so the condenser rise and TTD would be unchanged. The effects on the unit heat rate and maximum capacity are estimated from Reference 4.

The circulating water elevation head of the cooling tower in the present study is 34.2 feet, rather than 37.2 feet in the 1977 study (reference 4). As elevation head is only a portion of the circulating water total pump head, the power required by the circulating water pumps drops by a negligible amount. There are no fans. The effect on heat rate and unit capacity is the same as in the 1977 study.

In addition, the replacement capacity required while the new cooling system is cut-in would be the same as in the 1977 study. The plant capability losses are as follows:

Plant Capability Losses (per unit)

Loss of turbine capability - MW	2.5
Cooling System Auxiliaries - MW	1.7
Total Derating - MW	4.2
Max. Peak Temperature Derate - MW	22.1

The present average heat rate of 9500 BTU/net Kwh (VWO, 5% OP) would be 9566 BTU/net Kwh after a natural draft cooling tower installation.

#### 4.2.3 Environmental Aspects

The environmental impact of the natural draft cooling tower is described in detail in the 1977 cost-benefit report (Reference 1). Some aspects are described briefly here.

Natural draft cooling towers at Bowline would have effects on air quality, ambient noise, terrestrial ecology, water quality, aquatic ecology, and aesthetics. Air quality impacts result from the evaporation and drift. A potential concern associated with these emissions is fogging and icing, which are already common in the Hudson River valley. Noise effects would result from the movement of water droplets and air within the tower. Another concern is the potential of drift to deposit small amounts of salt on downwind areas; this is expected to have negligible effect on terrestrial ecology. The blowdown water discharge would be of quality high enough and temperature low enough to have negligible effect on aquatic ecology. The reduction of flow as a result of retrofitting a tower is significant at about 97% of the existing once-through flow.

The visual impact of the vapor plume and concrete structure of the cooling tower is a major effect.

## 4.3 Mechanical-Draft Cooling Tower

### 4.3.1 Engineering Aspects

A mechanical-draft cooling tower may be built of either wood or reinforced concrete, and may be in counter-flow or cross-flow configuration. A wood-built, counter-flow configuration was assumed for this study because of its compactness, economy, efficiency, low pumping cost, and demonstrated reliability over a 30-year plant life.

Each of the two units at Bowline Station would have one mechanical-draft tower. The tower would consist of approximately 18 rectangular cells arranged side-by-side in two horizontal structures. A review of the current site drawing indicates the 1977 tower location plan is still appropriate. Each cell would have a width of 54 feet, a length of 60 feet and a height of 33 feet, including the fan stack height. The structure would be constructed of wood supports. The structures would require about 1.4 acres of land area per unit. This tower is larger than the one evaluated in 1977, as the present study uses a design approach of 10°F rather than 15°F. There is adequate ground space, although the orientation to the prevailing wind may be unfavorable (reference 10, figure 2-1). The circulating water flow rate would be approximately 321,960 gpm and the pumping head would be approximately 26.5 feet. The fan power consumption would be approximately 200 hp per cell.

Construction of the cooling tower itself would require two construction seasons. Therefore, the tower could not be commissioned until at least two years after the decision to install it. The overall construction of the retrofit project however is estimated to require about 3 years.

Approximately 2,889 gpm of water, the blowdown, would be discharged to the Hudson River. Approximately 4,719 gpm of water would be lost through evaporation, as the thermally induced air flow comes in contact with the circulating water sprayed on the fill. Approximately 7 gpm of water (.002% of the circulating water flow) would be lost through drift, as water droplets become suspended in the air flowing through the tower and are

deposited on the surrounding land. All three losses (8,000 gpm) are replaced by makeup water drawn from the river. However, the drift would be deposited over a smaller area than for the natural-draft cooling tower.

#### **4.3.2 Cost Estimates**

Cooling tower costs are based on data supplied in January 1993 by cooling tower vendors. All other costs are based on the 1977 study (Reference 4), escalated to reflect 1993 equipment and labor costs.

The capital costs of the system using the wet mechanical-draft cooling tower are tabulated below.

The annual capability loss of the tower is determined in the same manner as in the 1977 study. The duty and circulating water flow would be the same, so the condenser rise and TTD would be unchanged. The effects of the lower water temperature on the unit heat rate and maximum capacity are estimated from reference 4.

The circulating water elevation head of the cooling tower in the present study is 26.5 feet, rather than 37.2 feet in the 1977 study (reference 4). As elevation head is only a portion of the circulating water total pump head, the power required by the circulating water pumps drops by about 10%. The tower in the present study has 18 fans of 200 HP each, instead of 20 fans of 200 HP each. Though likely within the accuracy of the estimate this also reduces fan power by 10%.

In addition, the replacement capacity required while the new cooling system is cut-in would be the same as in the 1977 study.

Section 1 had indicated why the cost of the cooling tower is only a small portion of the overall retrofit costs. The direct estimated capital cost of both Bowline units using the wet

natural draft cooling tower are listed in 1993 dollars as follows. Note that an escalation rate of 5.5% is used to project the earlier costs to 1993.

Cooling Tower	\$15,430,000
On-Line/Cleaning	9,846,000
Piping	16,486,000
Structural	13,426,000
Electrical	10,100,000
Indirects and Professional Services	<u>18,936,000</u>
TOTALS	\$84,200,000

The plant capability losses are as follows:

Plant Capability Losses (each unit)

Loss of turbine capability - MW	0.4
Cooling System Auxiliaries - MW	4.8
Total Derating - MW	5.2
Max. Peak Temperature Derate - MW	13.0

The present average heat rate of 9500 BTU/net Kwh (VWO, 5% OP) would be 9583 BTU/net Kwh after a mechanical draft cooling tower installation.

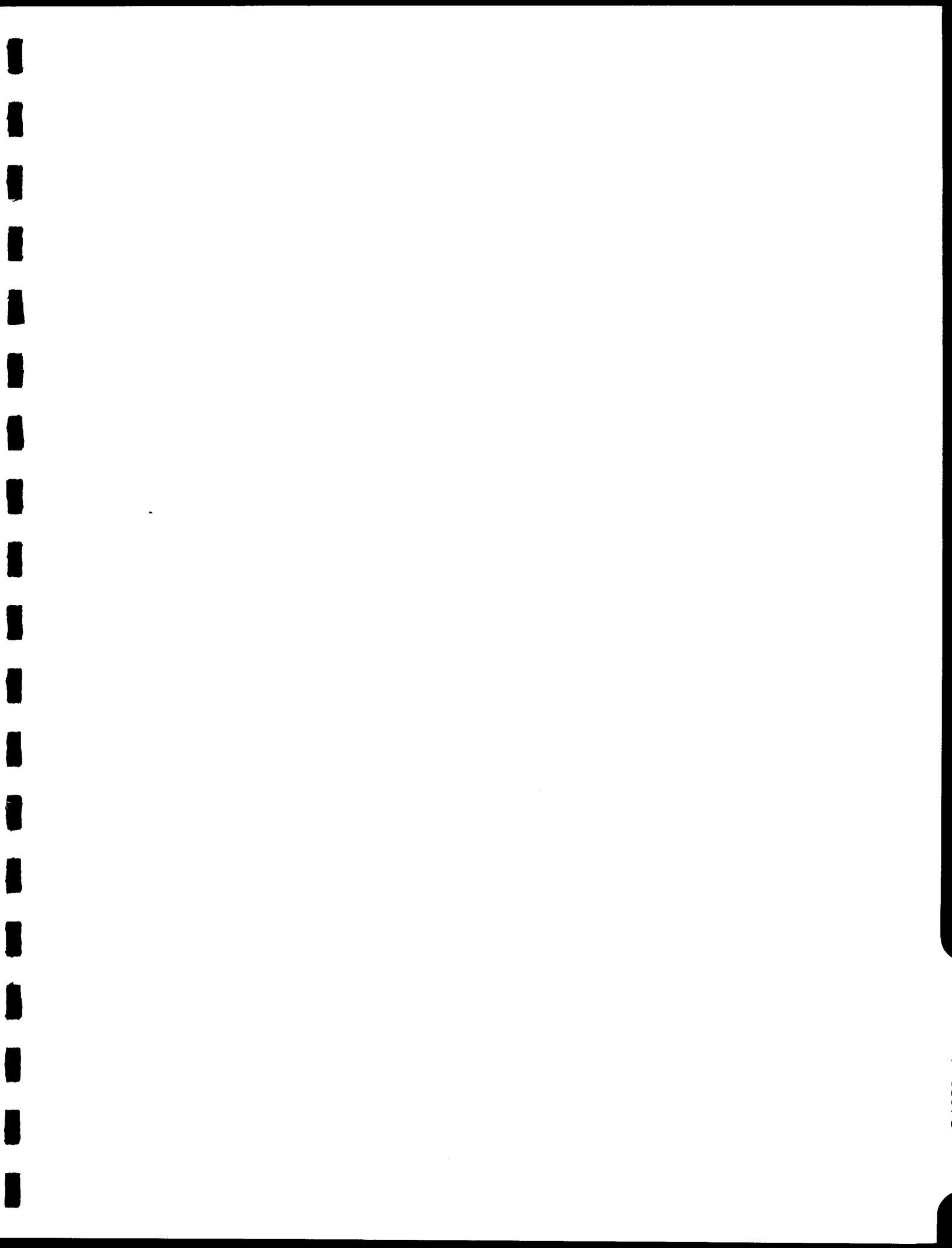
#### 4.3.3 Environmental Aspects

The 1977 cost-benefit study does not discuss environmental impact of mechanical-draft towers (Reference 1). However, a mechanical-draft cooling tower would have effects on ambient noise, aesthetics, terrestrial ecology and aquatic ecology.

The most severe effect is from tower noise. Measures can be taken to attenuate the fan and inherent water noise. However, it should be noted that the measures are very costly, increase fan horsepower and are not completely effective.

The effects on terrestrial ecology and aquatic ecology are similar to those for a wet natural-draft cooling tower.

The mechanical-draft tower is a low, rectangular structure, reducing the area of potential visibility as a passive structure. However, in operation the mechanical draft tower will emit a significant visual plume for most of the year. It should be compared on this latter visual basis to a natural draft tower and not simply as a passive structure.



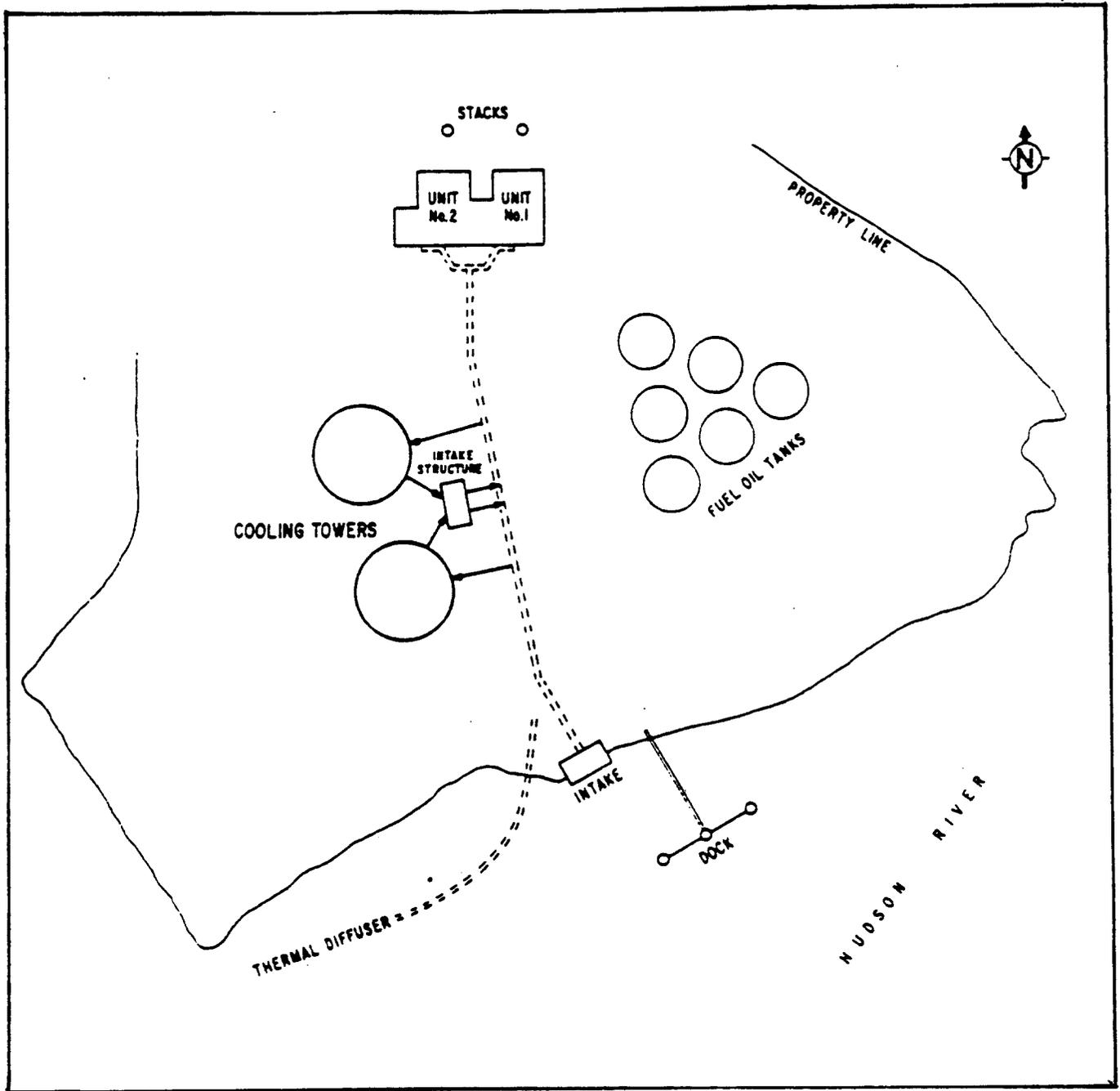
## 5.0 ROSETON ALTERNATIVE CLOSED CYCLE COOLING SYSTEMS

### 5.1 Summary Description of Existing Plant

The Roseton Generating Station, jointly owned by Central Hudson Gas and Electric Corporation, Consolidated Edison Company of New York, Inc. and Niagara Mohawk Power Corporation is located on the west shore of the Hudson River, approximately 66 miles upriver of the Battery, near the town of Newburgh, Orange County, New York. The Roseton facility consists of two 600 MWe (net) steam electric units capable of burning both residual No. 6 oil and natural gas.

The Roseton Plant was constructed in the period 1969-1974. Unit 2 began commercial operation on September 14, 1974 and Unit 1 began commercial operation on December 31, 1974. The main plant building and stacks are located approximately 1,700 ft from the shoreline and, except for an oil tank area located within an earthen embankment and miscellaneous service buildings and structures, the ground is level to the river edge. The station is visible from the river itself, from its east shore, and to anyone crossing the river on the Newburgh-Beacon Bridge, which is situated south of the plant.

The existing condenser cooling system of the Roseton Station is an open-cycle system. An intake structure is located directly on the river edge. Four pumps located in the intake structure withdraw water (about 641,000 gpm at maximum pumping) from a depth of approximately 26 feet. Water discharged from the four pumps flows through a single buried, 12-foot diameter, pipe to the steam condensers in each unit. The heated water from both units is returned to the Hudson River through another buried, 12-foot diameter pipe. Heated effluent is discharged into the Hudson River through a submerged, multi-port diffuser located approximately 500-feet downstream from the intake. The diffuser is oriented parallel to the longitudinal river axis, with 14 ports discharging essentially perpendicular to the river flow. Locations of the main plant building, and the intake and discharge structures are shown in Figure 7.



**Figure 7** GENERAL ARRANGEMENT FOR THE TWO NATURAL DRAFT COOLING TOWER CONFIGURATION AT ROSETON

Under the current Consent Order (Reference 6), the the volume of river water drawn into the Roseton pl efficient operation. These volumes are approximated below. Because the number of pumps which mus dependent upon the number of units in operation and rates for precise periods in the future cannot be spec from those set forth in the chart because of the nee other permit requirements.

Comments:  
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<u>Approximate Period</u>	<u>Expected Maximum River Temperature During Period (F)</u>	<u>Number of Pumps Operating</u>	<u>Approximate Flow, gpm</u>
Jan. 1 - May 14	60	2	418,000
May 15 - June 14	71	3	561,000
June 15 - Sept. 24	82	4	641,000
Sept. 25 - Oct. 16	72	3	561,000
Oct. 17 - Dec. 31	64	2	418,000

## 5.2 Natural-Draft Cooling Tower

### 5.2.1 Engineering Aspects

The natural draft tower design for the Roseton Station is the same as that determined in Reference 5. A recent site drawing was reviewed to ensure the design was still properly located. One tower is provided for each unit. Each has a reinforced concrete shell of hyperbolic cylindrical shape. the base diameter of each would be 310 feet and the overall height 390 feet. The tower would have a circulating water flow rate of 254,400 gpm, a range of 19.1°F and an approach of 18°F with an ambient wet bulb temperature of 75°F.

At the design point, about 4,600 gpm of water per unit would be lost through evaporation, as the thermally induced air flow comes in contact with the warm circulating water sprayed

on the fill. To maintain the concentrations of the circulated water at reasonable levels, approximately 3,100 gpm of blowdown water per unit would be discharged to the Hudson River. Approximately 5 gpm of water would be lost through drift. Drift is the term used for water droplets which become suspended in the air flowing through the tower and are deposited on the surrounding land. All three categories of water usage are replaced by makeup water drawn from the river. As reported in Reference 7, annual water consumption by the two unit station would be about 3.5 billion gallons per year.

### 5.2.2 Cost Estimates

Section 1.4 had indicated with overall retrofit costs. This using the wet natural-dry

*Missing construction period data base pg 31)*  
*And cut in outage data. (# of days units need to be off line to connect towers.)*

ly a small portion of the n for both Roseton units as follows.

Cooling tower	\$35,900,000
Allowance-On-Line	\$ 9,800,000
Piping	\$ 5,800,000
Structural	\$18,500,000
Electrical	\$ 1,300,000
Indirect and Professio.	<u>\$20,700,000</u>
Totals	\$92,000,000

*0-40*

Cooling tower costs are based on 1993 data supplied by cooling tower vendors. All other inherent construction and installation costs of the 1977 studies were reviewed and found to be appropriate. They are escalated and updated from the 1977 studies to reflect 1993 material and labor cost. They are based on an average escalation rate of 5.5%. Piping, and pump replacement costs are also based on the 1977 study but updated to 1993.

Section 1.4 also discusses the kinds of new added operating costs and the related loss in station efficiency which occurs when retrofitting.

The plant capability losses of this alternative are as follows:

Plant Capability Losses (each unit)

Loss of turbine capability - MW	4.6
Cooling System Auxiliaries - MW	2.1
Total Derating - MW	6.7
Max. Peak Temperature Derate - MW	22.7

The present average heat rate of 9393 BTU/net Kwh (VWO, 5% OP) would be 9503 BTU/net Kwh after a natural draft cooling tower installation.

**5.2.3 Environmental Aspects**

A natural draft cooling tower has effects on ambient noise, terrestrial ecology, aquatic ecology, and aesthetics. Reference 7 examined the environmental effects of a natural draft cooling tower with respect to fogging, icing, saline drift deposits and noise characteristics. The results from those studies are still relevant today and are summarized below.

The cooling towers would induce 85 hours of fog and 45 hours of icing (concurrent with fogging hours) in a representative year. Principal effects would be:

- (a) Restricted visibility on the West Shore and East Shore Railroads, on the Hudson River (navigation), in the hamlets of New Hamburg and Chelsea on the east shore of the River and in the residential areas north and south of Roseton on the west shore of the River.
- (b) Icing on the West Shore and East Shore Railroads, in the hamlets of New Hamburg and Chelsea and in the residential areas north and south of Roseton on the west shore of the River.

<u>Maximum</u>	<u>Maximum Deposition Rate Kg/Km<sup>2</sup>/Mo.</u>
June	19
July	50
August	70
September	26
October	70

The principal increase in community noise was predicted to occur at those residences located within the industrially zoned area south-southwest of the Roseton Plant property.

The number of stems of tree species which will be affected by salt aerosol drift over the life span of the proposed cooling towers at Roseton was estimated as follows:

Species	Low Estimate	Best Estimate	High Estimate
Flowering Dogwood	0	0	12,137(+1,228)
Canadian Hemlock	0	0	7
White Ash	0	0	7,442 (+1,371)
Cherry	0	0	2,370 (+536)

It is concluded that the reaction of the public to the scenic intrusion of a natural draft cooling tower will be highly variable ranging from approval by a few to strong objection by many who regard the scenic and historic Hudson Valley as a major regional or even national treasure. It is expected that the strong objections by many in the 1970's is still the prevailing thought in the 1990's.

## 5.3 Mechanical-Draft Cooling Tower

### 5.3.1 Engineering Aspects

A mechanical-draft cooling tower may be built of e may be in counter-flow or cross-flow configuration. A was assumed for this study because of its compact cost, and demonstrated reliability over a 30-year pl

Note :

30-year  
plant life

The tower for each unit would consist of approxi

by-side in two horizontal structures. Each structure would be approximately 400 feet long and 55 feet wide. The circulating water flow rate would be approximately 254,400 gpm, a range of 19.1°F and an approach of 10°F with an ambient wet bulb temperature of 75°F. The pumping head would be approximately 30 feet. The fan power consumption would be approximately 200 hp per cell.

The range and approach selected in this study for a wood rectangular tower are lower than those selected in Reference 5 for a concrete circular tower. This is due in part to improvements in technology since 1977 and in part to the lower cost of the wood construction. A recent site drawing was reviewed to ensure the tower locations of reference 5 are still applicable.

Evaporation, drift, blowdown, and makeup quantities would be about the same as the wet natural-draft cooling tower. However, the drift would be deposited over a smaller area.

### 5.3.2 Cost Estimates

The direct estimated capital costs of the system for both Roseton units using the wet mechanical-draft cooling tower are as follows: (1993 dollars)

Cooling tower		\$ 9,500,000
Allowance-On		\$ 9,800,000
Piping	<i>See correct on pg 40</i>	\$ 7,700,000
Structural		\$21,100,000
Electrical		\$ 7,100,000
Indirect and		<u>\$16,100,000</u>
Totals		\$71,200,000

Cooling tower cost: ing tower vendors. All other inherent constructi ~~ions~~ <sup>to 44</sup> s were reviewed and found to be appropriate. They are escalated and updated from the 1977 studies to reflect 1993 material and labor cost. They are based on an average escalation rate of 5.5%. Piping, and pump replacement costs are also based on the 1977 study but updated to 1993.

Section 1.4 also discusses the kinds of new added operating costs and the related loss in station efficiency which occurs when retrofitting.

The plant capability losses of this alternative are as follows:

Plant Capability Losses (each unit)

Loss of turbine capability - MW	1.8
Cooling System Auxiliaries - MW	3.4
Total Derating - MW	5.2
Max. Peak Temperature Derate - MW	13.4

The present average heat rate of 9393 BTU/net Kwh (VWO, 5% OP) would be 9479 BTU/net Kwh after a mechanical draft cooling tower installation.

### **5.3.3 Environmental Aspects.**

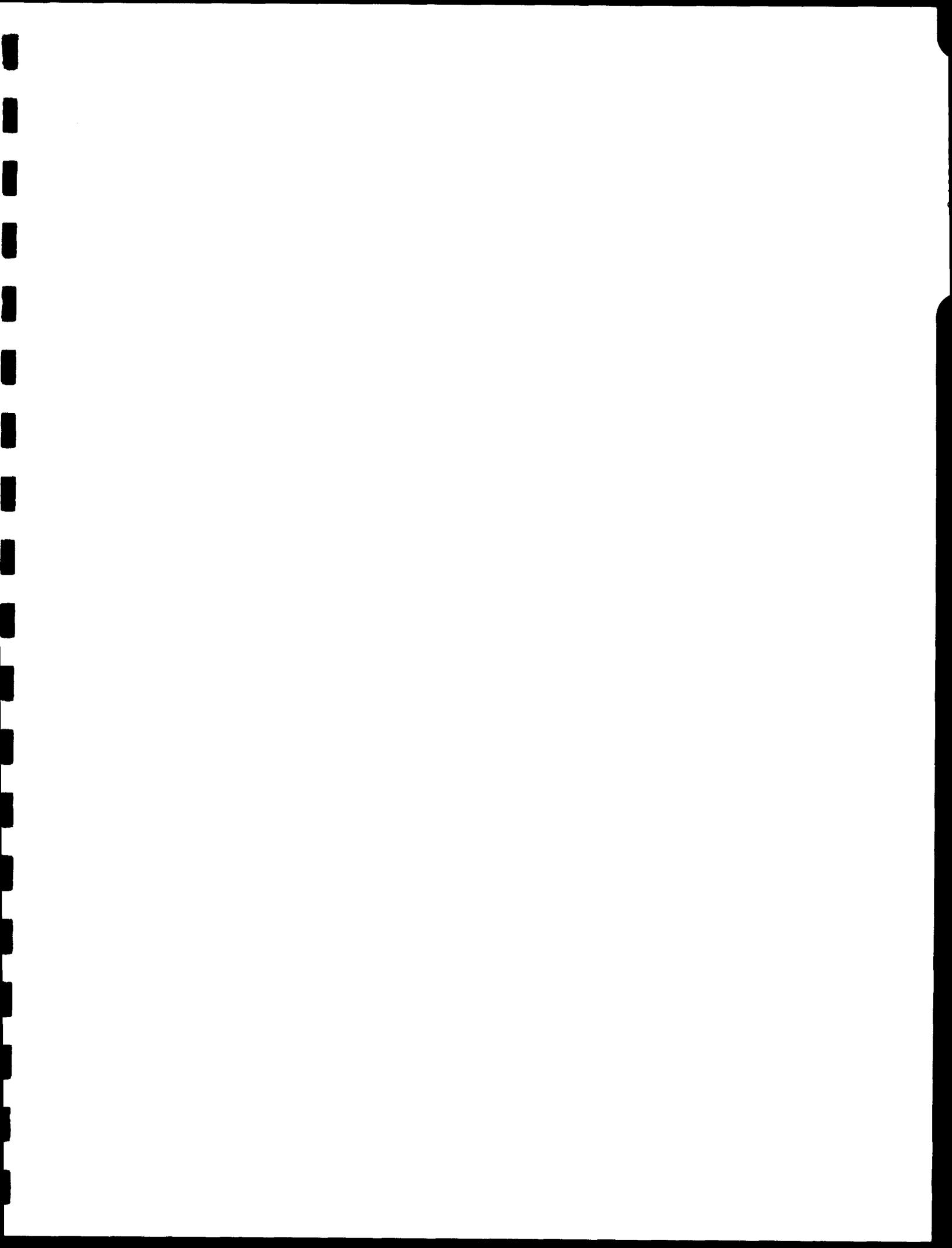
A mechanical-draft cooling tower would have effects on ambient noise, aesthetics, terrestrial ecology and aquatic ecology.

The most severe effect is from fan noise, produced by motors, gearboxes, and fan blades. Fan motor noise could be reduced by fitting noise attenuators around the motors. The level of noise at any given location depends on the distance from the noise source. Reference 5 excluded rectangular mechanical draft towers on the basis that the impact of noise would be unacceptable. Fan gearbox noise could be reduced by using low-noise gearboxes and optimizing the fan blade. Fan blade noise, particularly pure tone noise caused by blade-strut interactions, could be reduced by specifying an odd number of struts in relation to the fan blades.

Fogging, icing and drift deposits due to the plume from a mechanical draft tower would affect a smaller area than that of a natural draft tower, but the magnitude of those effects would be greater.

The effects on terrestrial ecology and aquatic ecology are similar to those for a wet natural-draft cooling tower.

It should also be noted that the mechanical draft towers have a lower profile, similar to that of other plant buildings, but are much more extensive than the natural draft designs. The visual impact of the mechanical draft tower should be evaluated with their frequent lengthy plumes when comparing them with the effects of the natural draft design.



## **6.0 ALL PLANTS: ALTERNATIVE CLOSED-CYCLE SYSTEMS THAT DO NOT REQUIRE AN SPDES PERMIT**

### **6.1 Description Summaries**

Report Sections 2 through 5 present summary descriptions of the existing plant cooling systems at the Indian Point 2 and 3 and the Bowline and Roseton Stations and the evaluation of alternative wet closed cycle cooling systems for each of these plants. The alternative wet closed cycle cooling systems included the wet mechanical-draft cooling tower and the wet natural-draft cooling tower. The wet closed-cycle cooling schemes which were determined to be most suitable for these plants, if an alternative to the present once-through cooling system is required.

The selection of a particular wet tower system design depend on the associated economics, available site area, and environmental considerations such as drift dispersion ability, height limits, plus other considerations discussed in the report. Since these are retrofit applications, environmental, engineering, and economic factors impose more restrictions than when evaluating wet cycle tower systems at a new site. These selections are not unusual because wet towers are generally utilized if sufficient water is available to accommodate the evaporation and blowdown requirements. Also available are "wet/dry cooling towers," which combine the features of the wet and dry cooling towers. However, the wet/dry technology was eliminated from consideration for the Hudson River plants for the reasons cited in Section 1.2.

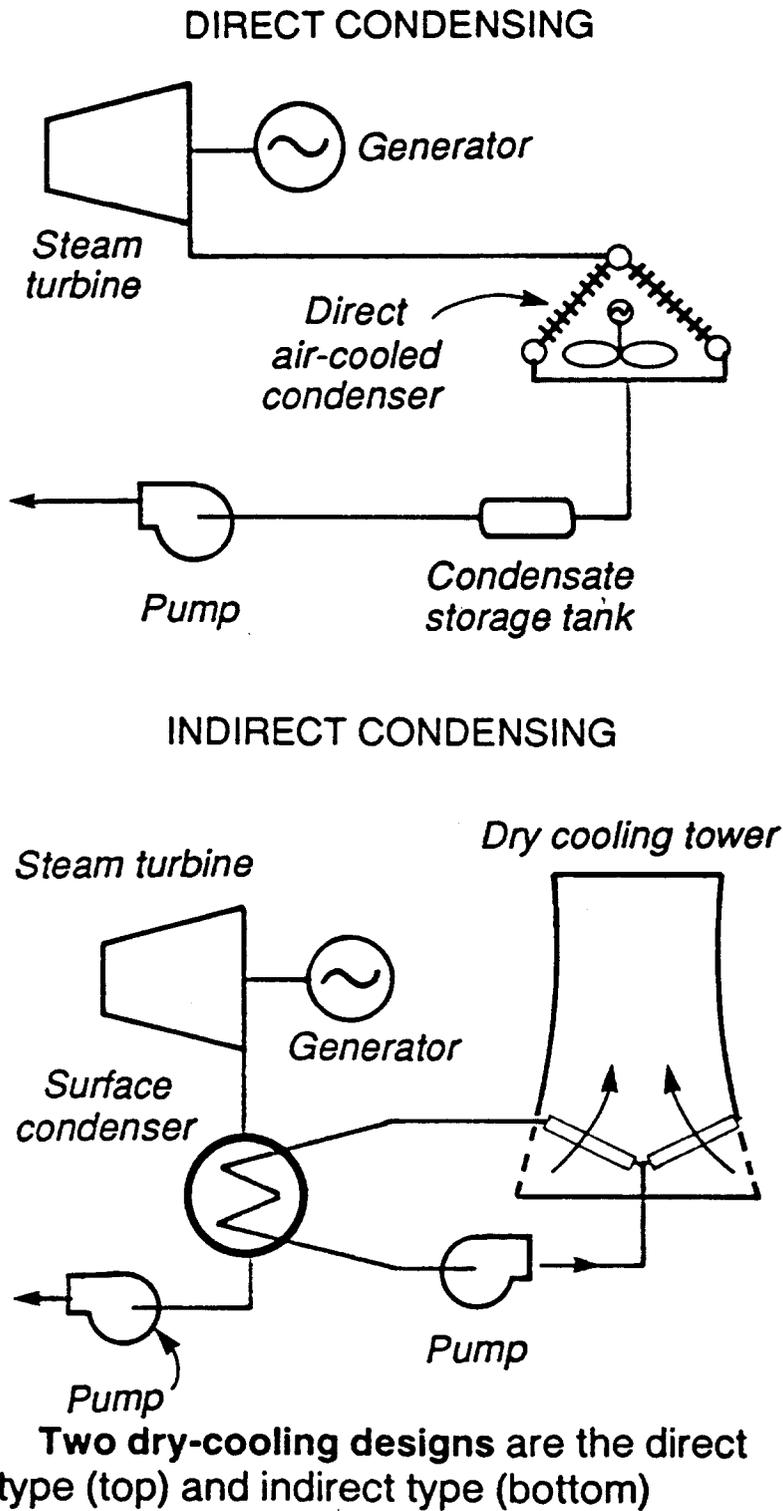
All of the above alternative cooling configurations, however, convey the heat load to the atmosphere mainly by the process of evaporation. Based upon current SPDES regulations, it appears that all cooling devices other than air cooling requires an SPDES permit. Therefore, an additional cooling device which will be evaluated with respect to these sites is the "dry" cooling tower. The dry cooling tower involves only sensible heat exchange and

consists of fixed tubes that totally enclose the medium to be cooled, making mass transfer through evaporation impossible.

Compared to a wet tower, the dry cooling tower requires considerably more land area and cools at a considerably higher capital and operating cost. Operating costs are higher because the plant operates less efficiently but the dry tower can provide a technically viable and environmentally attractive option in certain instances where water is not available provided operating or economic restrictions are not so extreme as to preclude its use.

## 6.2 Commercially Available Dry Cooling Towers

Dry cooled condensers may utilize either direct or indirect condensing. (Ref. 8) In the direct type, turbine exhaust steam is condensed within tubes which contain external fins. The huge quantities of tubes which are required for large scale power plants, such as the plants being evaluated at these Hudson River sites, are required to be grouped together in sections which are then generally configured as A-frames for efficient utilization of space. (Please see Fig. 8). Mechanical fans are installed in the horizontal deck below the A-frames and discharge air upwards past the surfaces of the tubes. A large open space is required to be provided below the fan deck to admit the enormous air volumes required for cooling. In the indirect system, turbine exhaust steam is directed to the main steam surface condenser. The condenser cooling water is circulated through the dry cooling tower. Because the indirect system circulates a coolant to the air cooler and then back to the surface condenser, it requires all of the attendant equipment and pumps which are associated with a station circulating water system. The indirect air cooler is nominally larger and more expensive than a direct air cooler for a given duty because of the use of an intermediate fluid. The terminal temperature difference between the turbine exhaust steam and the outlet water temperature of the condenser on the indirect system also creates a higher turbine back pressure. As a result, for a new station design a direct dry cooling system generally presents significant advantages. When considering a retrofit application for either the Bowline, Roseton, or Indian Point Stations, however, it is not a realistic design



**Figure 8 DIRECT AND INDIRECT DRY COOLING SCHEMATICS**

option. The complex geometry and expense associated with removing the existing main steam surface condensers and circulating water systems and replacing them with air-cooled condensing sections and an efficient ducting system to an air-cooled tower system positioned in proximity to the turbine building to minimize duct pressure losses and at the same time utilize available site area for the towers is not practical.

Therefore, although the indirect system requires extra circulating water system type components and provides relatively lower inherent performance potential as compared to the direct system, limitations imposed by the retrofit applications at the Hudson River plants precludes direct dry-cooling from further consideration.

### 6.3 Engineering Aspects

As a result of the required plant capacity requirements and in accordance with the established study objectives, only dry-cooling designs which allow the plants to produce full load will be evaluated. Mitigation alternatives or strategies which involve reductions in load are not considered to be viable alternatives and will not be evaluated. Indirect dry-cooling systems will be evaluated assuming the plants will be required to have the capability to generate close to their existing maximum output.

The engineering investigations will focus on those plant components or systems where the relative cost impact or component design limitations associated with dry cooling are greatest. Based upon the requirement to maintain plant full load capability, the impact of dry cooling on the steam generators, boilers and associated equipment is minimized and will not be addressed here. The infrastructure, civil works, and auxiliary plant components represent approximately 15 to 25 percent of the overall plant costs. The plants turbine/generators and cooling systems represent approximately 25 percent and 15 percent of overall plant costs, respectively. An exact assessment of the impact of retrofitting dry cooling on plant infrastructure is difficult to determine in advance of developing preliminary system designs which are beyond the scope of this report, but representative order of magnitude estimates

of the physical and economic impacts will be made. The engineering and economic evaluations thus will be focused on the turbine and cooling system areas of the plant which, as indicated, represent approximately 40 percent of the overall plant cost and which are also highly deterministic of the net cost associated with annual energy production.

### **6.3.1 Overview of the Mechanism of Indirect Dry-Cooling at the Hudson River Plants (Ref.8)**

For dry heat transfer, surfaces with external fins are typically utilized. Finned tubes are the most expensive component in the dry-cooling system and their proper selection is critical to the long-term success of the specific plant cooling design. Numerous types of finned tubes are commercially available — integral, extruded bimetal, helically wound, galvanized types, and others. Two common tube designs are elliptical steel tubes with steel fins, dip galvanized, and round steel tubes with aluminum fins. This elliptical design provides good service life. This round design provides good heat transfer properties at a given cost. However, the service life may not be as long as the elliptical tubes with steel fins.

For either selection of tube design, the heat of the ambient air passing over the air-cooled condenser tubes is increased exactly by the heat of the condensing steam from the turbine exhaust. For dry finned tubes, the temperature of the air increases. For an indirect system, the heat transfer gradient is provided by the difference in temperature between the cooling air and the coolant for the condensing steam. In most air-cooled condenser designs, usually not more than three to six rows of tubes are crossed in succession by the air which passes over them. The successive increases in air temperature produces a successively higher steam condensing pressure in each row. A large air volume is required to be delivered to cool the finned tube sections. A corresponding area of finned tubing is necessary. In order to develop minimal estimates of the required site plot areas for the retrofit applications at the Hudson River sites, forced mechanical draft air cooled exchangers with the bundles placed in typical A-frame type configuration will be used as the basis for the economic evaluations rather than other developmental designs or natural draft tower concepts.

The terminal temperature difference between the turbine exhaust steam and the outlet water temperature of the condenser on the indirect system is one of the governing parameters in the Hudson River site evaluations since this will create a higher back pressure on the existing low-pressure turbines than was originally designed for. The situation is particularly exacerbated during warm summer weather when these lower Hudson River plants are needed to deliver peak electrical loads to the entire service area.

The engineering and operational impact on the turbine-generators due to the higher back pressures associated with dry cooling is a critical aspect of the dry cooling feasibility determination.

### **6.3.2 Turbine Back-Pressure at the Hudson River Plants Using Indirect Dry-Cooling**

While the thermal performance of wet cooling towers is dependent on the existing ambient wet-bulb temperature, the performance of dry cooling towers is dependent on the local dry-bulb temperature patterns. The dry bulb temperature can equal or be greater than the wet bulb temperature, but it is usually 10 to 30°F higher during heat waves.

The following definitions are provided for reference.

Dry-bulb temperature: Temperature of the atmosphere as measured by a mercury thermometer or other device. The 1 percent dry-bulb temperature is the temperature which exists for 1 percent of the time during the warm weather months based upon historical records for the site vicinity.

Approach: In a dry-tower system, the approach is generally the difference between the initial temperature of the process gas or fluid which requires cooling minus the ambient dry-bulb temperature.

Range: Refers to the temperature range between the initial and final-cooled temperature.

Turbine backpressure corresponding to the Hudson River plants 1 percent dry bulb temperature may be estimated as follows:

<u>Variable</u>	<u>Value</u>	<u>Ref.</u>
1% dry-bulb temp., °F	94.	Ref. 9
Approach to dry-bulb, °F	30.	Engineering estimate
Indirect range, °F	35.	Engineering estimate
Condenser approach, °F	<u>10.</u>	Engineering estimate

Total temperature = 169°F

Therefore, the turbine maximum exhaust pressure corresponding to the 1 percent dry bulb temperature will be the exhaust pressure corresponding to a 169°F saturation temperature, or 12 in. HgA. Similarly, the turbine exhaust pressure corresponding to approximately the Hudson River 5 percent dry bulb temperature is estimated to be in the range of 10 in. HgA.

### **6.3.3 Effect of High Turbine Backpressures at the Indian Point 2 and 3 and the Bowline and Roseton Stations**

Large steam turbine-generators are critical components in the Indian Point 2 and 3 and the Bowline and Roseton Stations. Their continued reliability and operating efficiencies are a key consideration when making decisions with respect to plant modifications which could in any way effect their design service conditions. For these existing stations, any plant modifications which negatively influence the generating capability or the reliability of the turbine-generators will result in substantial lifecycle cost increases for the turbine-generators and, ultimately, for the station. These costs would include the cost of additional electrical

system generating capacity needed to compensate for expected turbine-generator unavailability or reductions in efficiency, as well as the cost of replacement energy which would need to be supplied from other (possibly more expensive) sources when the turbine-generator is unexpectedly out of service. These indirect costs could become substantial over the life of these plants. Small changes in the plants availability, reliability and operating efficiency have a substantial impact on the overall costs associated with producing electricity.

The plants involved are:

<u>PLANT</u>	<u>BOWLINE</u>	<u>ROSETON</u>	<u>INDIAN P2</u>	<u>INDIAN P3</u>
TYPE	Fossil	Fossil	Nuclear	Nuclear
Location-NY	Haverstraw	Newburgh	Peekskill	Peekskill
No. Units	2	2	1	1
Net MW	600	600	941	965

The Bowline and Roseton plant's turbine-generators are 3,600 rpm tandem-compound four-flow reheat machines which contain 30-inch last stage blades. The Indian Point 2 and 3 plant's turbine-generators are 1800 rpm tandem-compound six-flow machines which contain 44-inch last stage blades.

In the case of fossil and nuclear condensing steam turbine-generators of the large sizes represented here, the turbines are carefully matched to the overall cycle and assigned site conditions. The selection of the optimum low pressure steam turbine exhaust annulus area and the cooling system design are governing parameters in the design of the plants. Based upon the original design of these plants being cooling using Hudson River water, their low

pressure turbines have been designed for backpressures in the range of 1 1/2-inches to 3 1/2-inches HgA. As derived in Section 6.3.2, utilizing indirect dry-cooling at these plants would require the capability to operate with condenser backpressures of up to 10-inches to 12-inches HgA.

Therefore, the capability of the existing low-pressure turbines to operate efficiently and reliably at the higher back-pressure limits must be determined. Steam turbine blade failures are the leading causes of large steam turbine unavailability. Based upon industry experience, most reported blade failures occur in low-pressure turbines and most of these failures are found in the last two rows. These are the rows which would be most significantly influenced by operating with higher exhaust end back-pressures. Last stage blading design is very challenging for the following reasons:

- The volume flow through the last stage is greater than for other turbine stages. Therefore, the last stage blades are the longest and have the highest mechanical stresses.
- The last stage produces substantially more energy than any other stage in the turbine - up to 10 percent of the overall unit output. Therefore, maintaining last stage efficiency is critical.

- Long last stage blades require tuning to assure that there are no resonant modes coincident with the low multiples of running speed.
- Last stage blades operate in a wet steam environment.
- Last stage blades operate under variable exhaust volume flow, and thermodynamic conditions with changes in load and back pressure. Some of these conditions can result in flow-excited vibrations, such as stall flutter or buffeting.

Therefore, as a result of the highly complex design aspects of the low pressure turbines last stages of blading, assessments of high back-pressure operating capabilities are best addressed by the turbine suppliers. Several turbine-generator suppliers have been contacted and were requested to address operating these Hudson River plants at the higher back pressures which are associated with dry cooling. They were requested to describe some of the technical problems associated with operation at the higher backpressures, to describe backpressure limits being provided for their newer designs (since the existing designs are approximately 20 years old), and to assess the feasibility to modify the existing designs to suit the new operating limits being proposed.

With respect to the technical concerns associated with high backpressure operation, the respondents indicated several concerns. High exhaust pressure may cause overheating of

the low pressure turbine blading and possible extreme thermal expansion. During low steam flows through the low pressure turbines associated with low loads, the blades do work on the steam. This is called "negative work" and heats the steam. High exhaust temperatures can cause damage to the low pressure turbine blading. Distortion of the exhaust areas can affect bearing loading, gland clearances, and blade path seal clearances. Vibration, seal rubs, and steam leaks can result. These can cause considerable equipment damage and can result in a forced shutdown of the unit. The basic concern associated with off-design, high backpressure operation at higher loads is one of high dynamic buffeting loads that can cause high-cycle fatigue damage and ultimate failure of the blading.

The manufacturers generally indicated that specialized high backpressure (in the range of up to 15-inches HgA) turbines can and have been provided. This generally means a unit which has been specifically designed to suit the given application and which contain shorter (20 to 25-inch vs. the 30 to 44-inch last stage blade lengths which have been used on the Hudson River machines), more rugged last stage designs and associated limits on the flow and loading capability of a given turbine element.

With respect to the backpressure limits for current machine designs of the same general end size as the Hudson River plants, the manufacturers generally recommend alarming in the range of 5.0-inches HgA and tripping in the range of 7½-inches to 8.0-inches HgA backpressure. However, operation at backpressures approaching 8.0-inches HgA, where permitted, generally requires that specific limits with respect to load and time durations at

these higher backpressure limits be observed. Based upon the responses received [to date], even the current nuclear and fossil turbine designs do not provide the operational flexibility for the 10-inch to 12-inch HgA backpressure limits desired for dry-cooling at these Hudson River plants.

With the respect to the capability to modify the existing machines, one manufacturer stated that the option to operate at up to 10-inches HgA would probably mean operation with the last one or two low pressure turbine stages removed, with an associated substantial reduction in unit capacity. Much more detailed engineering studies would be required to determine if this option is feasible. However, the need to maintain close to full load capability at these Hudson River plants does not make this a viable option, assuming that it could be accomplished.

Other manufacturers stated that it might be possible to design new back ends to retrofit into these machines; however, this would represent major modifications and substantial engineering studies to evaluate this further. None of the manufacturers who have responded to date have indicated that they currently have any development programs in-place associated with high backpressure operation. Therefore, at this time, it does not appear that this type of a development program represents a realistic near term option for these Hudson River plants. However, not all manufacturers have responded to date on this issue.

#### 6.4 Cost Estimates

Section 1.4 discusses the cost of the cooling towers as compared to the overall costs associated with retrofitting a closed cycle cooling system. The capital costs of the cooling tower fabrication and materials for a system using an indirect dry mechanical draft tower listed in 1993 dollars, are as follows:

<u>PLANT</u>	<u>Cost-Cooling Tower Fabrication and Materials</u>
Bowline (each unit)	\$19,000,000
Roseton (each unit)	16,000,000
Indian Point 2	51,000,000
Indian Point 3	51,000,000

In addition to those high cooling tower material costs for the indirect dry system, substantial costs would also be incurred associated with retrofitting the dry towers. These costs would be associated with the significant land usage requirements and the costs associated with the balance of plant equipment. Based upon the development nature of the modification required for the turbine-generators, plus significant other balance of plant equipment costs, the costs associated with the dry-cooling retrofit are considered to be prohibitive.

## **6.5 Environmental Aspects**

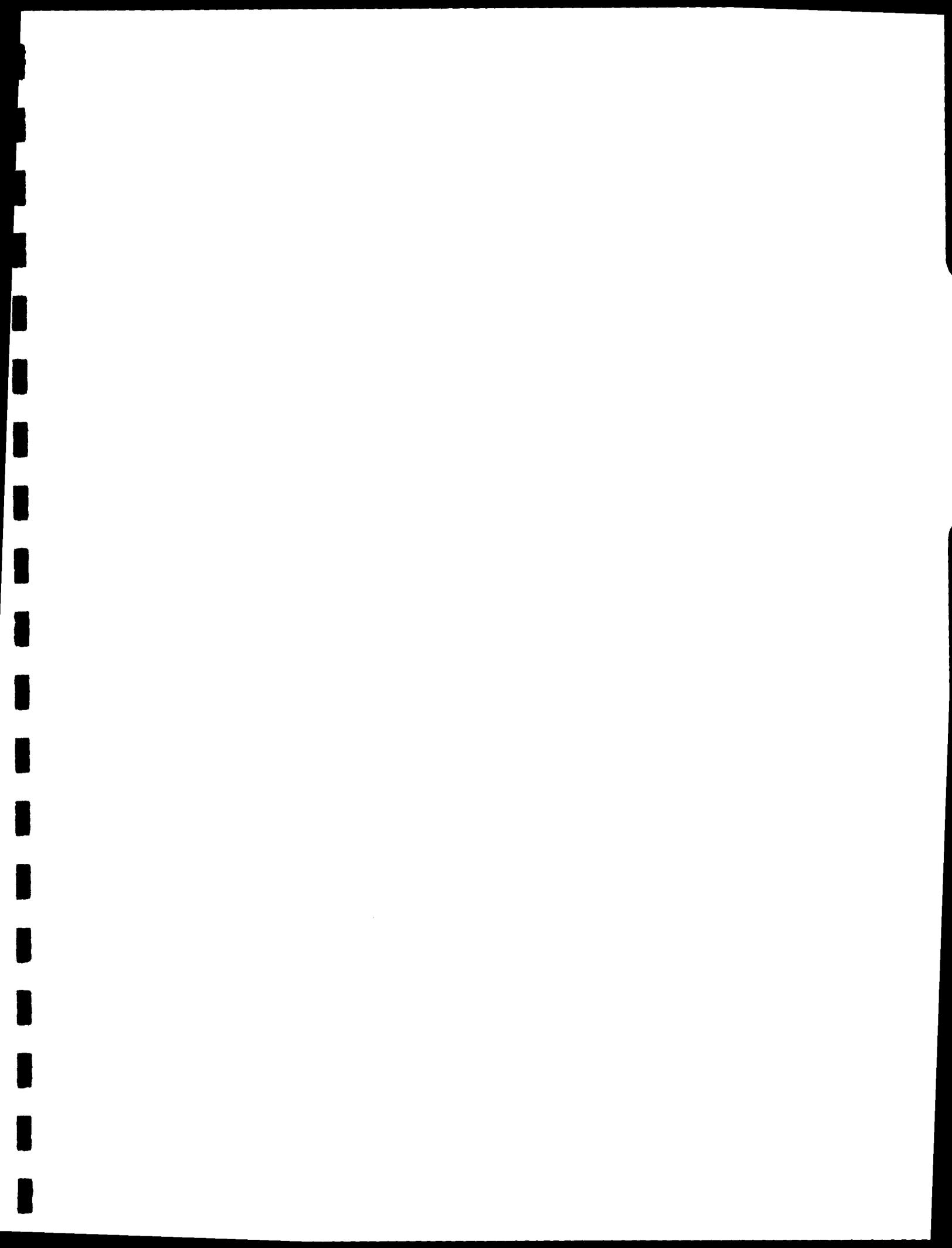
An indirect dry mechanical-shaft cooling tower would have effects on ambient noise, terrestrial ecology, and aesthetics. The most severe effect would likely be associated with fan noise produced by motors, gearboxes, and fan blades. The major advantage associated with the dry tower is the elimination of the need for an SPDES permit due to elimination of water usage for cooling.

## **6.6 Summary and Conclusions**

The use of dry-cooling systems in large scale (greater than 100 MWe) power plants is a relatively new development which has only recently begun to experience growth worldwide. In general, plants which have utilized dry cooling have been located at sites where wet cooling systems could not be used economically due to a shortage of water. New, large dry-cooled plants have been successfully built following highly detailed design optimizations which determine the optimum location of the cooling cells with respect to the balance of plant, and which carefully match the turbine design and operating characteristics to the dry-cooling system design from the outset of the plant design. This involves matching the main power cycle design characteristics with annual ambient air temperature and required operating loads during periods of peak temperature conditions, considering auxiliary loads and fan power requirements associated with dry cooling.

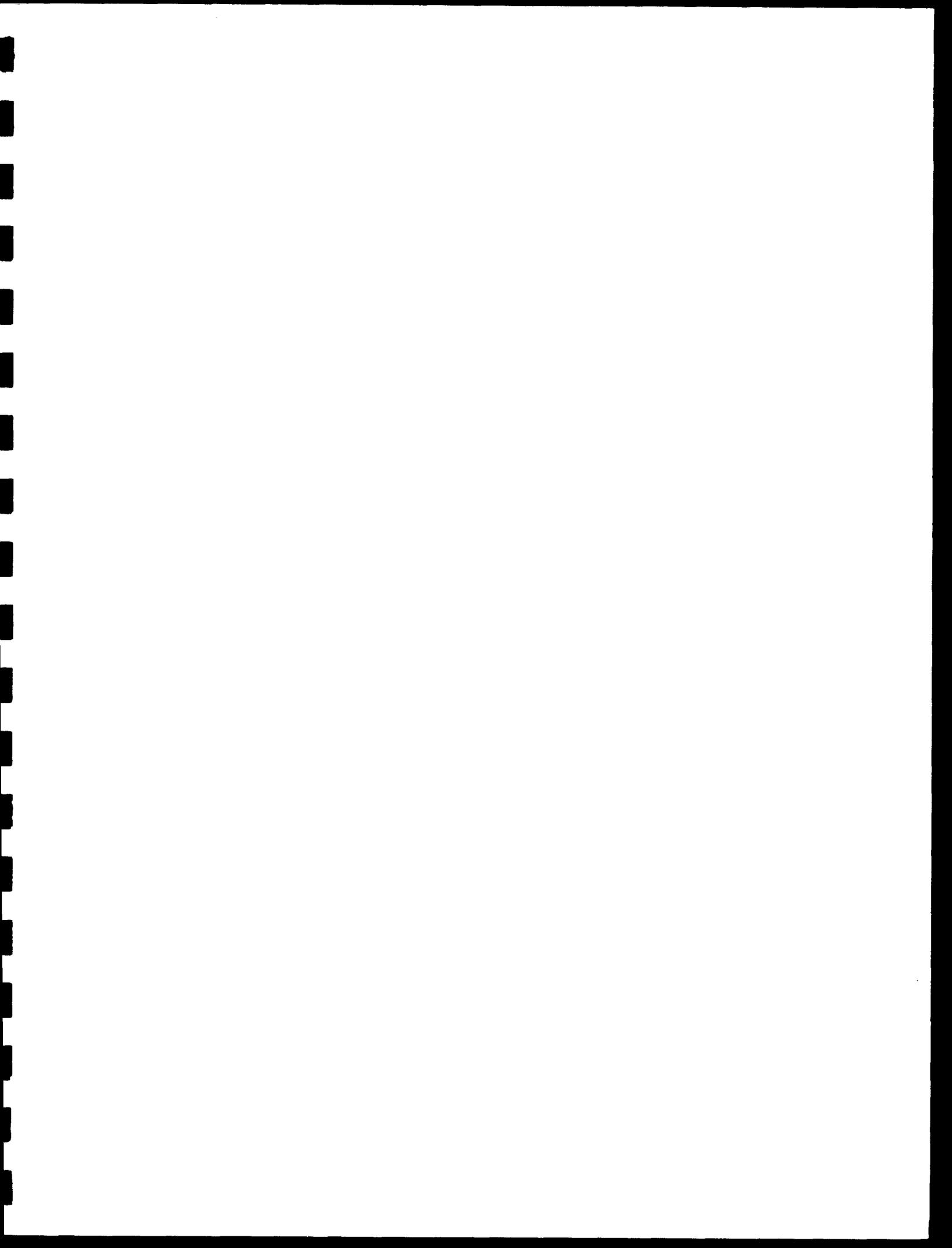
At a new site, given the flexibility to optimize these design variables, large-scale dry-cooling has been shown to be a viable option where alternative cooling system designs could not be utilized. However, for retrofit applications such as at these Hudson River plants, the design is necessarily compromised. As discussed in the above sections, the higher turbine backpressures associated with an indirect dry-cooling system are not compatible with the existing turbine generator units. Modifications to these machines which involve eliminating the last row of blading with the attendant reductions in load carrying capability are not considered to be viable options. Modifications to these units to allow full load carrying capability with the higher backpressures would require significant new and costly development programs and equipment modifications which are not considered to be viable near term options for these plants.

Therefore, since other more viable closed loop cooling system designs have been demonstrated to be available if alternative cooling measures are required, it is not believed that dry-cooling should be investigated further at these plants.



## **7.0 REFERENCES**

- (1) Report on Cost-Benefit Analysis of Operation of Hudson River Steam Electric Units With Once-thru and Closed Cycle Cooling Systems, July 1, 1977.
- (2) Economic and Environmental Impacts of Alternative Closed-Cycle systems for Indian Point Unit No. 2, December 1, 1974.
- (3) Economic and Environmental Impacts of Alternative Closed-cycle Cooling Systems for Indian Point Unit No. 3, January 1976.
- (4) Cooling Tower Study, Bowline Point Station, Units 1 and 2, June 1977.
- (5) Central Hudson Gas & Electric Corporation, Roseton Steam Electric Station Cooling Tower Installation Study, February, 1978.
- (6) Consent Order from the Supreme Court of the State of New York, dated March 13, 1992.
- (7) Roseton Generating Station Engineering, Environmental (Non-biological), and Economic Aspects of a Closed-Cycle Cooling System, July 1977.
- (8) "Air Cooled Condensers", Power, January 1990.
- (9) USA FETAC Air Weather Service Data, Newburgh, New York, 1942-1969.
- (10) Environmental Analysis of Natural Draft Cooling Towers for Bowline Generating Station, January 1977



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January 19, 1993

Stone & Webster Engineering Corporation  
245 Summer Street  
Boston, MA 02210

NOTED JAN 25 1993 T. MUSAFF

Attention: Mr. Tom Adams

Subject: Fine Screen Evaluation

Dear Tom:

The attached report evaluates the impact of retro-fitting the existing intakes at Indian Point, Roseton and Bowline Stations.

The chart breaks the project into five (5) major considerations and determines the degree of impact to each of the screen modules. The write up discusses the impact to each module and provides a course of action. The sketches have been provided to supplement and clarify the written discussion.

Please review this information and then give me a call and we can discuss site by site.

Regards,



Michael Quick  
Product Sales Manager  
Traveling Water Screens

MQ/mmf

**Project:** Backfitting existing thru-flow traveling water screens with 2mm - 6mm opening screening media.

Backfit being explored as a means to prevent marine life larvae from being entrained into cooling water system. Current screening media is equivalent to  $\frac{1}{8}$  square opening.

**Project Scope:** Address operational and environmental considerations that are brought about by the proposed backfit and anticipated increased velocities and differentials.

Provide potential mechanical and material enhancements that will ensure adequate performance and reliability.

Identify areas that will require either laboratory testing/demonstration or field testing/demonstration or both.

Equipment should provide service life and reliability at least equivalent to the current designs employed.

#### (1) Drive

Consideration No. 1, Increased Velocities:

- Potentially Higher Differentials
- Higher Traveling Speeds
- Fatigue

The attached charts indicate the increased velocities, resulting from using the smaller opening mesh, will generate higher differentials at <sup>what does this mean</sup> [equal percent clean figures] for the current mesh. The rate of blinding is dramatically increased, thus the potential for a catastrophic failure is increased unless the following changes are made to the drive system. These items can all be [numerically modeling] no field or lab work. ? modeled

- Possibly the use of a 4-speed or variable speed (preferred) drive to more accurately respond to the rapidly changing conditions.
- Possibly increased horsepower to pull out of potentially higher differentials.
- Higher operating speeds to respond to increased loading and plugging rates. Recommend 5-10-15-30 FPM (see charts).
- Components of the drive system need to be evaluated in light of the increased cycles expected. Bearings, seals, etc will need to be of the heaviest duty possible.



Evaluation of the above seals should be performed in our lab.

(3) Baskets

Consideration No. 1, Increased Velocities:

- Higher Differentials
- Basket Vortexting (lip interior\_?)
- Fatigue Failure
- Structural integrity

The basket will see the highest level of impact as a result of the proposed change in screening media. The higher differentials and increased travel speeds will place a great deal of stress on the baskets. A numerical model of the loading needs to be made along with laboratory tests.

The mesh, in particular, warrants close scrutiny. A review of literature on past attempts to utilize fine screening medias in a backfit mode have shown a high incidence of failures. These failures occur from two (2) primary sources.

- Structural damage from large sharp, heavy debris
- Fatigue failures in the areas of clamping the mesh to the basket frame.

For a screening media to be successful, the following attributes need to be present in the design.

- Mesh to be constantly and evenly tensional so as to maintain as strong a surface area as possible both in ascending and descending sides.
- Tensioning to be accomplished in a manner that will eliminate load concentrations and "flexing" of mesh over a stationary surface, such as clamp bias etc.
- The screening media needs to be supported in the case of substantial differentials.

It has already been determined that the finer screening media radically impacts the fish bucket. The current configuration in use at Indian Point with larger opening media has proven itself to be highly successful in providing a static environmental for captured marine life. In the laboratory, when the finer screening media was supplanted for the larger media, violent vortexting to place. It will be necessary to reconfigure the basket profile to accommodate the finer screening media. This is presently being done in our laboratory.

took?

It is strongly recommended that non-metallic materials be used on all basket components, where possible, to achieve performance requirements, both operationally and environmentally.

- Consideration No. 2, Spray Wash:
- Outside sprays
  - Higher GPM's, lower PSI's
  - Longitudinal sprays

- Front discharge

The basket again is of a main concern in this area. It is anticipated that the inside spray will be greatly impacted by the finer screening media. Inside sprays will be highly diffused and more reliance will be placed on outside sprays. The debris will "mat" on the surface of the finer screening and tend to "sluff" or slide off the screening surface. A large amount of it will end up in the fish bucket.

The possibility of a front cleaned bucket should be explored, which will necessitate redesign to prevent vortexing. Survival in the debris laden bucket also needs to be examined. Front discharge will also require the use of longitudinal sprays to clean the corners of the bucket. This was demonstrated on previous designs. A front clean fish bucket will require a totally new design and will require a complete new work up.

Consideration No. 3, Sealing: - Basket to Basket

- The running clearance between baskets is typically approximately 1/8". The solution to sealing between baskets will vary with the basket design.
- The metallic fish basket currently in use at Indian Point Station will utilize a sealing strip bolted to and extending upward from the face of the upper lip of the basket. This seal would bridge the gap between adjoining baskets and maintain constant contact with the curved surface of the bucket forming lower lip.
- The 1/4" die formed basket currently in use at Roseton Station will utilize a sealing strip bolted to and extending downward. This seal would bridge the gap between adjoining baskets and maintain constant contact with the curved outer surface of the upper lip.

↙ The angle lip style basket used at Bowline Station will require total replacement.

cost implications

Additional "considerations" may render current basket designs unacceptable. Initial evaluation indicates the use of the non-metallic basket with front removal marine life capability may be the preferred design.

- Non-metallic baskets will utilize a large "P" seal bolted to the outside edge of the flange to which the basket wire is bolted. The "P" seal maintains constant contact with the curve face of the upper "D" shaped basket lip. Additionally a seal will be bonded to the face of the lower lip extending to a point overlapping and contacting the upper lip.

(4) Chain

Consideration No. 1, Increased Velocities: - Fatigue Failures

The current material and design of the carrying chain will need to be reviewed. Due to increased speeds and potentially higher loadings, the fatigue factors and cyclic loadings are going to be dramatically increased. The wearability and strengths need to be analyzed numerically and both in



Longitudinal sprays, front discharge and the spray shower at water level are all new and will need laboratory and field work.

- Consideration No. 4 & 5:
- Debris Behavior
  - Debris Removal
  - Marine Life Behavior
  - Marine Life Discharge
  - Marine Life Transport

The anticipated impact from the fine screen media will cause a total re-think on the interaction of debris and marine life. A complete evaluation, design, laboratory and field should be conducted.

(7) Housing/Troughing

- Consideration No. 1, Increased Velocities:
- Potentially Higher Differentials
  - Higher Travel Speeds

The possible change in drive assembly may result in the requirement for a new chain guard.

- Consideration No. 2, Spray Wash:
- Front Discharge
  - Higher GPM
  - Longitudinal Sprays

Any change in the spray configuration will directly impact the design of housings and possibly the troughing/collection system. Testing of the basket design will ultimately dictate the housing configuration.

- Consideration No. 3, Sealing:
- Trough to Basket

With the added importance of removal of very small organisms, refinement of the basket to trough transfer area will be required. This minimizes reintroduction of screening into the intake channel.

Considerations 4 & 5, Debris & Marine Life Handling:

Evaluation of the transport capability of the housings and trough will be required once the basket and spray wash configuration has been finalized.

(8) Controls

- Consideration No. 1, Increased Velocities:
- Higher Differentials
  - Higher Travel Speeds

The requirement for increased travel speeds, as discussed in (1) Drives, will necessitate a matching control package capable of reacting to the conditions as shown on the charts. Controls, in response to

changes in differential head, will adjust travel speed proportionally. This item can be numerically modelled - no field or lab work.

Consideration No. 2, Spray Wash: - Usage of Higher GPM

Higher travel speeds will require evaluation of spray vs travel, this may require the interaction of controls and auxiliary spray system valving.

## **ANALYSIS, TESTING VERIFICATION**

The general approach to each consideration listed on accompanying matrices and their related anticipated impacts is as headlined above.

- Analysis-numerical, mathematical and historical to identify areas of deficiency, and also prior applications which may resolve the considerations.
- Testing-physical tests - full scale and modeled were necessary in a laboratory environment to demonstrate the problems and remedial actions.
- Verification - this means full scale application of the remedial actions in the field, under real life conditions.

### **Consideration No. 1, Increased Velocities**

The velocities can be calculated using accepted formulae. The resulting differentials can be calculated the same way. Analysis of the increased loadings from this new differentials, increased cycles can be performed using several accepted processes. Certain critical, easily tested, components need to set up a laboratory.

- Basket/mesh flexures and cycles under load
- Chain component reevaluations
- Hydrodynamics of fish bucket to minimize vortexing intensity

Field verification of all anticipated impacts and their remedial actions needs to be performed.

### **Consideration No. 2, Spray Wash Diffusion**

It will be necessary to gather the data on this consideration empirically. There is some historical information available from prior installations but it will be necessary to perform specific test for each installation.

These must be done in conjunction with Consideration No. 4 & 5 (debris and marine life).

- Baseline current spray system with fine screening media at velocities and speeds.
- Test various remedial configurations proposed to determine most effective one.

These remedial changes will then need a full scale, real time verification.

### **Consideration No. 3, Sealing**

This consideration is extremely important to environmental performance. Analysis can help determine materials and applications but laboratory testing is required to demonstrate effectiveness. Observation of the action of the many seals required is only possible in a laboratory environment. The tests should be done in conjunction with Consideration No. 4 & 5 (Debris & Marine Life).

- Baseline current screen sealing effectiveness by debris, marine life runs on model screen. The need may arise for synthetic replica of the marine life for observation purposes.
- Quantify improvement in performance with debris and marine life runs in model screen.
- Separate testing on mesh-basket frame interface to minimize entrapment and damage to marine life during capture and release.

The remedial changes will have to be verified in a full size, real time field runs.

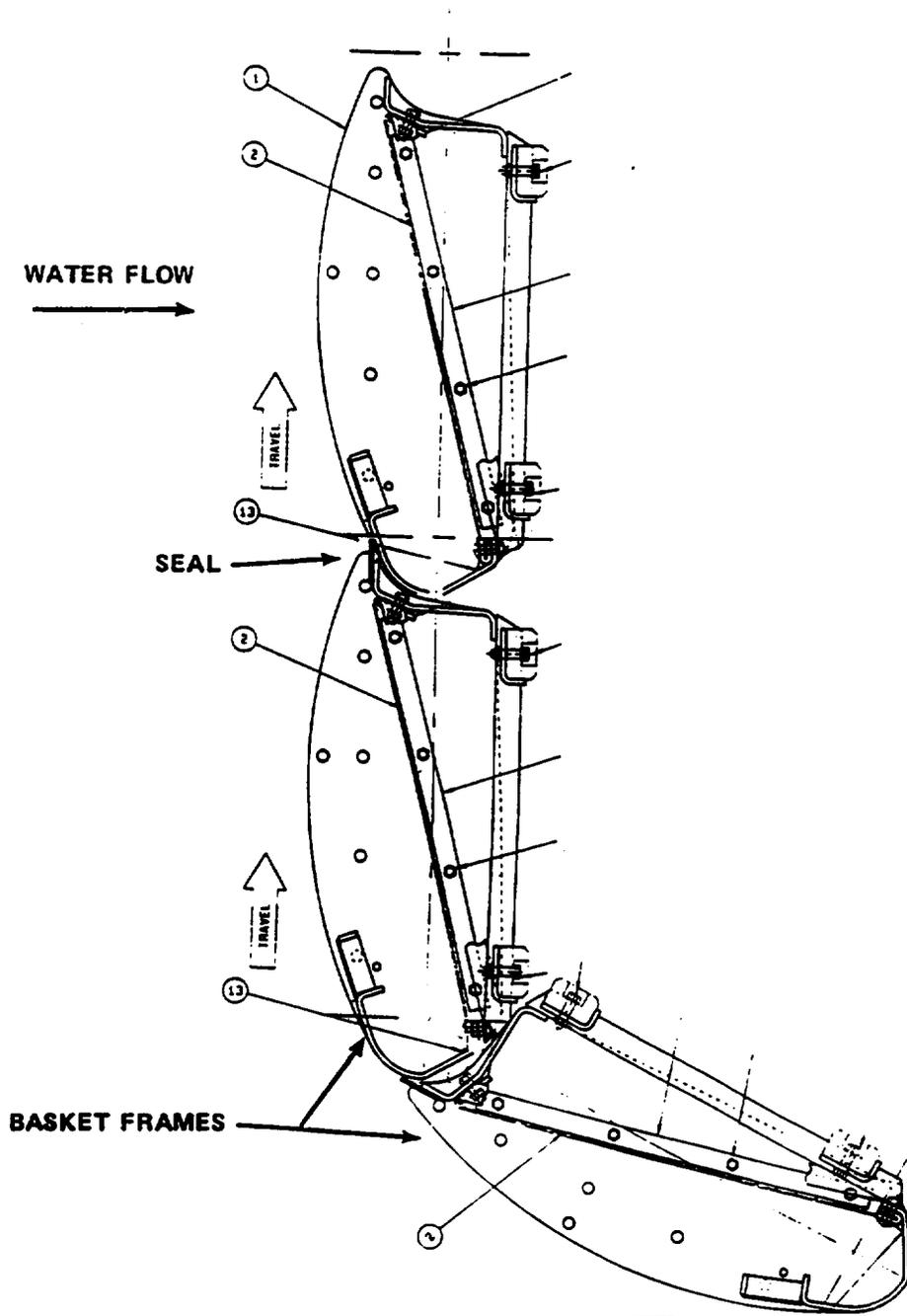
#### Considerations No. 4 & 5 - Debris and Marine Life Handling

The majority of the work will be done in conjunction with No. 2 and 3 above. The transport systems will need to be evaluated on a full scale basis by field verification.

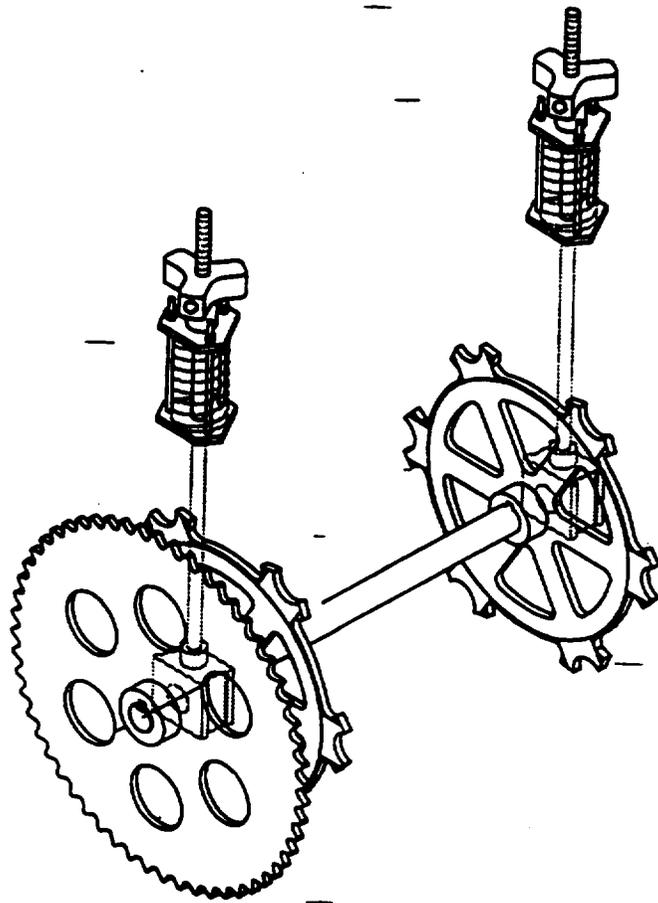
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ORDER NO	
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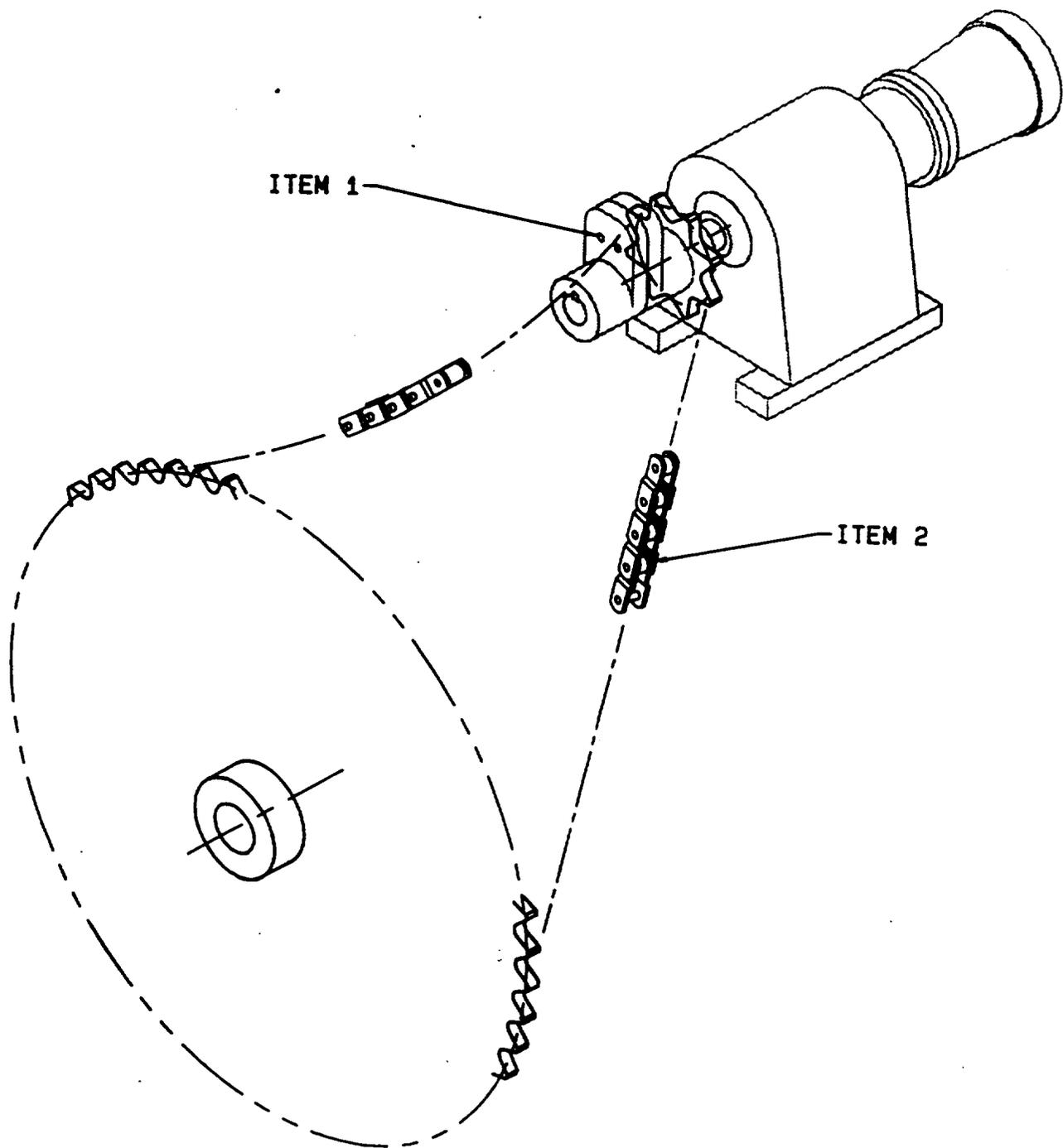
**BASKET CONFIGURATION**



**FISH BASKET AT CON. ED & NYPA**



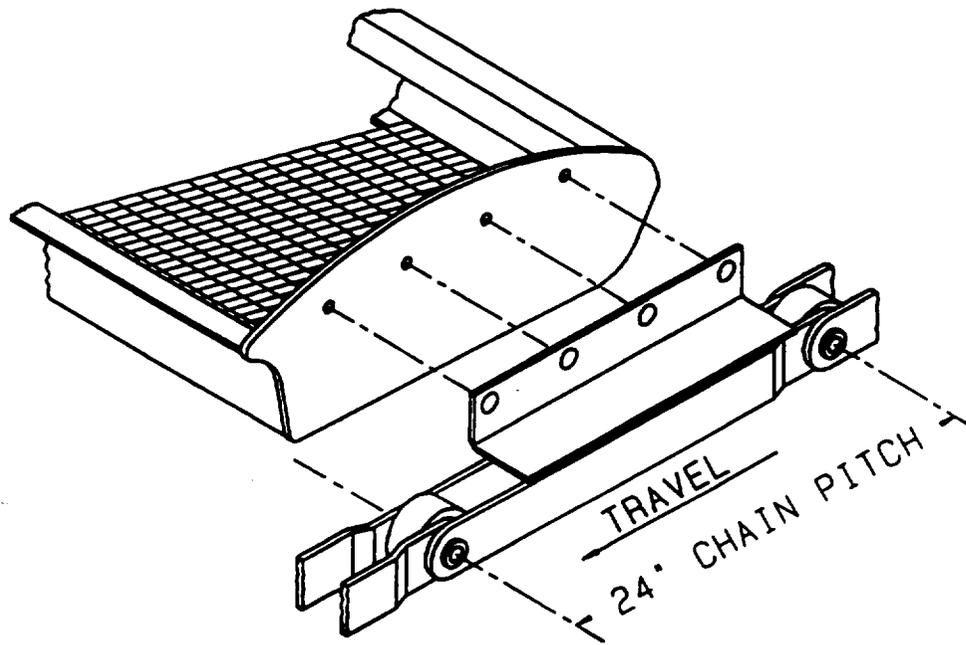
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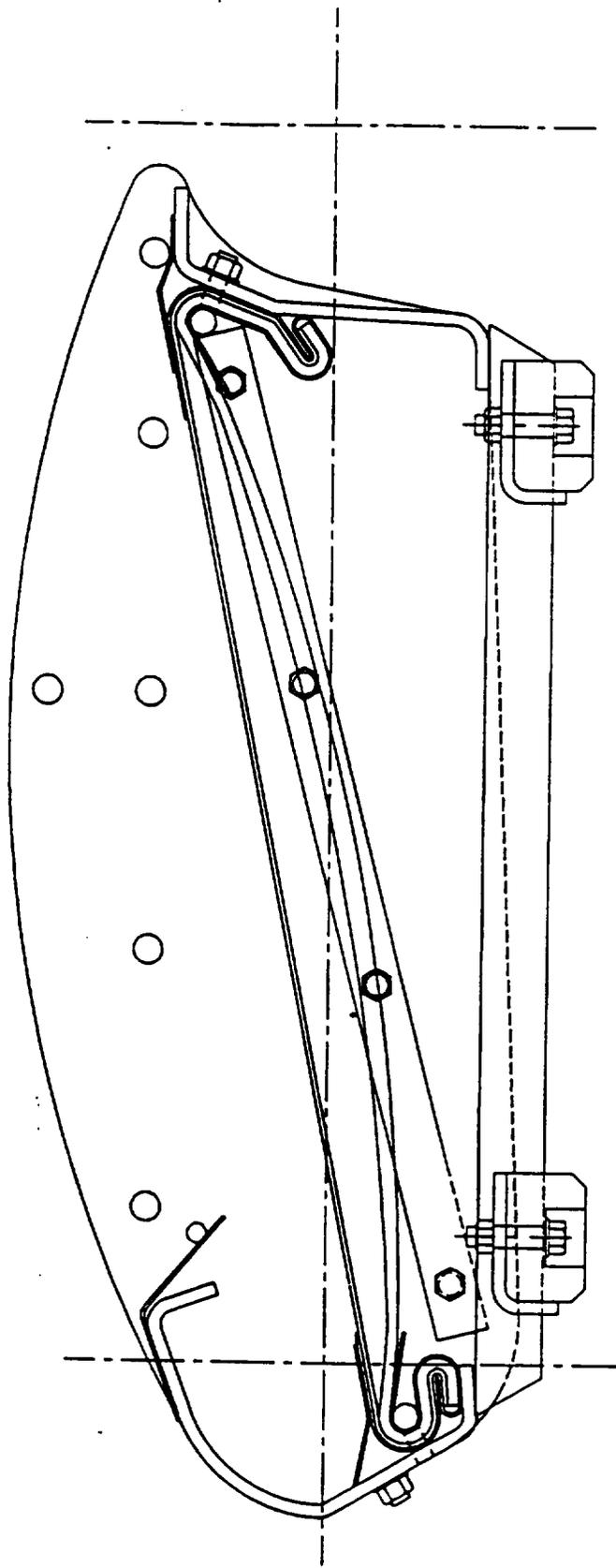
ITEM 1

ITEM 2

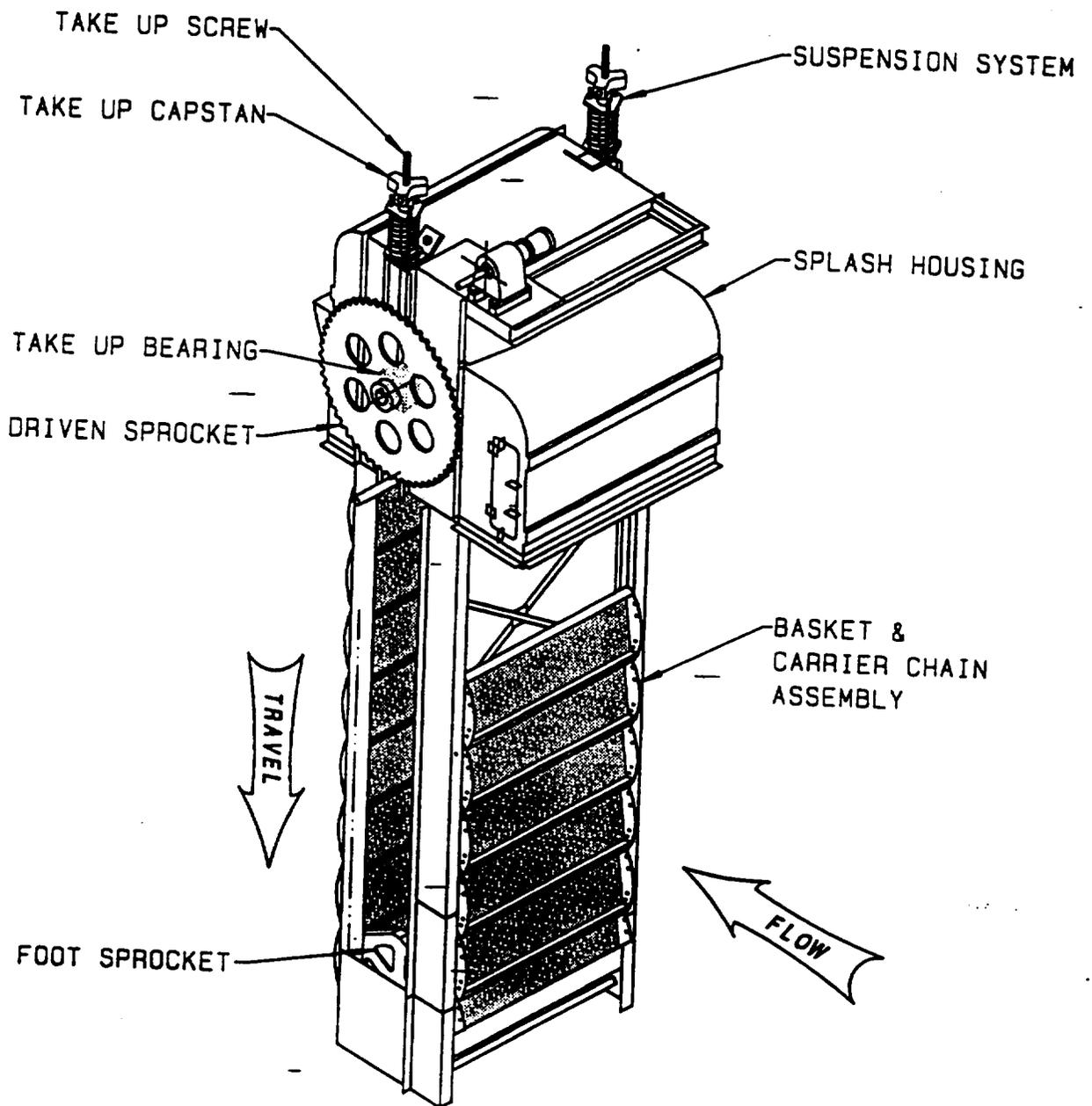
DRIVE CHAIN & SPROCKETS

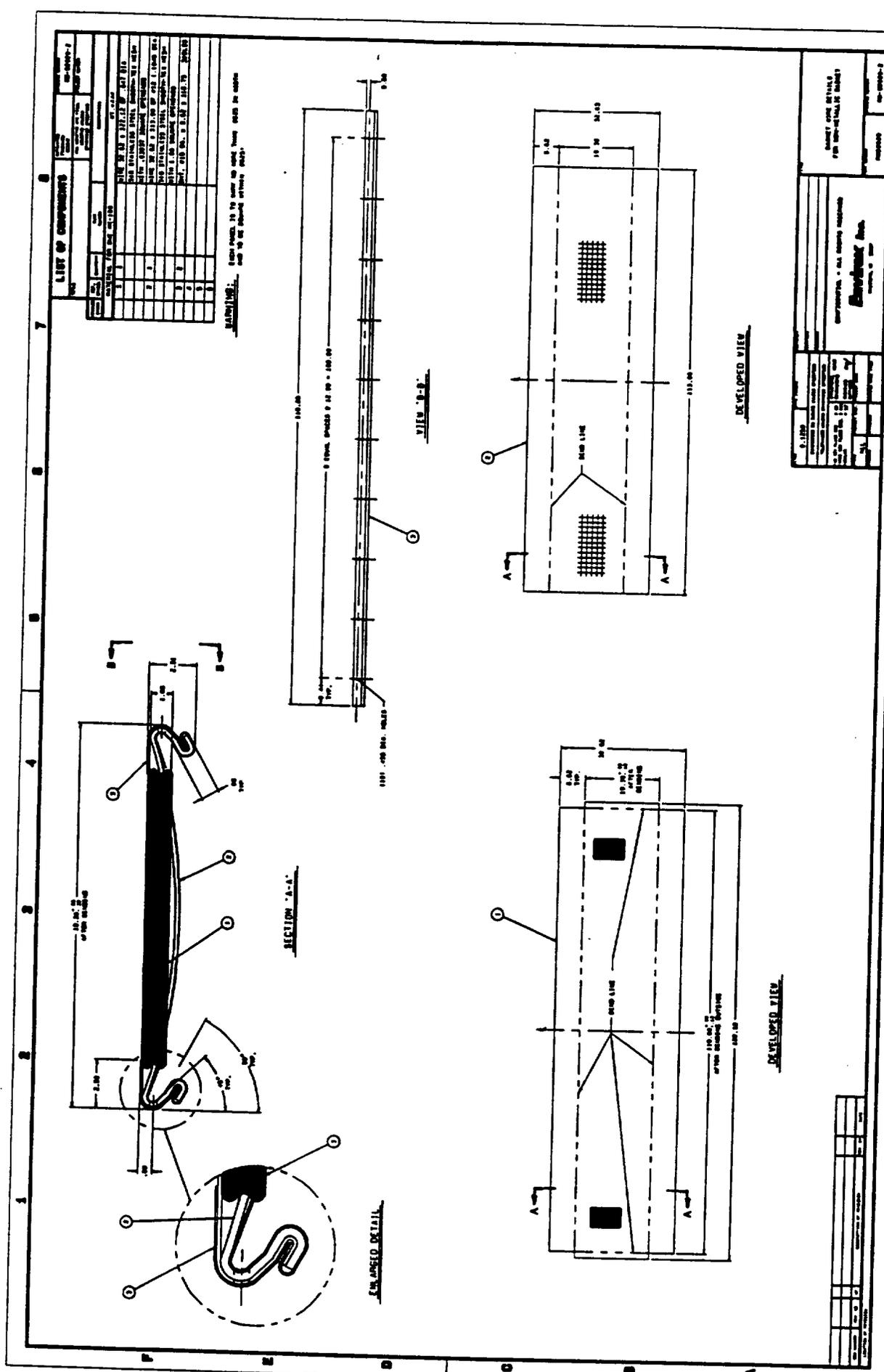


FISH BASKET



000-10 00000  
0000 111





**LIST OF COMPONENTS**

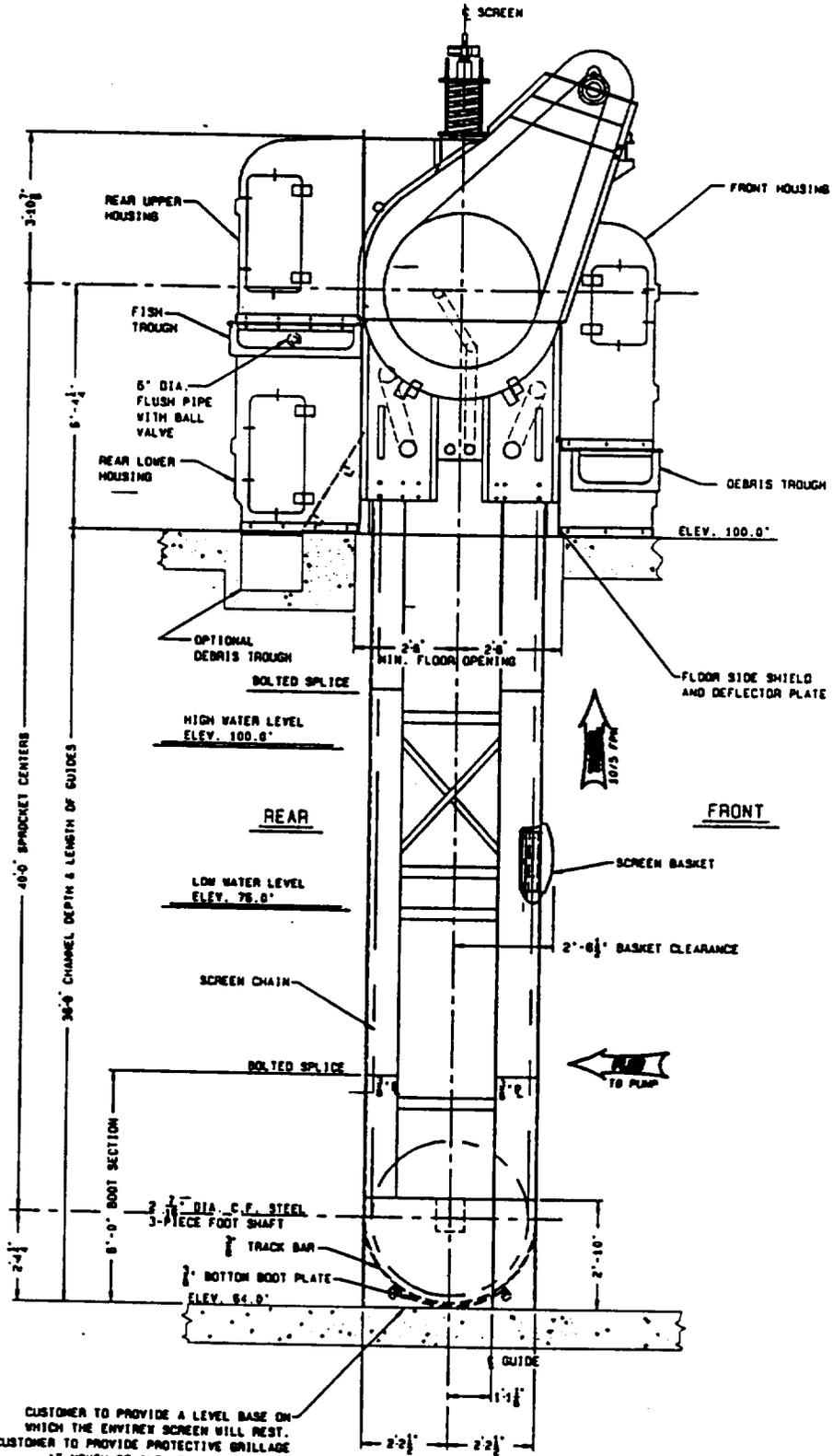
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2	WIRE, COPPER, 18 GA., 10 FT.	1	
3	WIRE, COPPER, 18 GA., 10 FT.	1	
4	WIRE, COPPER, 18 GA., 10 FT.	1	
5	WIRE, COPPER, 18 GA., 10 FT.	1	
6	WIRE, COPPER, 18 GA., 10 FT.	1	
7	WIRE, COPPER, 18 GA., 10 FT.	1	
8	WIRE, COPPER, 18 GA., 10 FT.	1	
9	WIRE, COPPER, 18 GA., 10 FT.	1	
10	WIRE, COPPER, 18 GA., 10 FT.	1	

NOTE: REFER TO DRAWING FOR DIMENSIONS AND MATERIAL SPECIFICATIONS.

PROJECT NO.	DATE
DESIGNED BY	CHECKED BY
DRAWN BY	APPROVED BY
SCALE	

# Envirox Inc.

800 F Street S. Birmingham, Alabama  
BIRMINGHAM, AL 35207

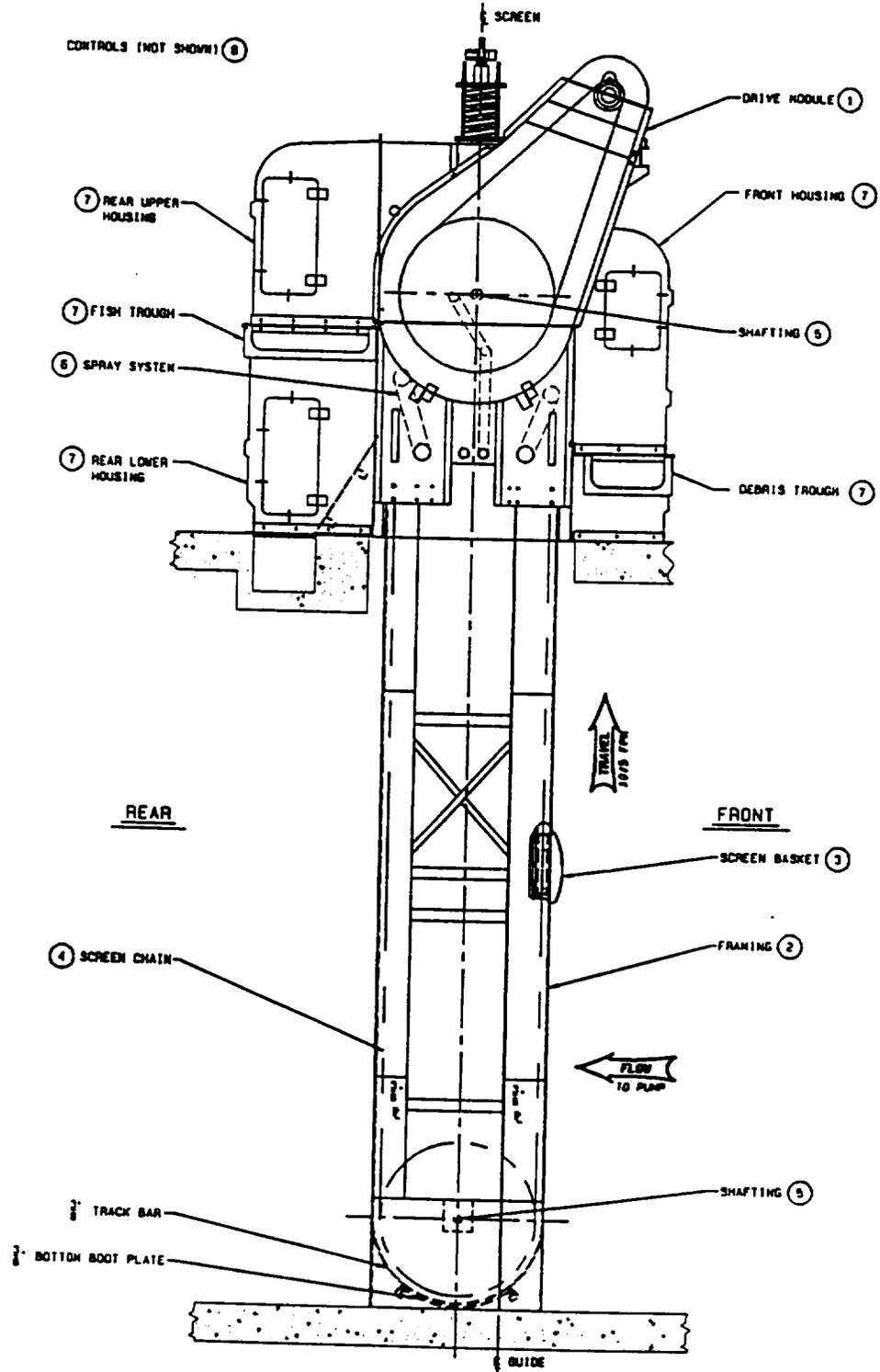


CUSTOMER TO PROVIDE A LEVEL BASE ON WHICH THE ENVIROX SCREEN WILL REST. CUSTOMER TO PROVIDE PROTECTIVE BRILLAGE AT MOUTH OF INTAKE, IF REQUIRED.



# Envirox Inc.

500 F Street & Environmental Services  
MARIETTA, GA 30157





Date: January 13, 1993

**SUMMARY MATRIX FOR OPERATIONAL/ENVIRONMENTAL IMPACT ON SCREENS**

			Y = Yes N = No		Screen Module Impacted E = Evaluate    N = New M = Modify    X = Okay								Y = Yes N = No	
			Operational	Environmental	Drive (1)	Frame (2)	Basket (3)	Chain (4)	Shaft Assembly (5)	Spray (6)	Housing/Trough	Controls (8)	Lab Test	Field Test/Verify
1	Increased velocities from reduced open area of screening media	<ul style="list-style-type: none"> <li>• Potentially higher differentials</li> <li>• Basket vortexing</li> <li>• Higher traveling speeds to maintain differential control</li> <li>• Fatigue failures of components due to accelerated cycles</li> <li>• Structural integrity of finer mesh</li> </ul>	Y	Y	N	E	N	E	X	X	X	X	Y	Y
			N	Y	X	X	N	X	X	X	X	X	Y	Y
			Y	N	N	E	E	E	E	E	X	N	N	Y
			Y	N	E	E	E	E	E	E	E	E	Y	Y
			Y	N	X	X	N	X	X	X	X	X	Y	Y

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			Operational	Environmental	Drive (1)	Frame (2)	Basket (3)	Chain (4)	Shaft Assembly (5)	Spray (6)	Housing/Trough	Controls (8)	Lab Test	Field Test/Verify
2	Spray wash design review due to fine mesh diffusing inside spray	<ul style="list-style-type: none"> <li>• Usage of outside spray in lieu of inside spray</li> <li>• Usage of higher GPM's and relatively lower PSI's</li> <li>• Usage longitudinal sprays</li> <li>• Usage of front discharge of marine life</li> <li>• Spray shower at or near water level to flush mesh</li> </ul>	Y	Y	X	X	E	X	X	M	E	E	Y	Y
			Y	Y	X	X	E	X	X	M	E	E	Y	Y
			Y	Y	X	X	E	X	X	M	E	E	Y	Y
			N	Y	X	E	N	X	N	M	E	E	Y	Y
			N	Y	X	E	X	X	X	M	X	E	Y	Y

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3	Sealing between member/components to provide consistent 2mm-6mm opening	<ul style="list-style-type: none"> <li>• Seal frame to basket inclusive of boot</li> <li>• Seal basket to basket</li> <li>• Seal structure to intake (Flow orientation basket)</li> <li>• Seal between Basket to trough</li> </ul>	Y	Y	X	M	E	E	X	X	X	X	Y	Y
			Y	Y	X	M	E	E	X	X	X	X	Y	Y
			Y	Y	X	M	E	E	X	X	X	X	Y	Y
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Date: January 13, 1993

**SUMMARY MATRIX FOR OPERATIONAL/ENVIRONMENTAL IMPACT ON SCREENS**

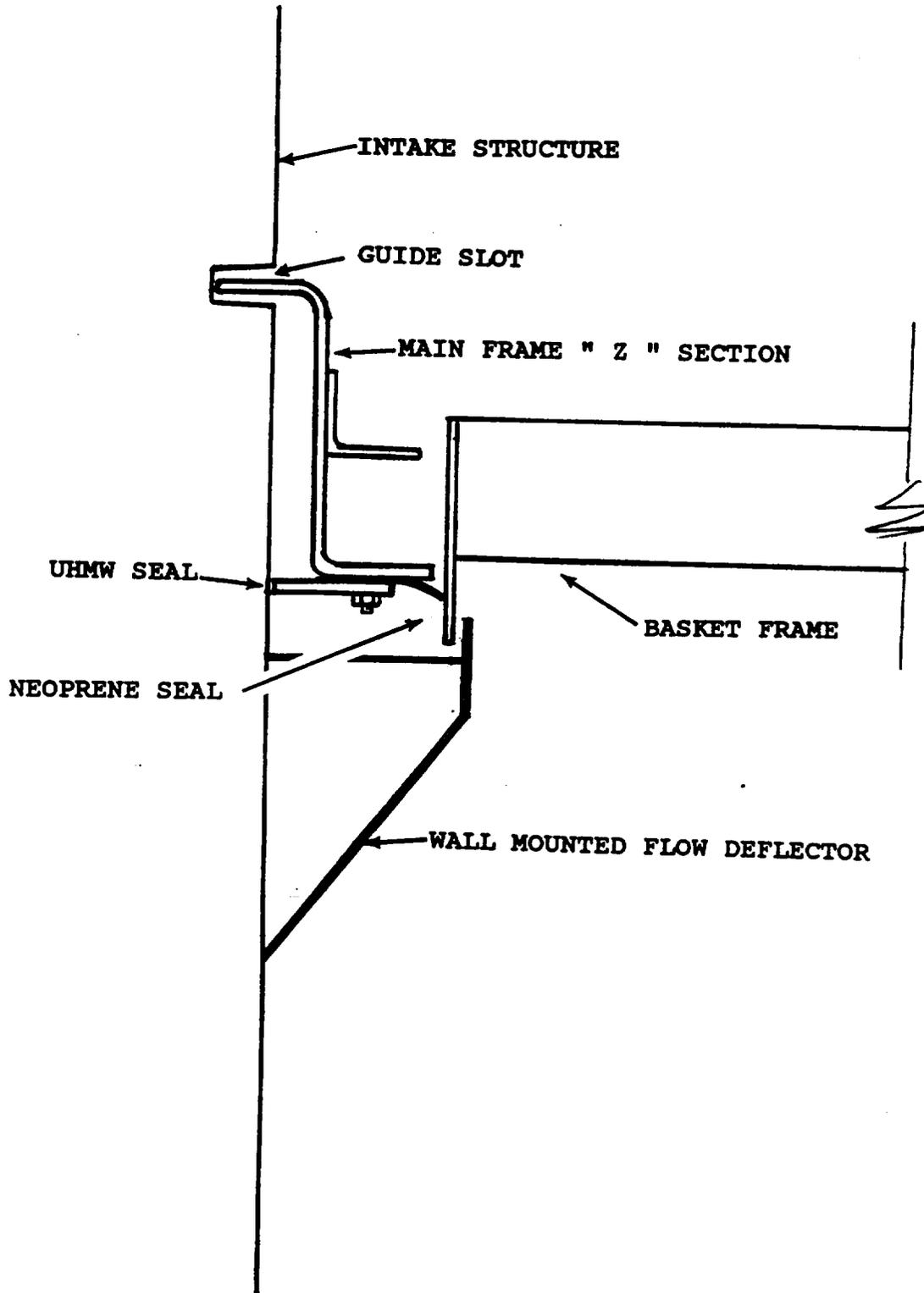
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			Operational	Environmental	Drive (1)	Frame (2)	Basket (3)	Chain (4)	Shaft Assembly (5)	Spray (6)	Housing/Trough	Controls (8)	Lab Test	Field Test/Verify
4	Debris handling and transport	• Debris behavior/ interactive with screening surface (orientation) mattingus stapling	Y	N	X	X	E	X	X	E	X	X	Y	Y
		• Debris removal from screening surface	Y	Y	X	X	E	X	X	E	E	X	Y	Y
		• Large debris impact in fine mesh	Y	Y	X	X	E	X	X	X	X	X	Y	Y
		• Debris transport	Y	N	X	X	E	X	X	X	E	X	Y	Y

Date: January 13, 1993

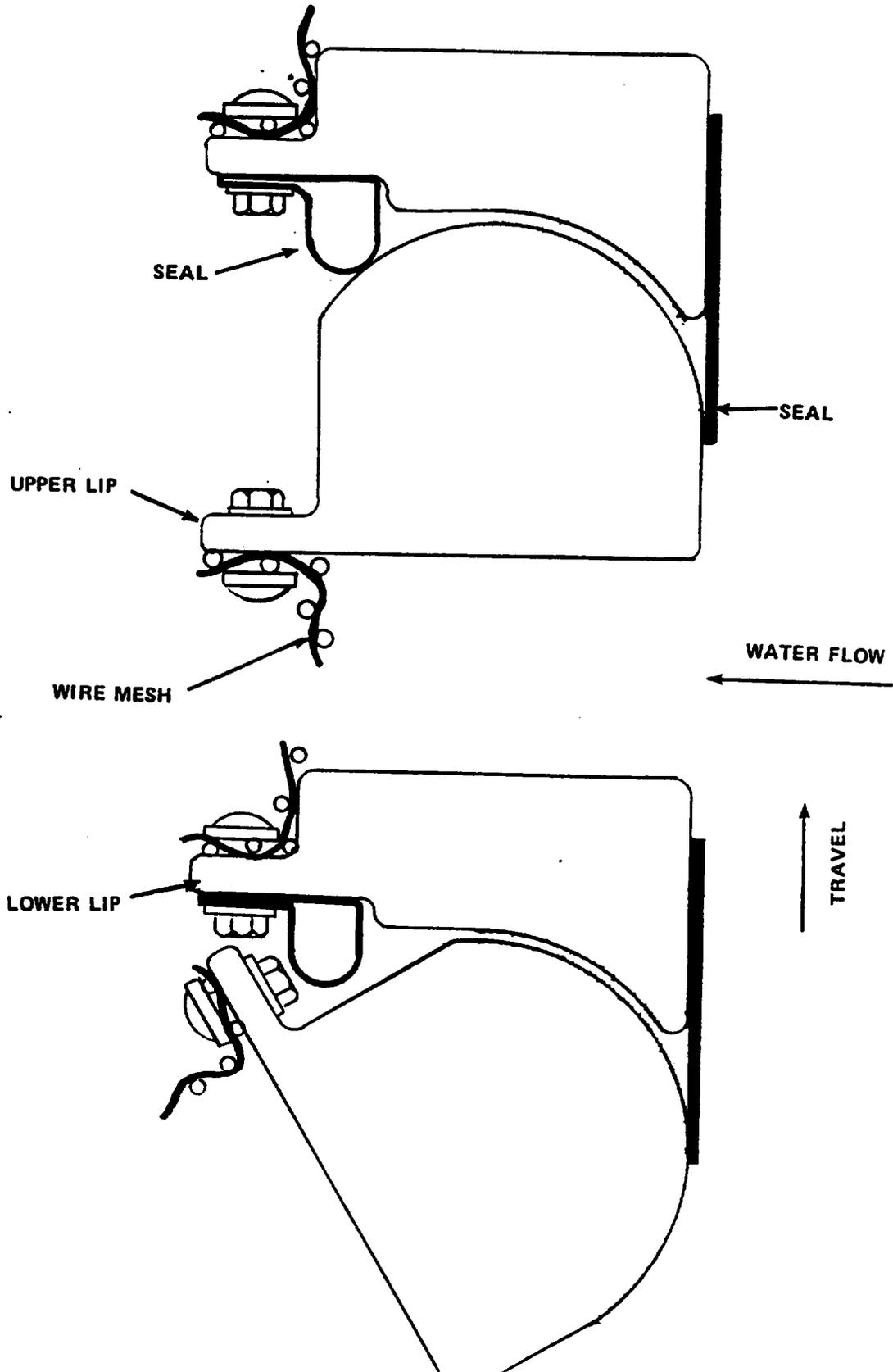
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	Consideration	Anticipated Impact	Y = Yes N = No		Screen Module Impacted E = Evaluate N = New M = Modify X = Okay								Y = Yes N = No	
			Operational	Environmental	Drive (1)	Frame (2)	Basket (3)	Chain (4)	Shaft Assembly (5)	Spray (6)	Housing/Trough	Controls (8)	Lab Test	Field Test/Verify
5	Marine life handling and transport	<ul style="list-style-type: none"> <li>• Behavior/interaction with screening surface</li> <li>• Removal from surface surface</li> <li>• Marine life transport</li> </ul>	N	Y	X	X	E	X	X	E	X	X	Y	Y
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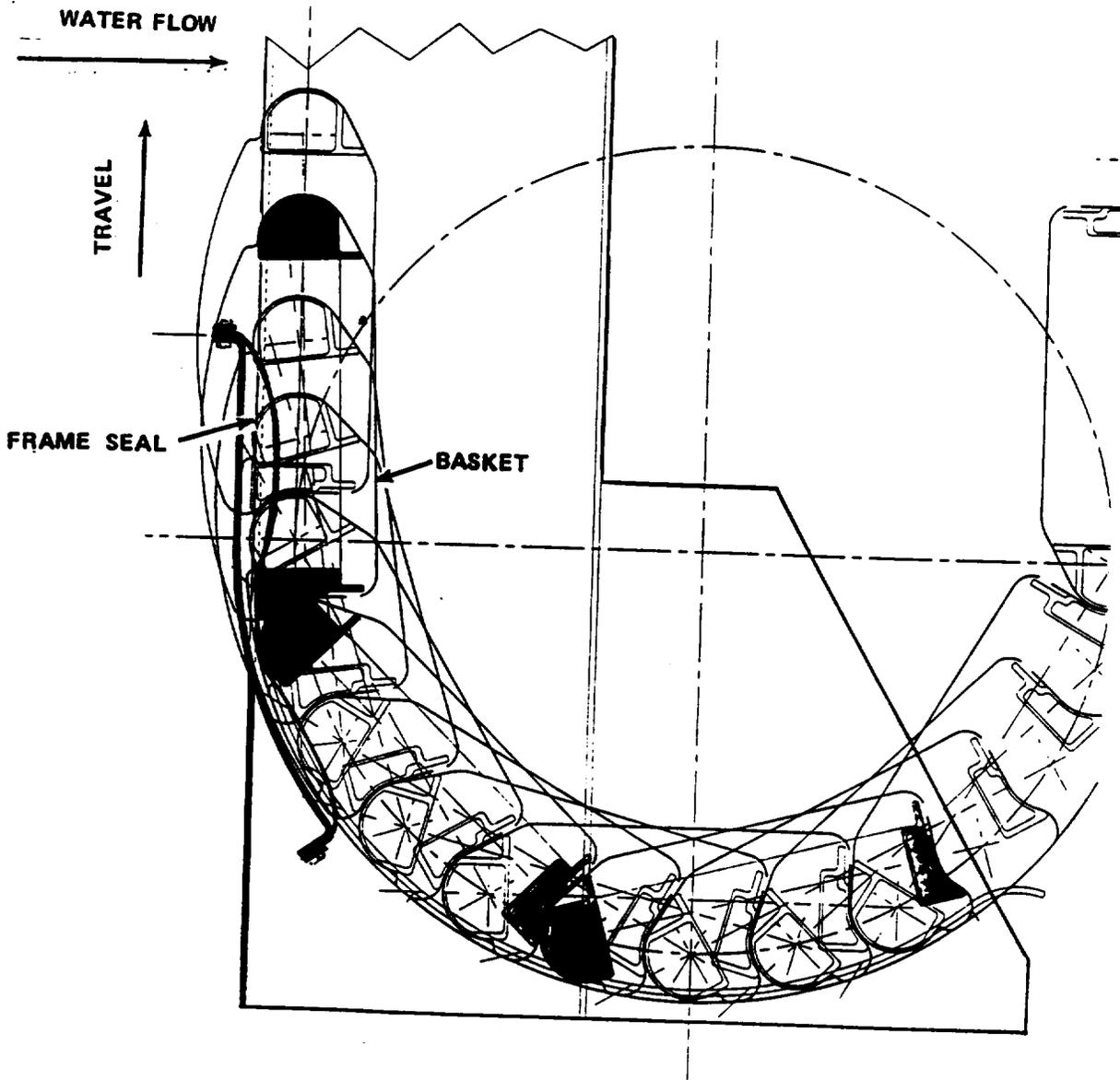
MAIN FRAME TO INTAKE SEAL



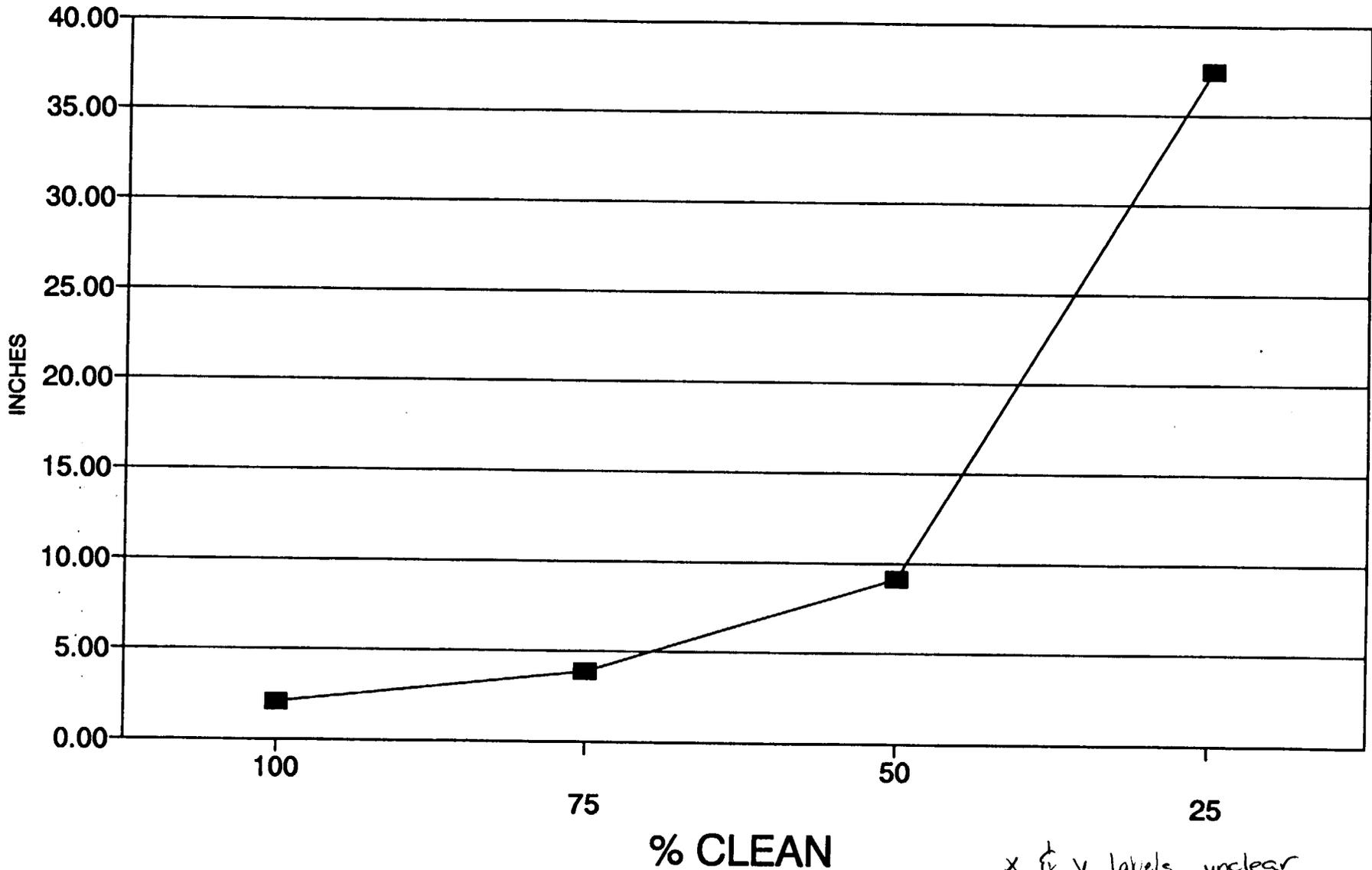
**BASKET LIP CONFIGURATION**



**BOOT SECTION SEAL**



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NYPA IP NO. 3

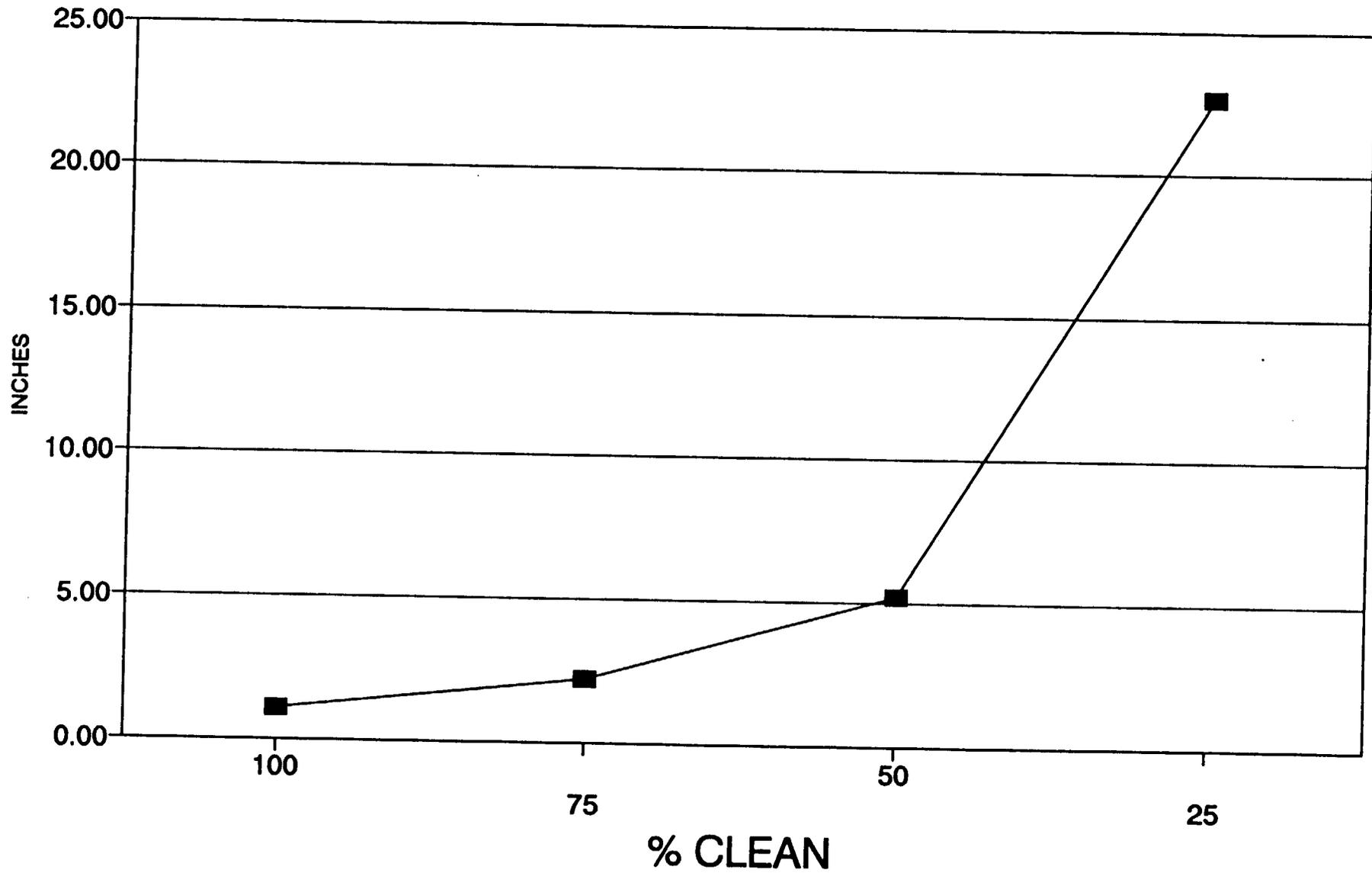


x & y labels unclear

27' Water Depth

■ 2MM FINE MESH

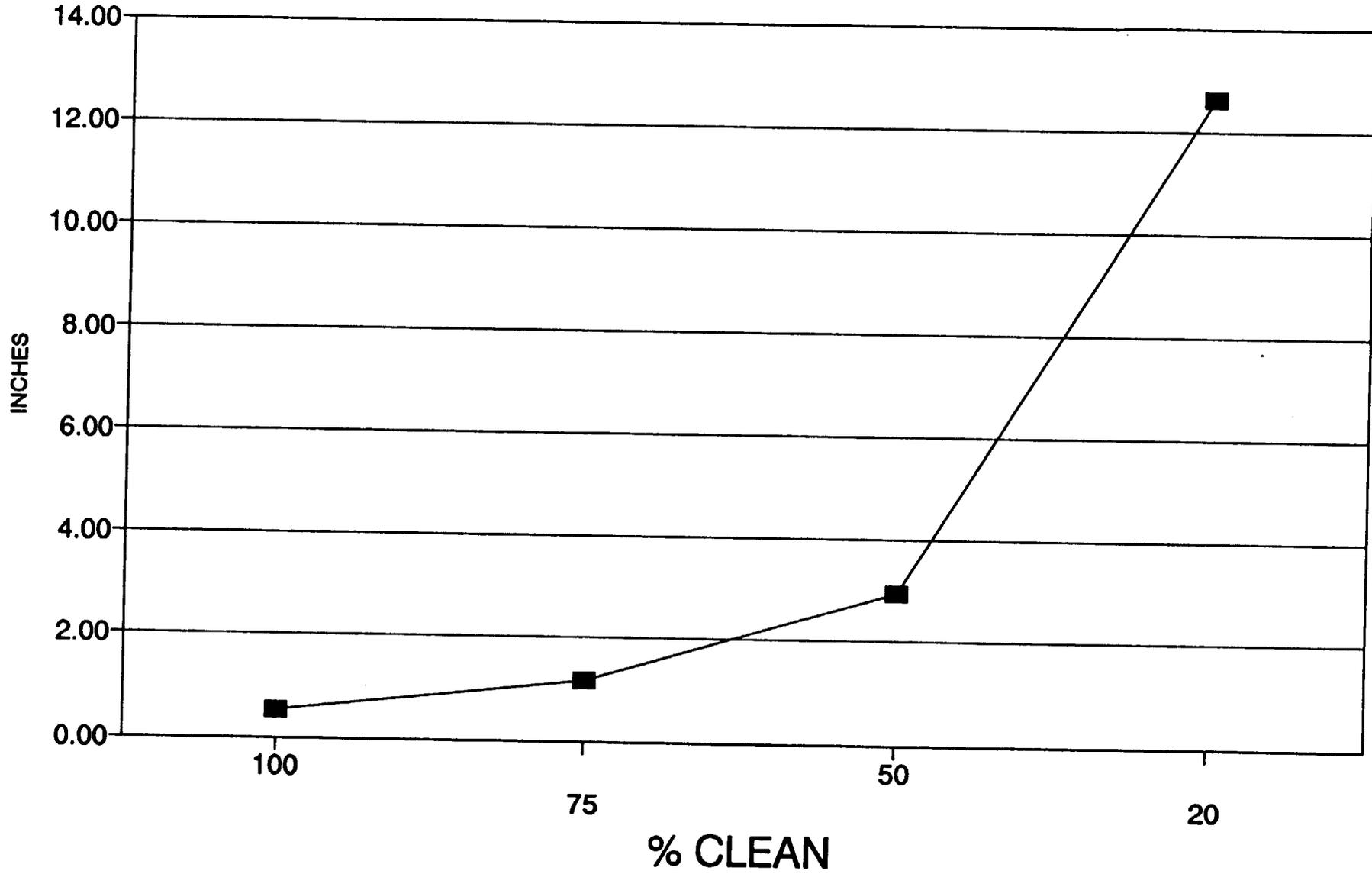
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27' Water Depth

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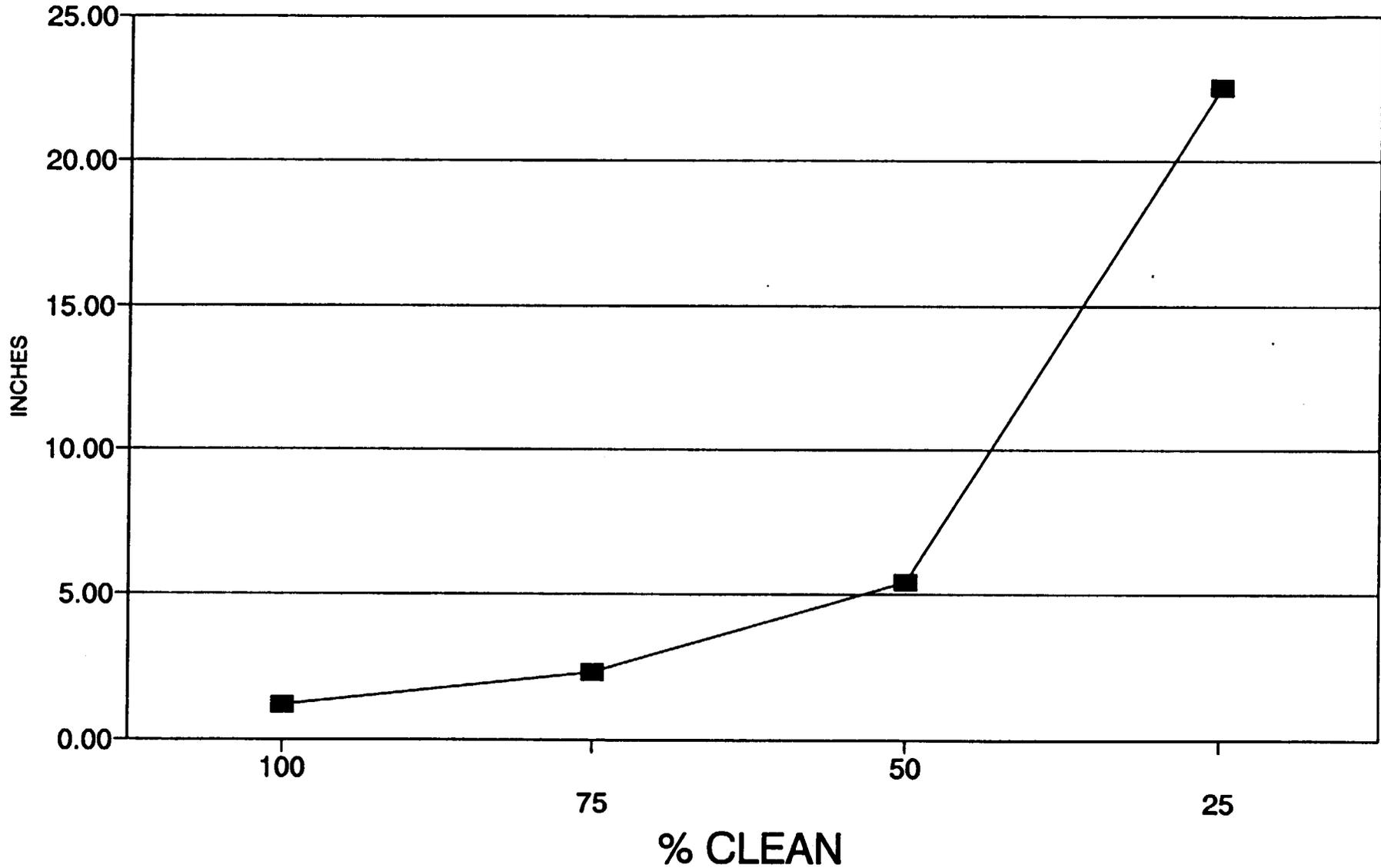
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26' WATER DEPTH

—■— 3/8" DIA. MESH

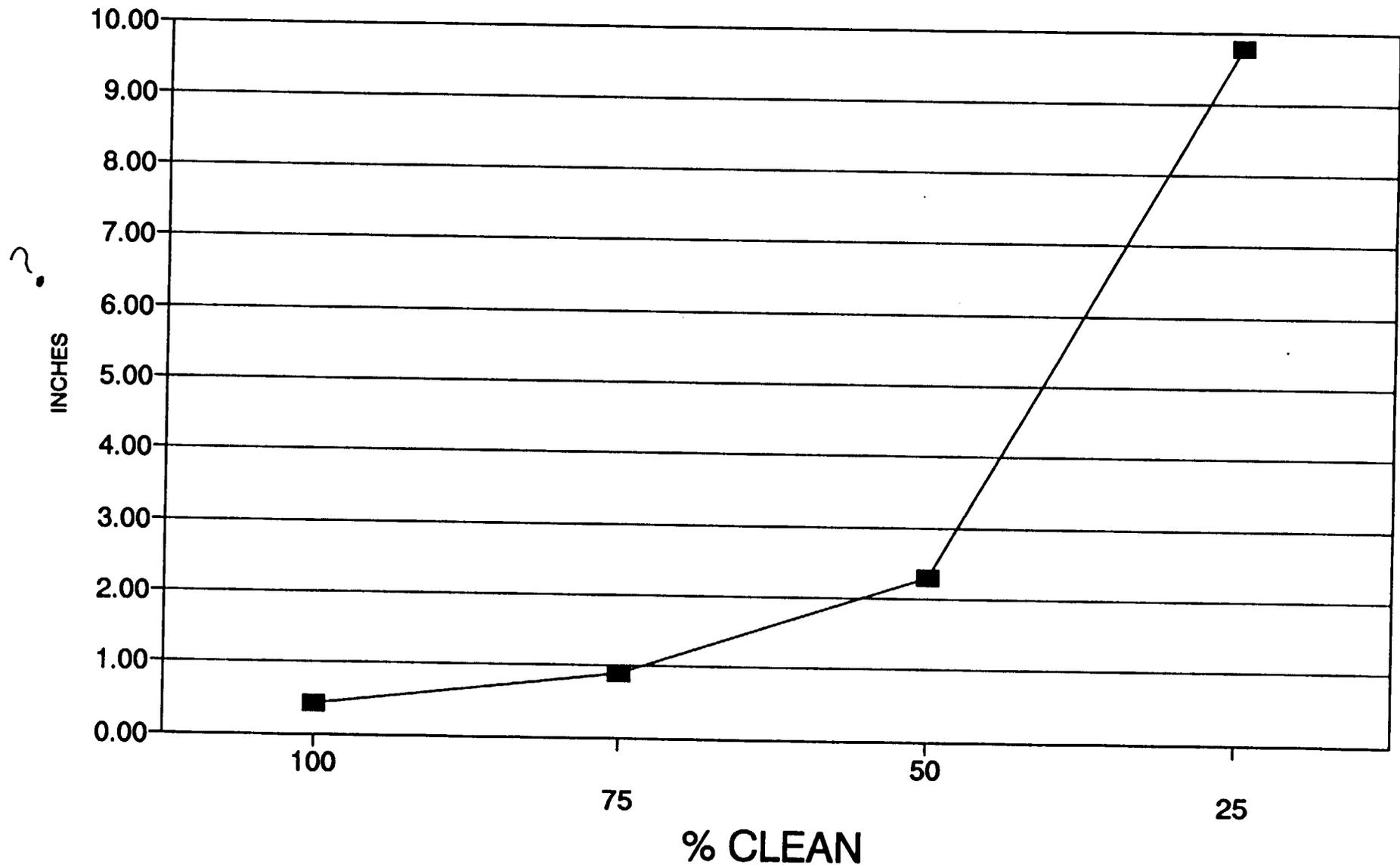
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26' WATER DEPTH

■ 2MM MESH

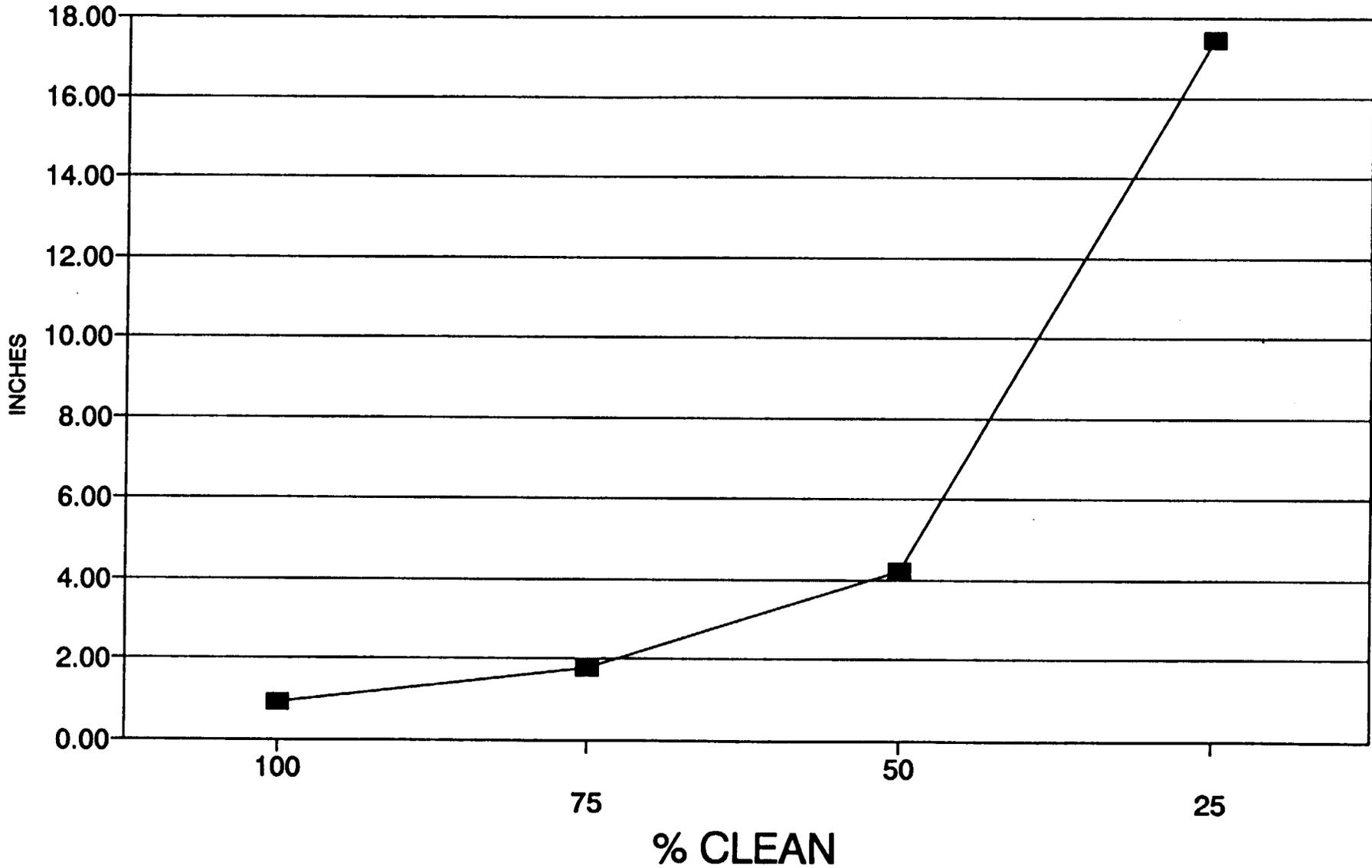
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28.5' WATER DEPTH

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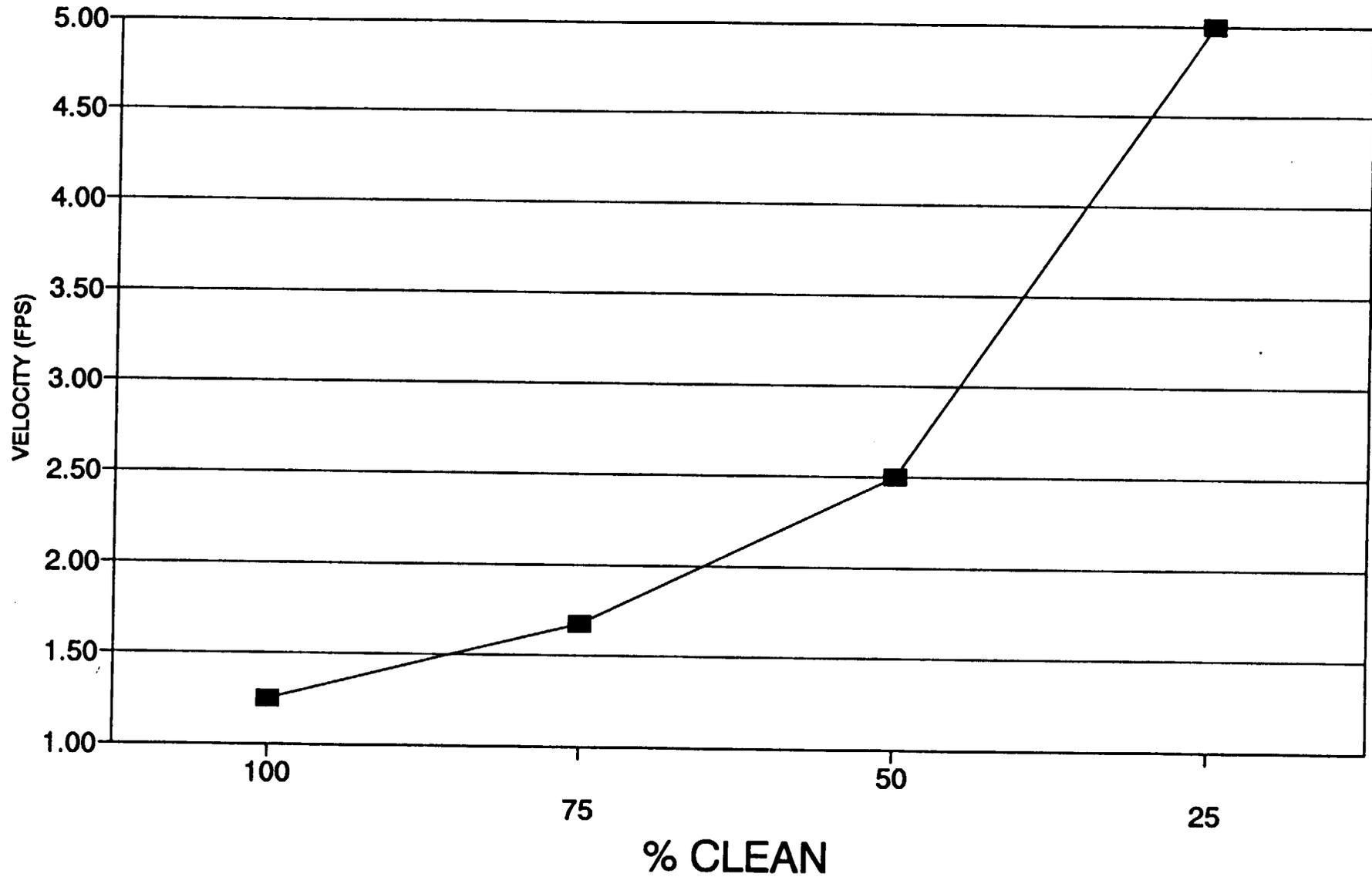
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28.5' WATER DEPTH

■ 2MM MESH

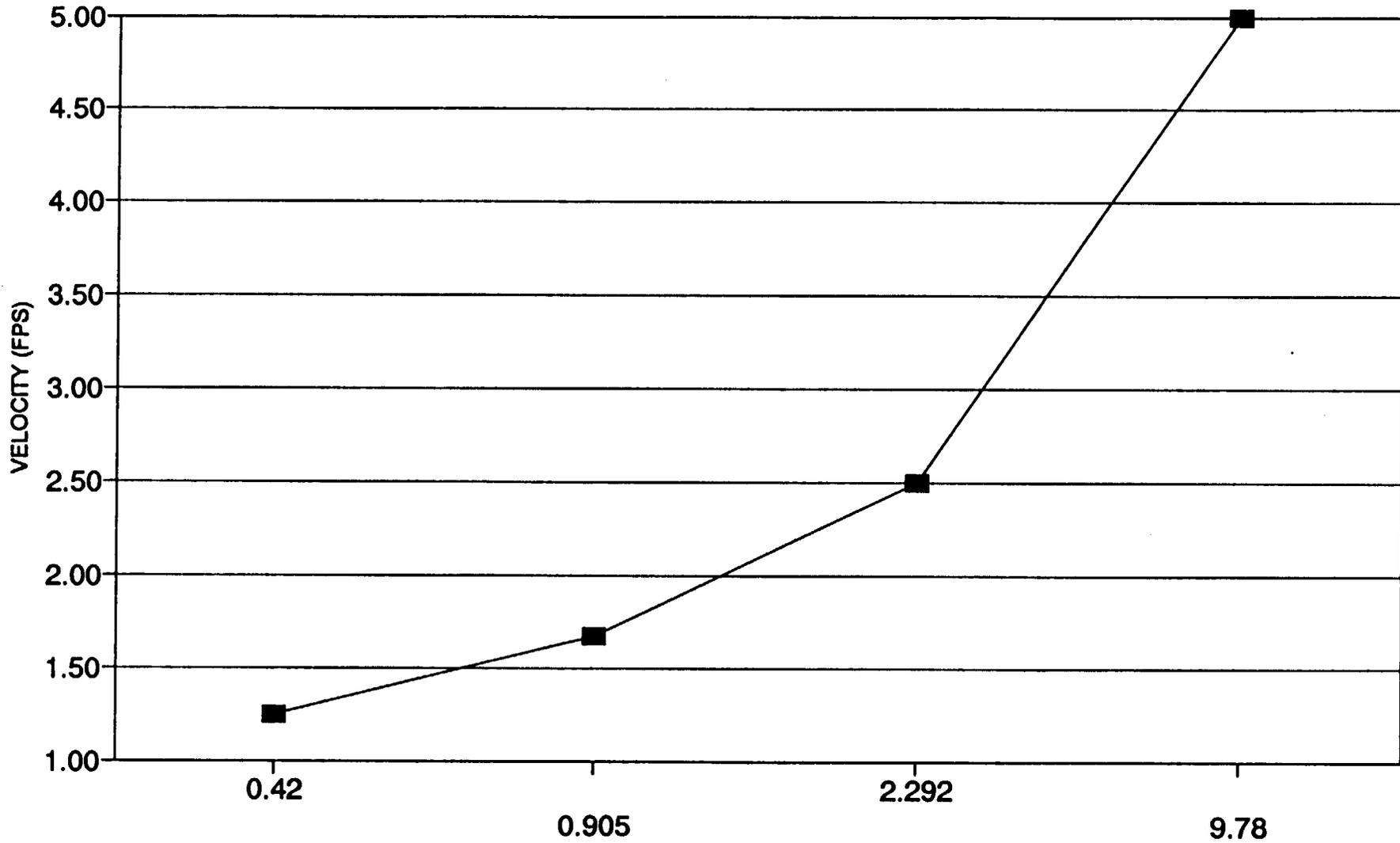
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28.5' WATER DEPTH

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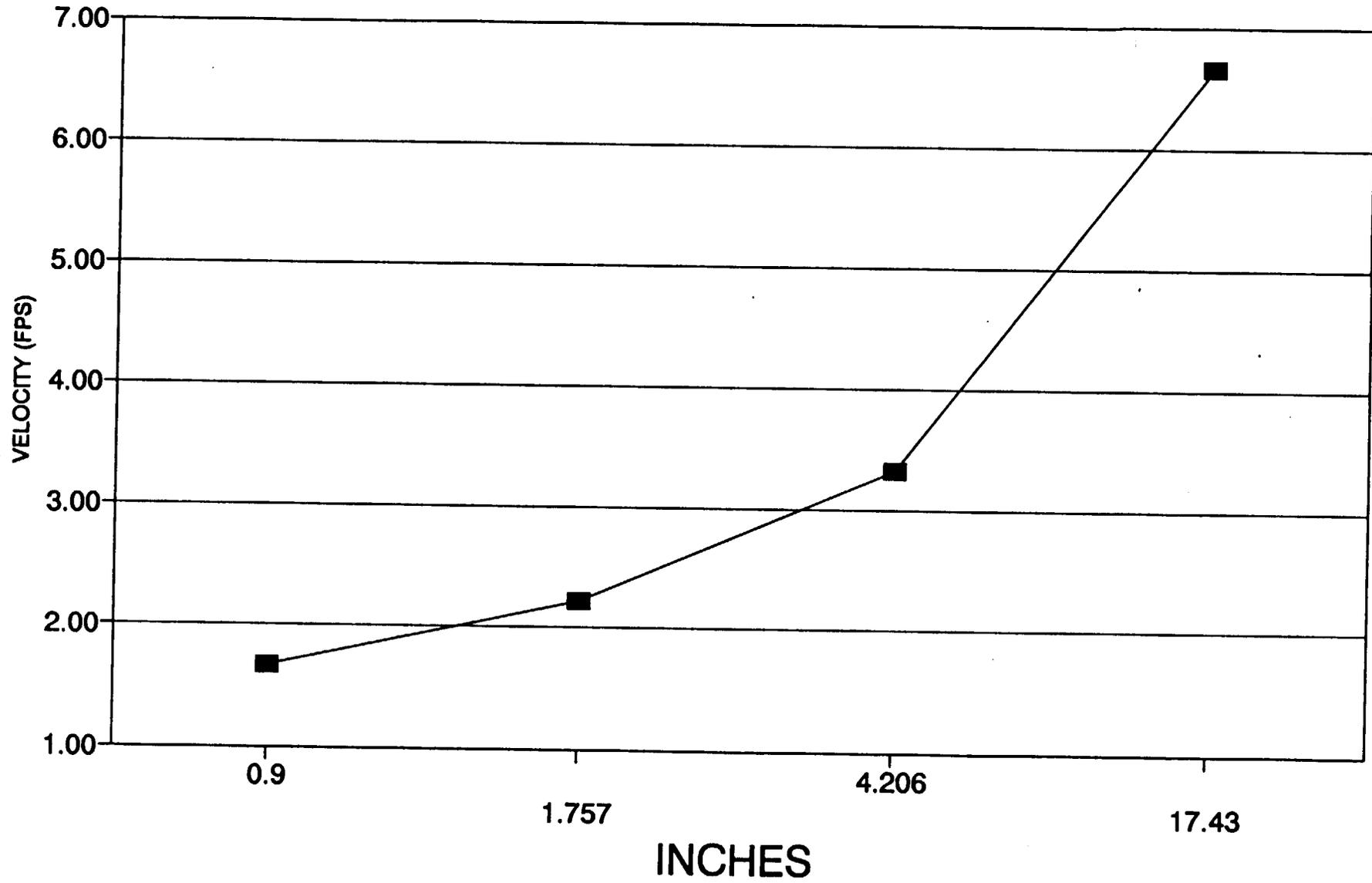
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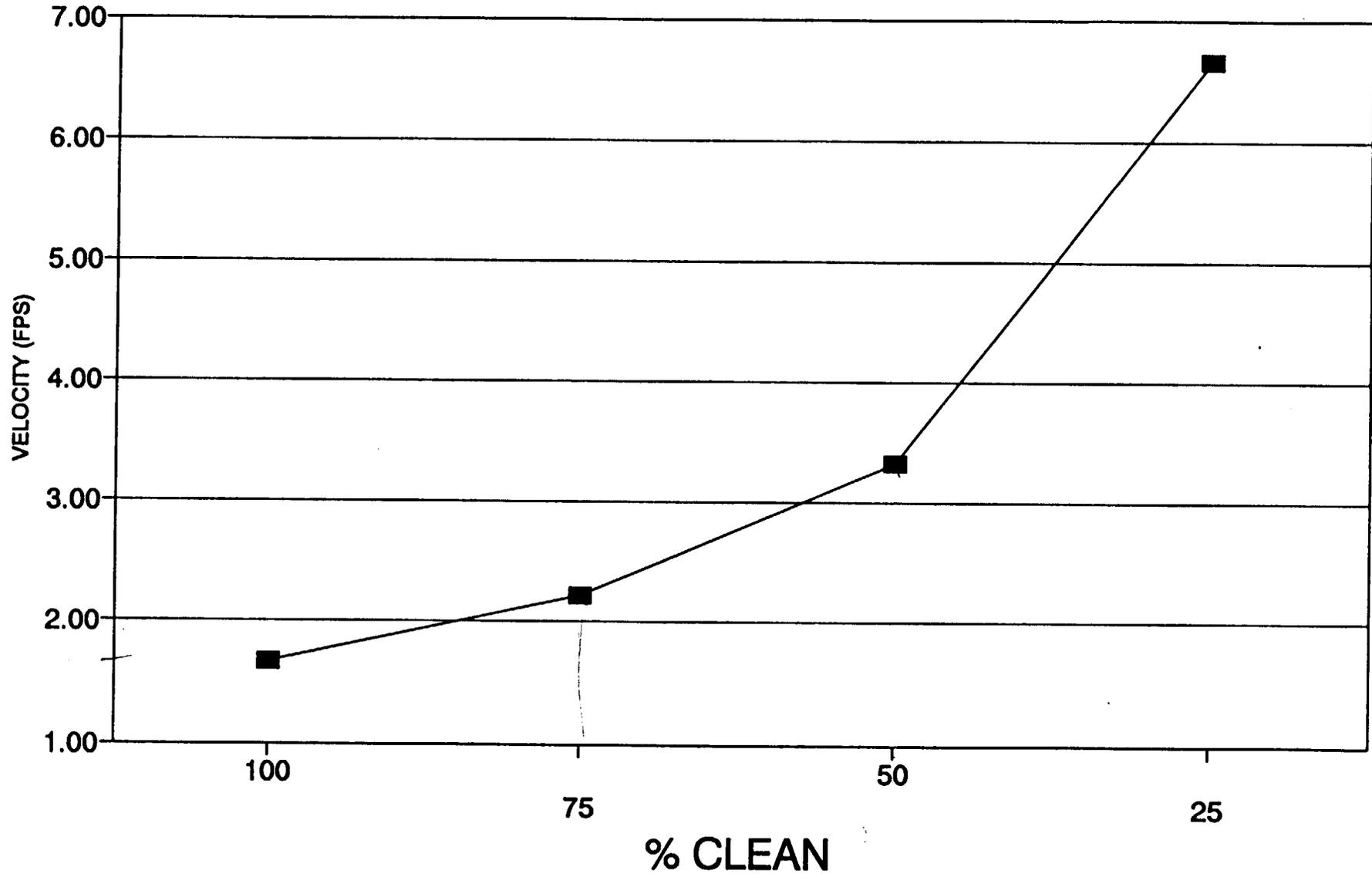
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28.5' WATER DEPTH

—■— 2MM MESH

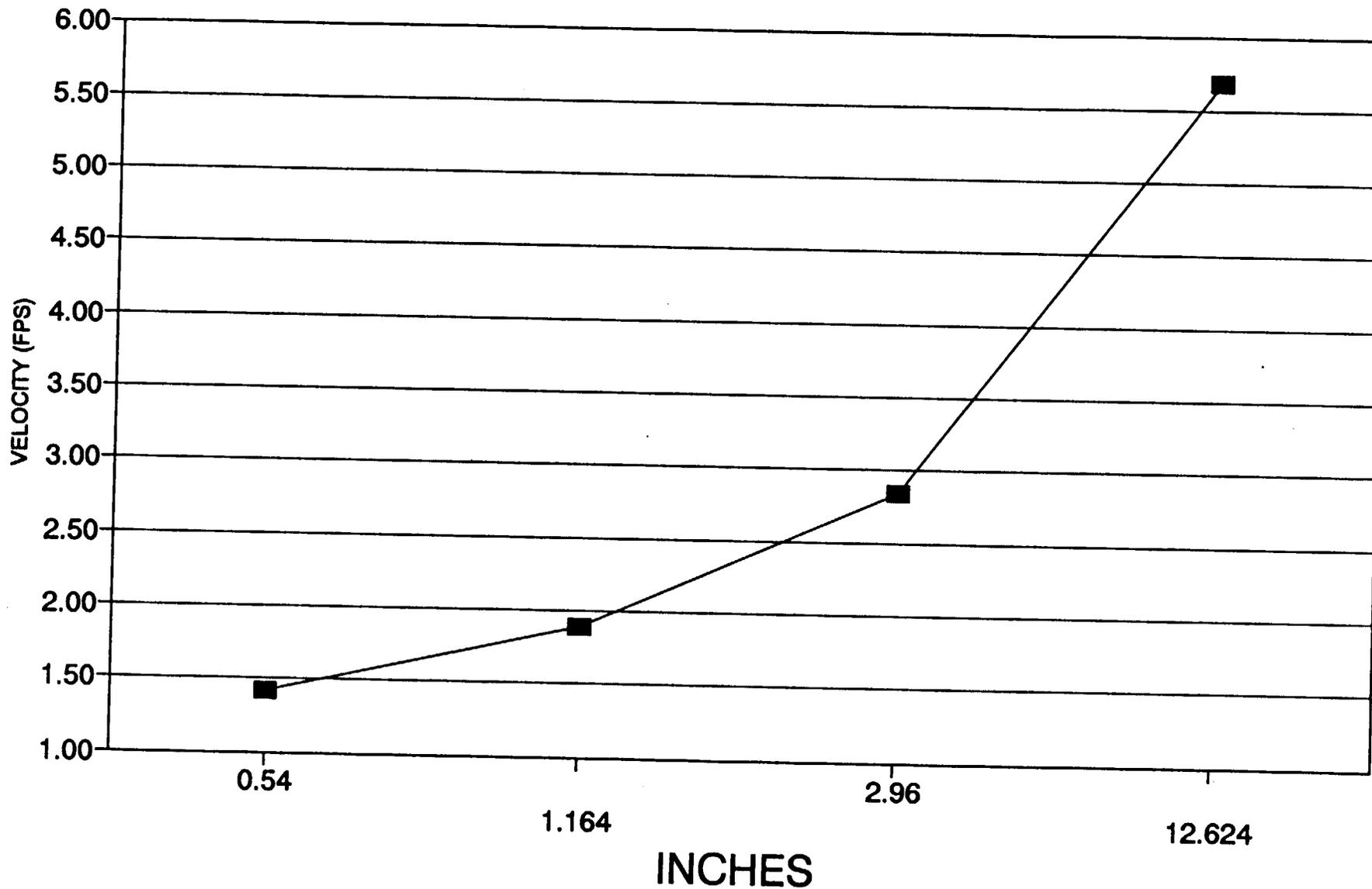
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—■— 2MM MESH

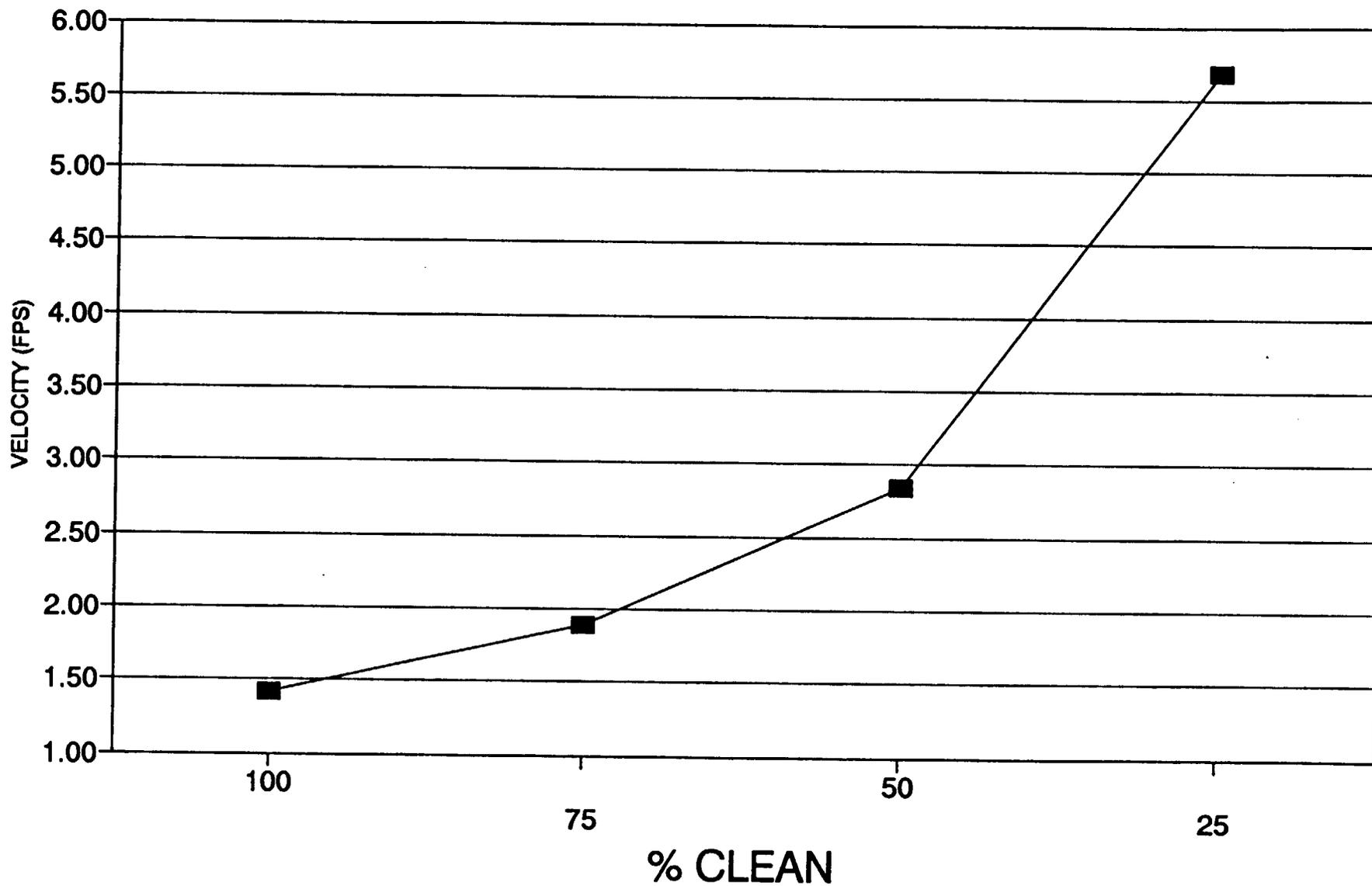
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26' WATER DEPTH

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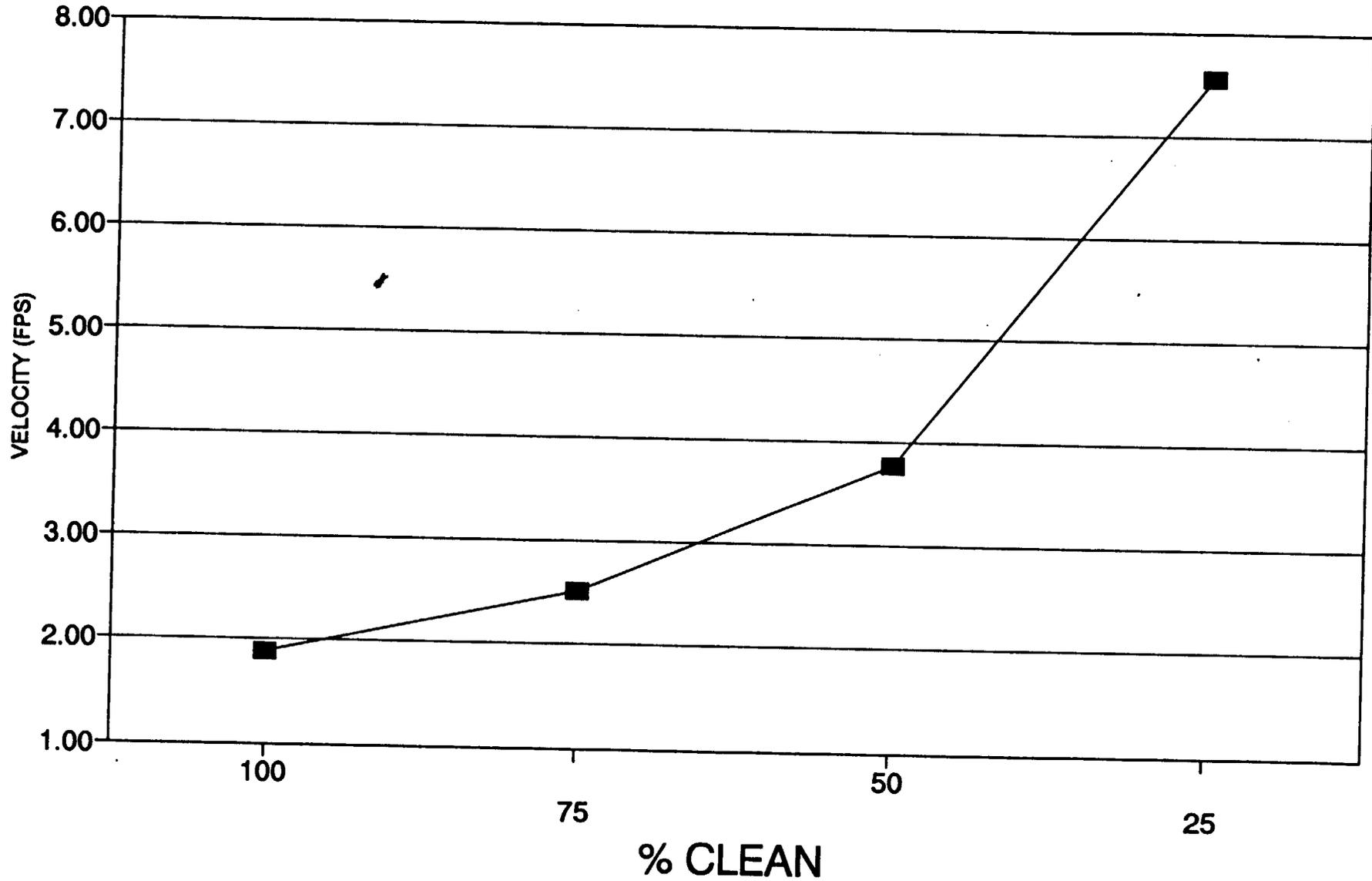
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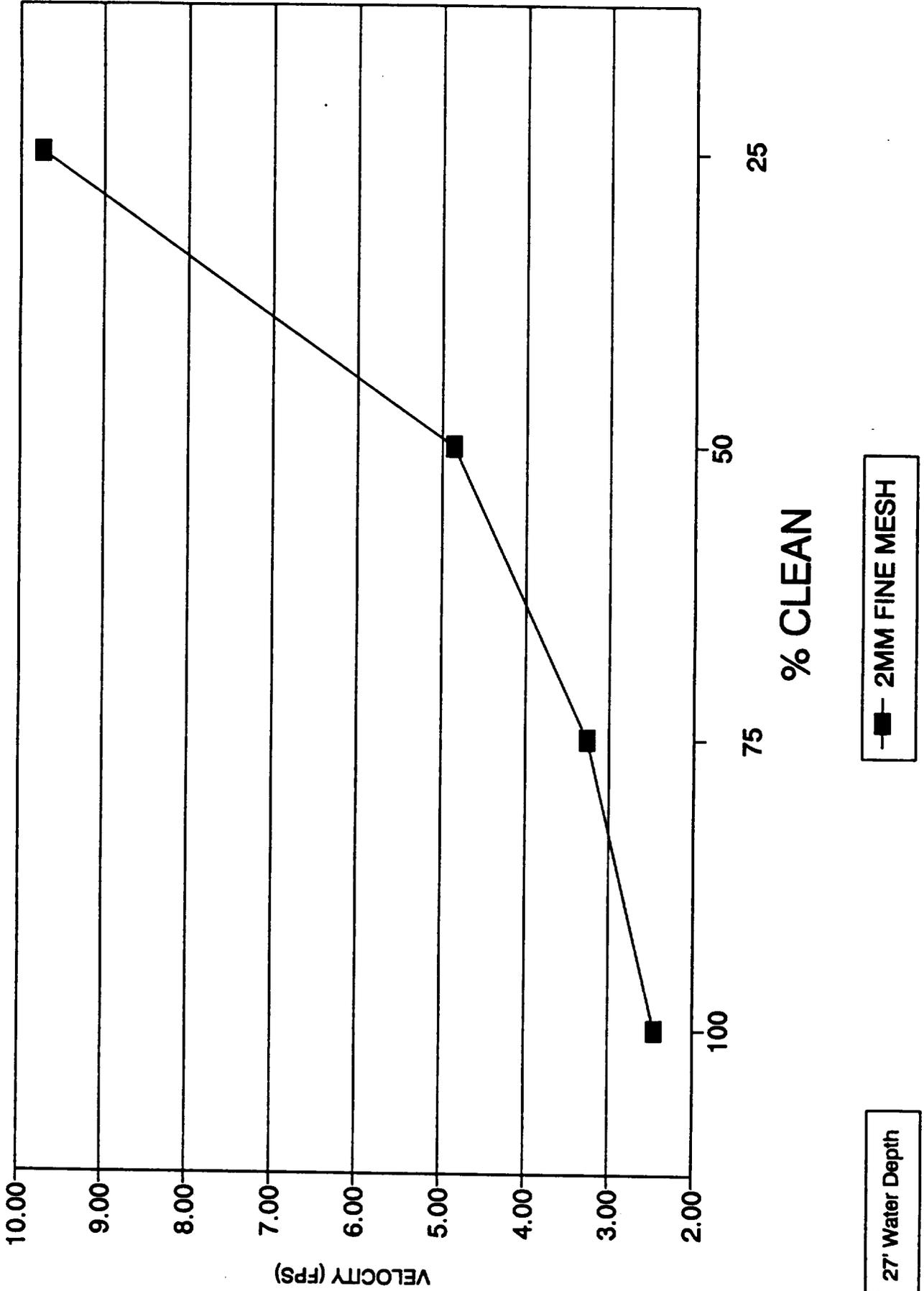
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26' WATER DEPTH

—■— 2MM MESH

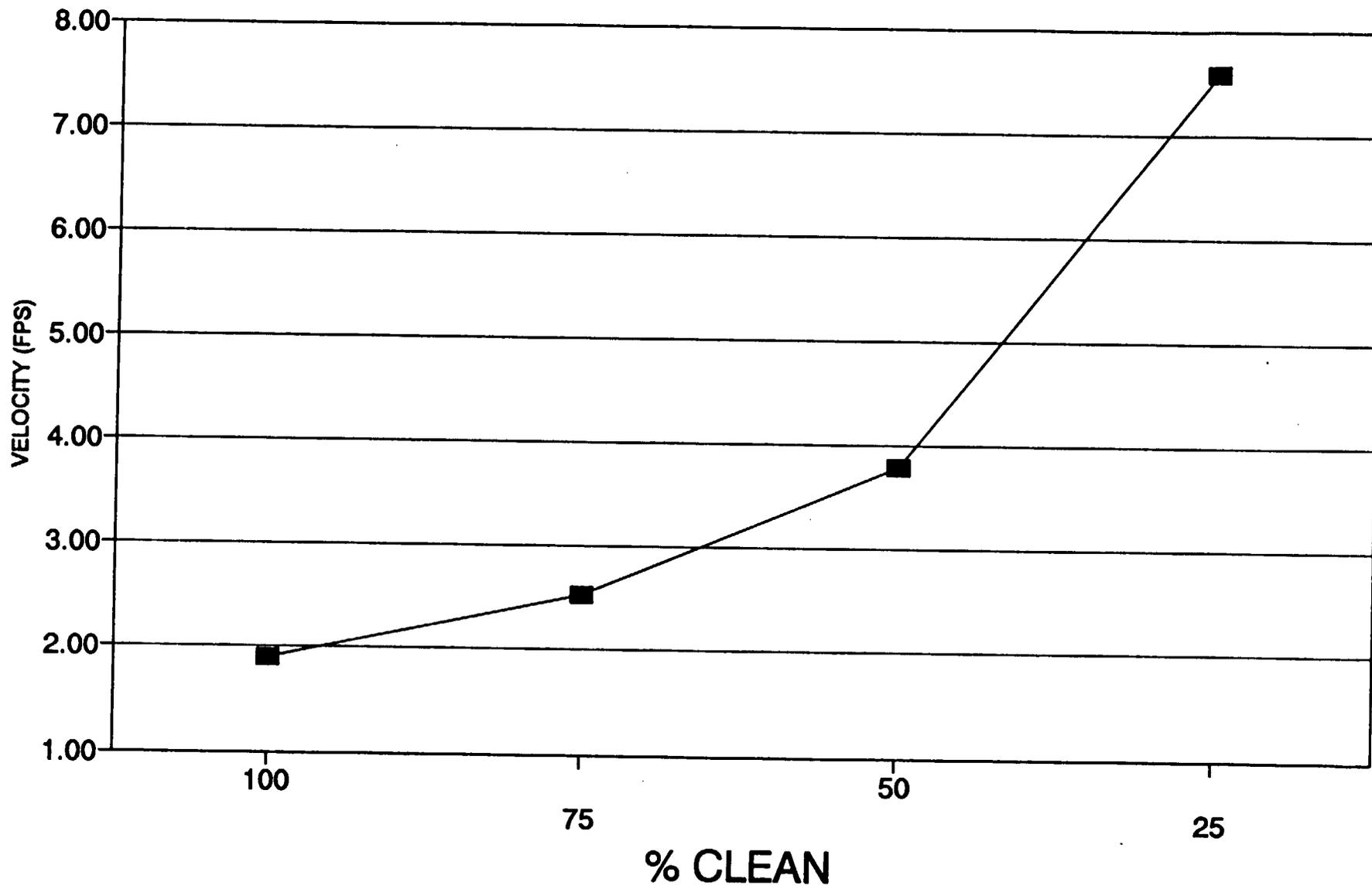
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NYPA IP NO. 3



27' Water Depth

■ 2MM FINE MESH

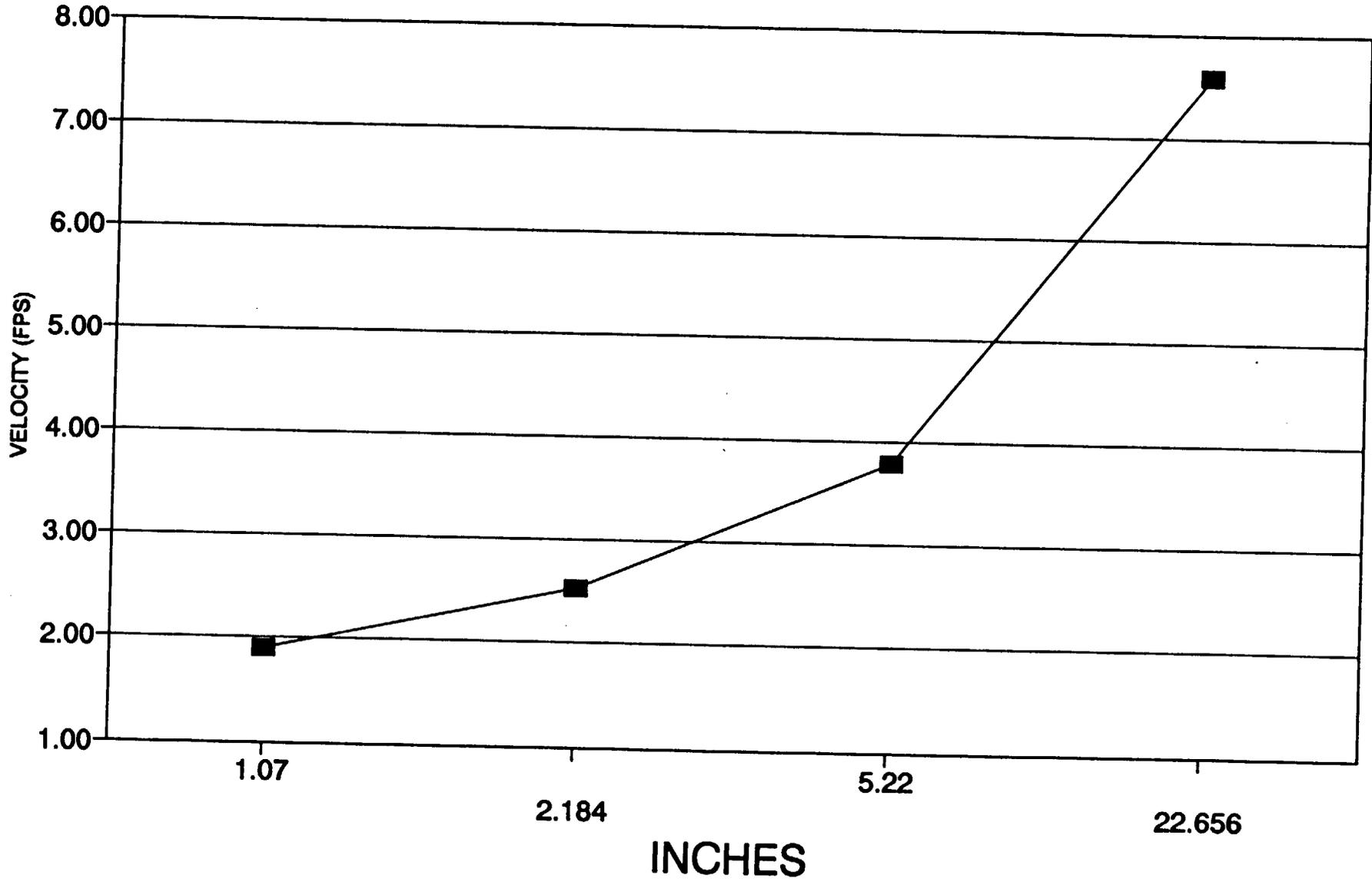
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27' Water Depth

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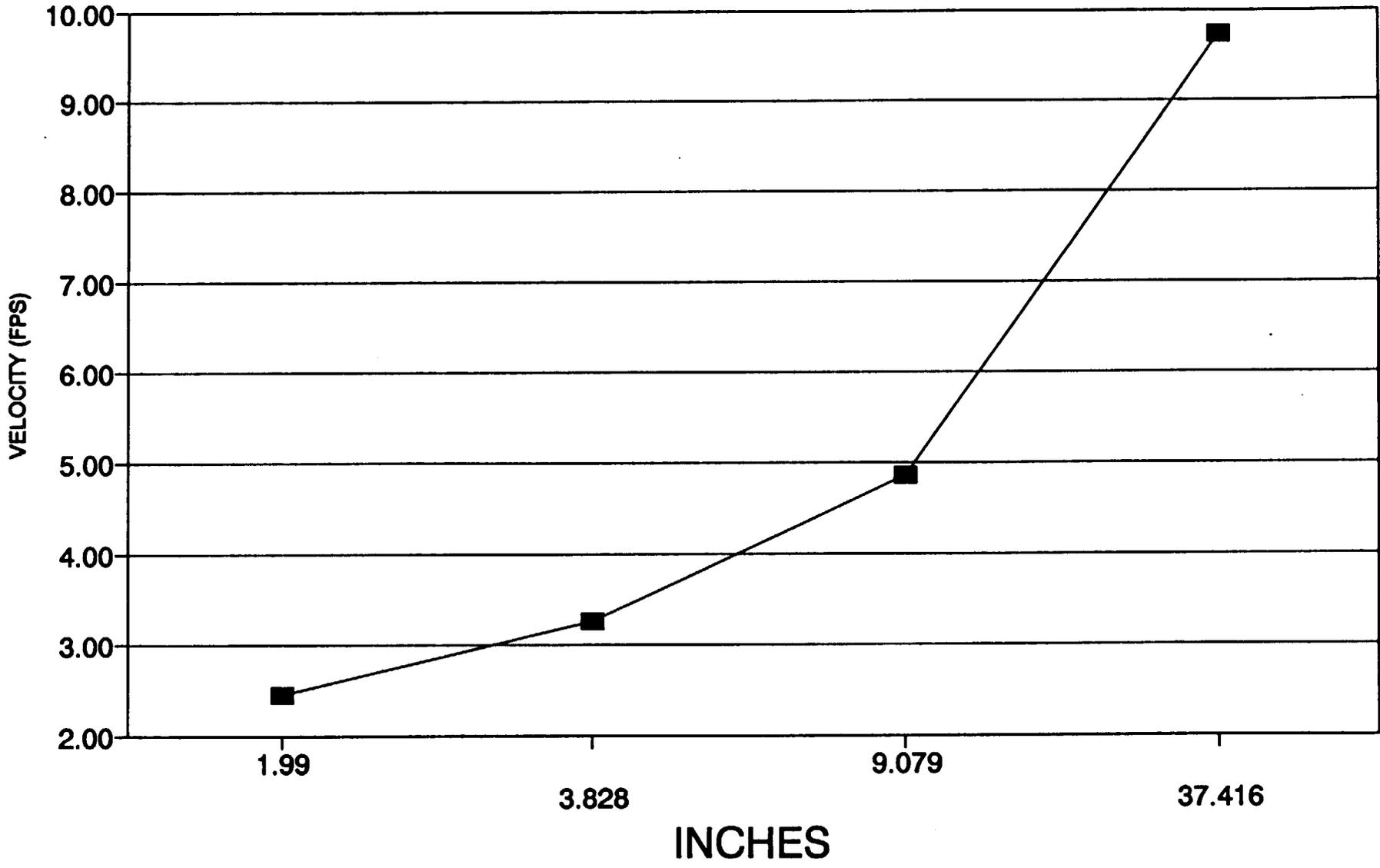
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27' Water Depth

—■— 1/4"X1/2"SMOOTH TEX

CON ED IP NO. 2  
NYPA IP NO. 3



27' Water Depth

—■— 2MM FINE MESH

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**EVALUATION OF ENTRAINMENT ABUNDANCE SAMPLING DESIGNS**

Prepared by

George Casella, Ph.D.  
Douglas S. Robson, Ph.D.  
Steven J. Schwager, Ph.D.  
William D. Youngs, Ph.D.

Ithaca, New York 14850

July 1986

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**ADDENDUM**

to

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William D. Youngs, Ph.D.

Ithaca, New York 14850

February 1990

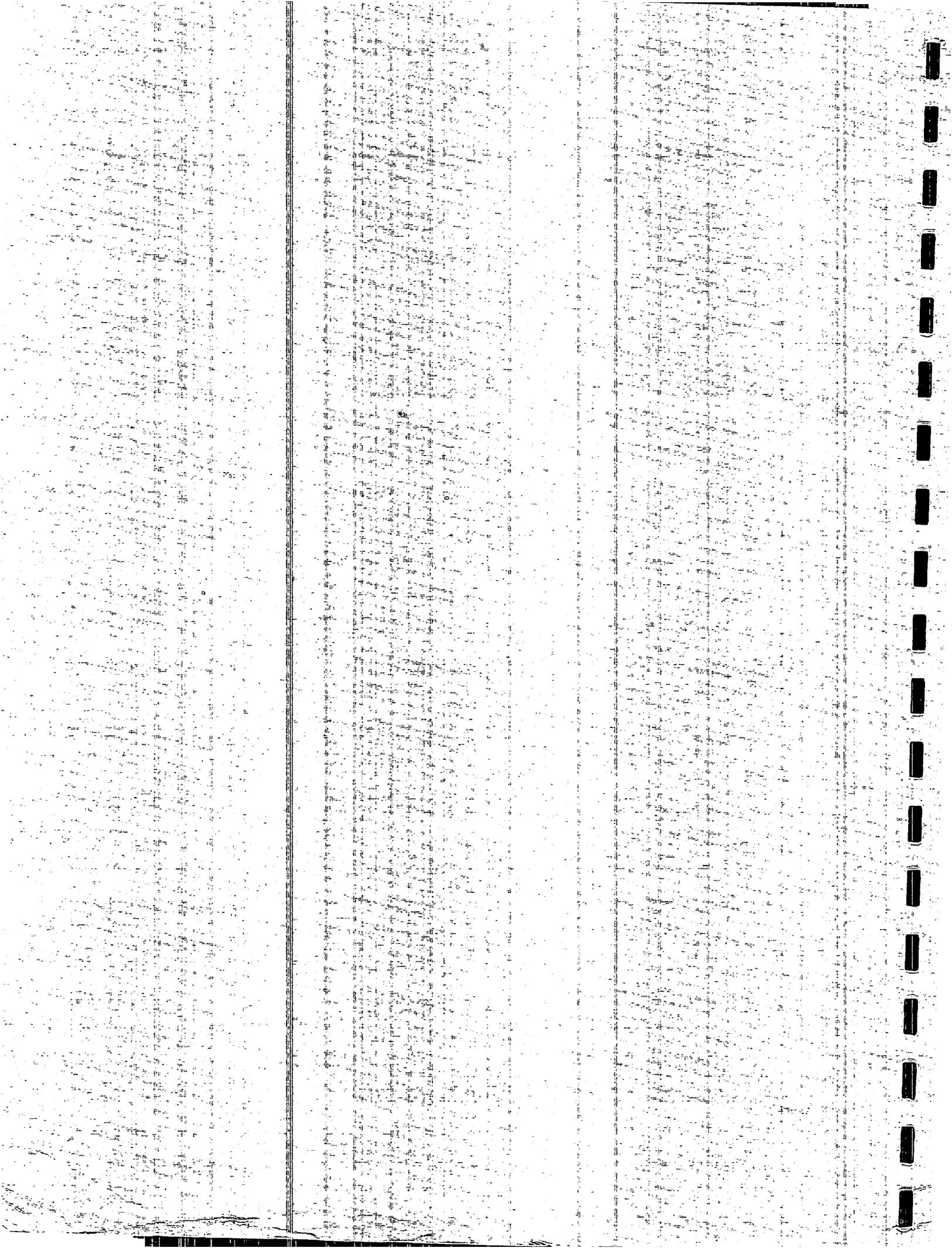
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Prepared Under Contract With

Consolidated Edison Company of New York, Inc.  
4 Irving Place  
New York, New York 10003

Jointly Funded By

Central Hudson Gas and Electric Corporation  
Consolidated Edison Company of New York, Inc.  
New York Power Authority  
Niagara Mohawk Power Corporation  
Orange and Rockland Utilities, Inc.



655

**EVALUATION OF ENTRAINMENT ABUNDANCE SAMPLING DESIGNS**

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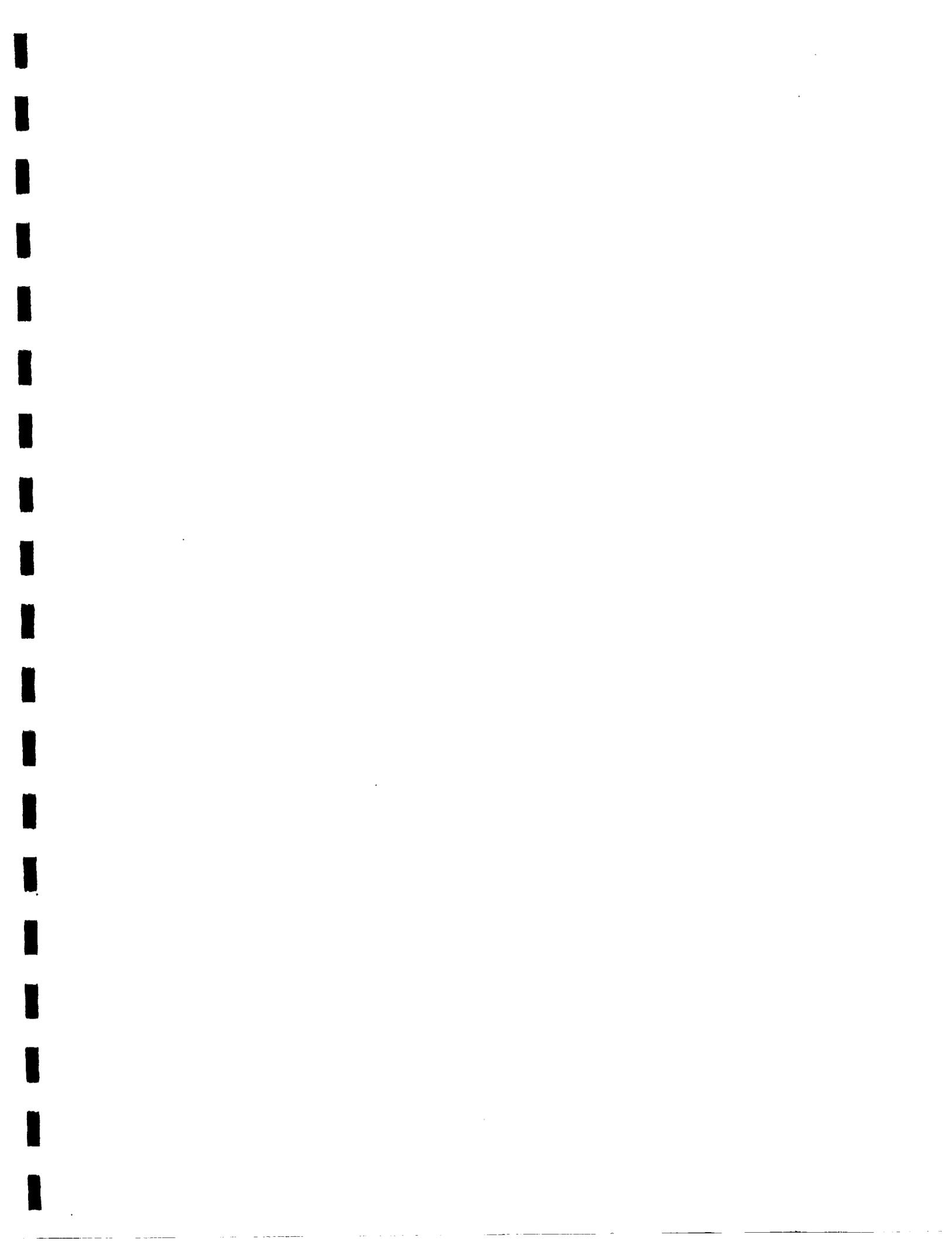
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July 1986

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## 1. STUDY OVERVIEW AND BACKGROUND

### 1.1 STUDY OVERVIEW

The Indian Point, Roseton, and Bowline Point Generating Stations employ once-through cooling systems to dissipate waste heat resulting from the process of electric generation. Cooling water from the Hudson River is pumped through condensers where heat is transferred to the water from the plant's exhaust steam. The warmed circulating water is then returned to the river. This cooling water may contain aquatic organisms small enough to pass through the intake screens. Eggs, larvae, and juveniles of various taxa are entrained, resulting in some mortality. Concern for the young stages of fish species of recreational and commercial importance has resulted in regulatory requirements. A schedule of unit outages and flow reductions to lessen the impact of entrainment is directed by the Settlement Agreement of December, 1980 among the Hudson River Utilities, the Environmental Protection Agency, the New York State Department of Environmental Conservation, and other parties.

The purpose of this study is to determine how efficient various sampling designs are in evaluating the effectiveness of plant outages in reducing entrainment. Existing entrainment data from the Indian Point (Units 2 and 3), Bowline Point, and Roseton Generating Stations are analyzed to accomplish this task. The entrainment sampling programs seek to estimate as accurately as possible the numbers of ichthyoplankton entrained under various plant operating conditions, including those low and no-flow periods that qualify for outage credit. Total ichthyoplankton counts and counts of several specified taxa are analyzed, with primary emphasis on striped bass, bay anchovy, white perch, and American shad.

Estimates of the number of organisms entrained and the reduction in the number entrained due to outages are developed. The reduction in entrainment attributable to the outages is the difference between the number of ichthyoplankton entrained during the outage periods and the number that would have been entrained during these periods under more normal operating conditions. Thus estimating these entrainment abundance levels provides a method of estimating the reduction in entrainment.

A common approach to sampling design, laboratory procedures, and data analysis is employed for all three generating stations as far as possible, in order to facilitate drawing overall conclusions. The work conducted in the course of this study can be divided into the following broad tasks.

The first task is to specify the class of sampling designs under consideration and their attributes. This class of designs will include those designs presently in use. Sampling models to be compared include various two-stage and three-stage sampling designs. In two-stage sampling, the total volume of water passing through a plant is divided

## 1.2

into primary sampling units (p.s.u.'s) and samples of these p.s.u.'s are drawn for laboratory analysis. In three-stage sampling, the samples are divided further by drawing splits for analysis. In either two-stage or three-stage sampling, the p.s.u.'s selected for sampling constitute a sample from the overall population of interest, the total volume of water passing through the plant; all estimates and other inferences will be directed toward this overall population.

The accuracy of estimates of the ichthyoplankton counts in the total volume of water (not just the sample volume drawn for analysis) will be affected greatly by both the type of sampling (two-stage or three-stage) and the design parameters. Two designs of particular interest are the sampling procedures used at the plants during recent years. In the first design, a p.s.u. corresponds to a three-hour period, and sampling is continuous, i.e., sampling occurs over 8 p.s.u.'s per day, 7 days per week. In the second design, a p.s.u. corresponds to a one-hour period, and a full day of 24 p.s.u.'s is sampled on days selected to focus on entrainment abundance sampling during the outage period.

The second task is to specify formal criteria by which the performance of sampling designs will be measured. The main objective of the study is estimation of the entrainment abundance level and the effect of outages in reducing entrainment. Thus, standard statistical properties of estimators can be used in making comparisons among the estimates resulting from different sampling designs. A desirable sampling design will be associated with a point estimate of entrainment reduction characterized by little or no bias and low variance compared to other estimates, under widely varying conditions. The ability to deal with relatively rapid changes in the level of entrainment over a short period of time is an important feature of a good sampling design. Because the behavior of sampling designs must be investigated under a broad range of underlying river conditions, the choice of criteria must reflect this range.

The third task is to assess the distributional information available from the data collected at the three generating stations over the last few years. This information provides guidance concerning the type of conditions likely to be observed in future years. One aspect of this task is the development of a model relating the entrainment of early life stages to plant operating factors, especially decreased flow and outages. This leads to a unified approach to the estimation of entrainment abundance under full flow and under decreased flow, all other factors being equal. The difference between these estimates of normal flow entrainment and decreased flow entrainment provides an estimate of the reduction in entrainment due to decreased flow.

Another aspect of this task is the determination of whether distributional assumptions that have been proposed in previous entrainment studies are consistent with recent data from the three generating stations. One commonly proposed assumption is that sample or split ichthyoplankton counts within each p.s.u. may be independent, identically distributed Poisson random variables. This report contains evidence that the Poisson model does not provide an adequate representation of the entrainment

### 1.3

process. The Poisson model often leads to systematic underestimation of standard deviations and coefficients of variation, making entrainment abundance estimates seem more precise than they actually are. The investigation of the distribution of ichthyoplankton counts leads to more effective sampling designs and estimation procedures. These sampling designs include replicate observations, or counts, taken during several p.s.u.'s. Information from these designs may be incorporated into the analysis of data from years in which no replicate sampling was performed, such as the years prior to 1982. The result will be more accurate estimation for those years.

The fourth task is to analyze the efficiency of estimation procedures under the sampling designs described above, using the criteria chosen to compare designs. This evaluation hinges on the estimation of entrainment reduction due to outages. Comparisons between the two sampling designs used recently and competing designs are of special interest.

The fifth and final task is to recommend sampling designs for future entrainment abundance sampling at the three generating stations. The efficiency of the two sampling designs used in recent years, relative to other designs that could be employed, is judged by comparing the variance of the estimate of entrainment reduction due to outages, which differs among these designs.

## 1.2 PLANT DESCRIPTION

Figure 1.1 depicts the geographic location of the Bowline Point, Indian Point, and Roseton power plants. The following general description of the physical system at each of these plants has been taken verbatim from the 1983 Annual Reports for Bowline Point, Indian Point, and Roseton (EA, 1984a, 1984b, 1984c).

### 1.2.1 BOWLINE POINT

The Bowline Point plant consists of two oil- or gas-fired steam-electric units, each having a net generating capability rating of 600 MWe and a gross capability of 622 MWe. Unit 1 began commercial operation in September 1972 and Unit 2 in May 1974.

Each unit has a separate once-through cooling water system. The cooling water is pumped from an intake structure located on Bowline Pond (Figure 1.2). Each of the three intake bays for each unit is approximately 5-m (16 ft.) wide and equipped with a bar trash rack, a 9.5-mm (0.374 in.) mesh vertical traveling screen, and a 700-m<sup>3</sup>/min (185,000-gpm) circulating water pump. The circulating water pumps for each unit

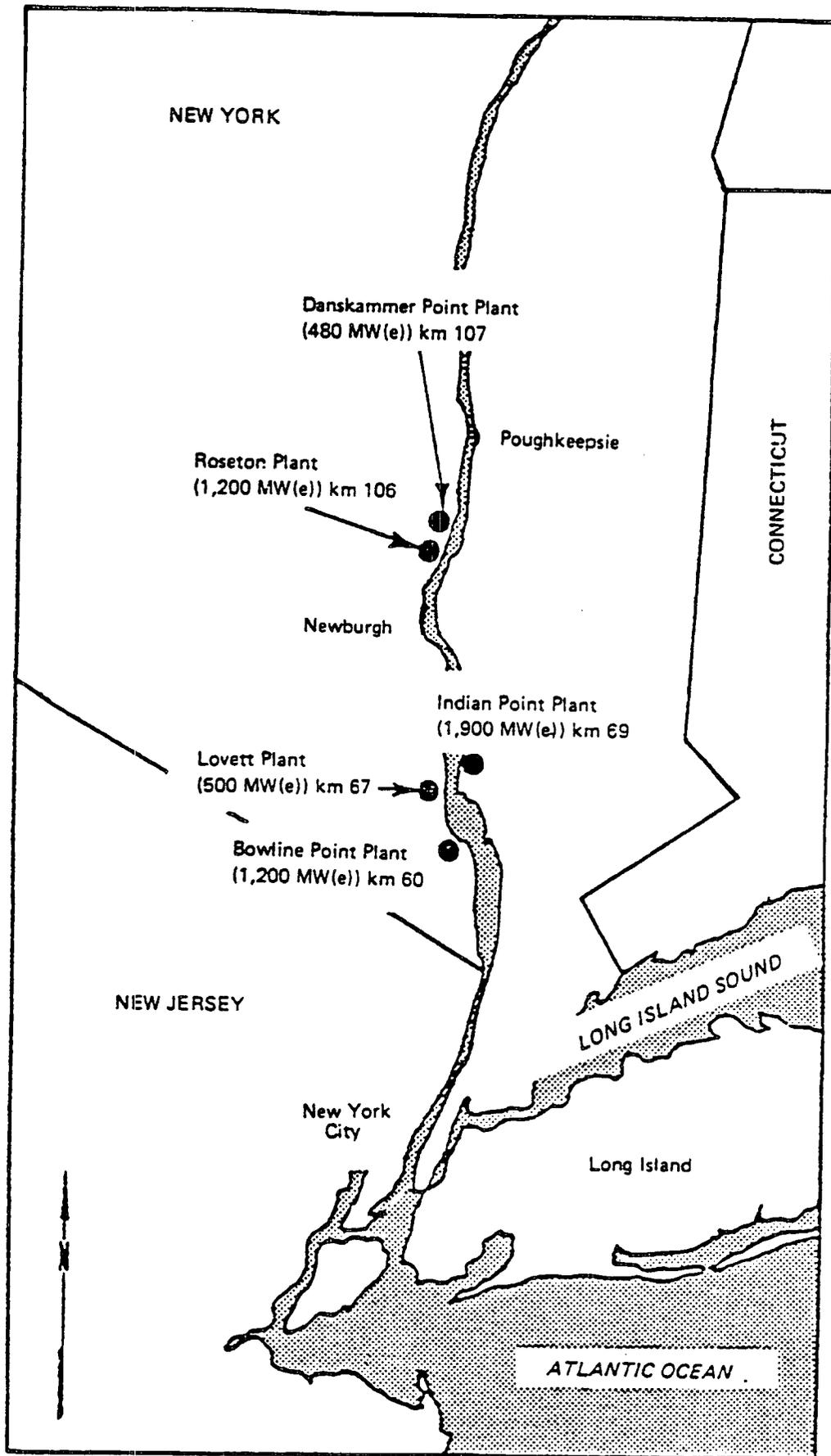


Figure 1.1. Location of the Bowline Point, Indian Point, and Roseton Generating Stations on the Hudson River. (Reproduced from EA, 1984a)

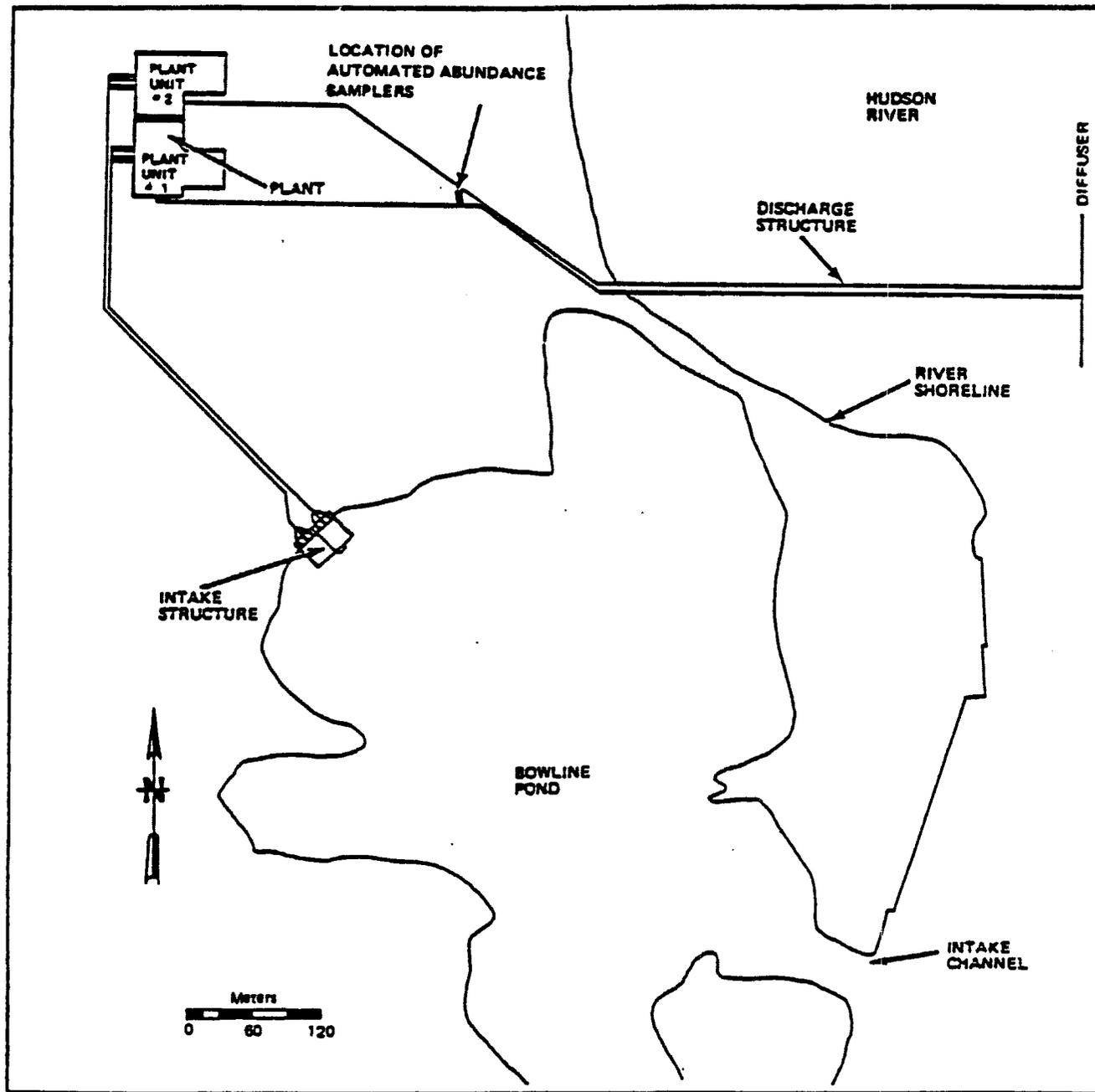


Figure 1.2. Diagram of Bowline Point plant site.  
(Reproduced from EA, 1984a)

can be operated individually or in combination. Circulating water flow and approach velocities at the bar racks vary with pumping mode:

<u>Number of Pumps Operating</u>	<u>Total Flow at Mean Water Elevation m<sup>3</sup>/sec (gpm)</u>	<u>Intake Approach Velocity m/sec (fps)</u>
3	24.2 (384,000)	0.23 (0.77)
2	20.0 (316,000)	0.18 (0.59)
2 (throttled)	16.2 (257,000)	0.15 (0.49)

During the 1983 sampling season, the Bowline Point plant operated in the two-pump throttled mode ( $140 \times 10^4 \text{ m}^3/\text{day}$ ) at each operating unit through late May and operated with two pumps full flow ( $172 \times 10^4 \text{ m}^3/\text{day}$ ) for the duration of the sampling period.

The circulating water is pumped through the condensers where the excess heat of the system is transferred to the cooling water. The maximum condenser cooling water flow is  $24.2 \text{ m}^3/\text{sec}$  (384,000 gpm). The circulating water is returned to the Hudson River about 400 m from the river shoreline where dispersion of the heated waters is effected by discharge perpendicular to the river flow through submerged, multiport, high velocity diffusers.

### 1.2.2 INDIAN POINT

The Indian Point Generating Station consists of three nuclear-fueled electric generating units. Unit 1, owned by Con Edison, has not been operated for commercial production since October 1974, although its circulating water and service water pumps are operated occasionally. Unit 2, owned and operated by Con Edison, has been in operation since 28 September 1973 and has a net rated capacity of 873 MWe. Unit 3, owned and operated by the New York Power Authority, has been in operation since 30 August 1976 and has a net rated capacity of 965 MWe. All three units use Hudson River water for once-through cooling.

Each unit has a separate shoreline intake structure for the withdrawal of water from the Hudson River (Figure 1.3). The intake structure for Unit 1 has four rectangular openings that extend 8.0 m (26 feet) below mean low water. The intake structures for Units 2 and 3 each have six intake openings extending 8.2 m (27 feet) below mean low water. The intakes to Units 1 and 2 are equipped with fixed screens at the entrance to the intake bays and vertical traveling screens located behind the fixed screens, whereas the intake to Unit 3 has only vertical traveling screens at the entrance to the intake bays. All screens are 9.5-mm (0.375-in.) square mesh.

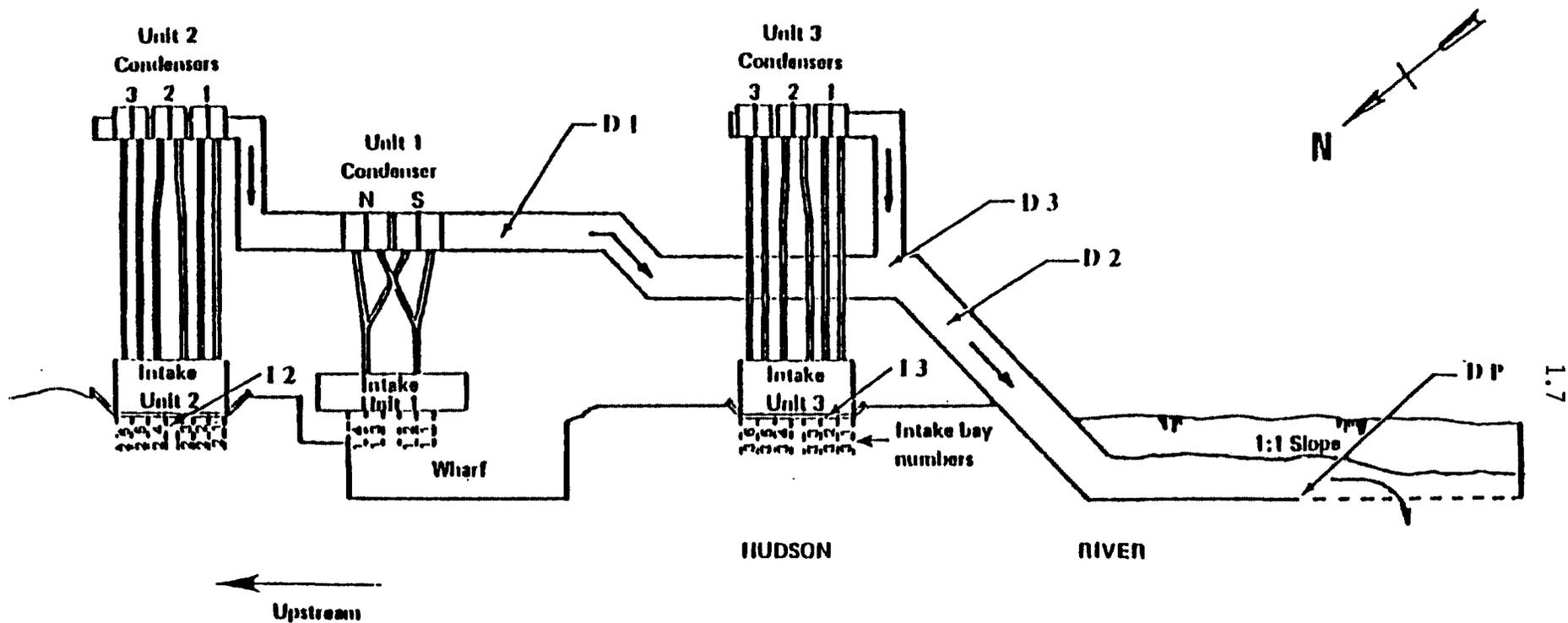


Figure 1.3. Diagram of the Indian Point Generating Station circulating water system.  
 (Reproduced from EA, 1984b)

Circulating water pumps with rated capacities of 530 m<sup>3</sup> per minute (140,000 gpm) are used to pump Hudson River water through the condenser cooling system of each unit. Unit 1 has two circulating water pumps with a total rated capacity of 1,060 m<sup>3</sup> per minute (280,000 gpm), and two service water pumps with a combined rated capacity of 144 m<sup>3</sup> per minute (38,000 gpm). Units 2 and 3 each have six circulating water pumps that withdraw water from separate intake bays (Figure 1.3). The circulating water systems for Units 2 and 3 are designed to operate at either 100 or 60 percent flow. When the ambient water temperature is above 4.4 C (40 F) (spring through fall), the circulating pumps are operated so that the rated maximum cooling water flow for each unit is 3,200 m<sup>3</sup> per minute (840,000 gpm). During the winter, 40 percent of the cooling water is returned to the circulating pumps without passing through the condenser, reducing the rated maximum intake flow for each unit to 1,900 m<sup>3</sup> per minute (504,000 gpm). Service water for Units 2 and 3 is drawn through a separate intake forebay located at the center of each intake; the rated maximum total service water flow for Units 2 and 3 is 114 m<sup>3</sup> per minute (30,000 gpm).

The cooling water and service water from all three units flow into a common discharge channel. The combined discharge is returned to the Hudson River via an outfall structure located downstream of Unit 3. The outfall structure consists of 12 ports submerged 3.6 m below the surface (12 feet from center of port to water surface) at mean low water.

Calculated transit times of cooling water traveling from intake to river outfall for Units 2 and 3, when operating at full pumping capacity, are 9.7 minutes for Unit 2 and 5.6 minutes for Unit 3. The calculated transit time from the intake to the condensers is about 1.5 minutes, and the calculated transit times through the condensers are 0.14 minutes for both units. Thus, much of the total transit time through the cooling water systems of Unit 2 and Unit 3 occurs in the discharge canal. Because the discharge canal receives cooling water from all three units, transit times through the canal are dependent upon the total circulating water flow through all units combined.

Estimated velocities of the cooling water vary as a function of cross-sectional area at different locations within the Indian Point plant condenser cooling water system. Estimated flow velocities are lowest at the intakes, ranging from 0.57 ft/sec at Unit 1 (when circulating water pumps are operating) to 0.84 ft/sec at the intakes of Units 2 and 3. Estimated water velocities in the common discharge canal are consistently higher than velocities at the intakes, and vary according to the number of circulating pumps operating at each unit. At the point of sampling in the common Unit 1 and Unit 2 discharge canal (Figure 1.3), estimated maximum average velocity would be 3.85 ft/sec with all pumps operating at 100 percent at both units and 2.82 ft/sec with only Unit 2 pumps operational. At the sampling station in the canal serving all three units, maximum average velocity would be 5.14 ft/sec with all three units operating, 4.35 ft/sec with Units 2 and 3 operating, and 2.17 ft/sec

with only Unit 2 or Unit 3 operating. At the discharge structure, however, gates to the submerged ports are opened or closed to maintain an estimated velocity of 10.0 ft/sec through the ports, regardless of the number of units operating.

The temperature rise ( $\Delta T$ ) encountered by organisms passing through the condenser cooling systems of the Indian Point plant depends on the cooling water flow rates and levels of power output (Con Edison 1977, Tables 1-13 and 1-14). At Unit 2, with six pumps operating at full flow and the unit at 100 percent capacity, the predicted condenser temperature rise ranges from 8.8 to 8.9 C (15.8 to 16.1 F), depending upon river temperature. During full capacity winter operation, with Unit 2 circulating pumps operating at 60 percent flow capacity (i.e., 40 percent recirculation), the predicted condenser temperature rise is approximately 14.7 C (26.5 F). At Unit 3, the predicted condenser temperature rise ranges from 9.5 to 9.7 C (17.1 to 17.4 F) for 100 percent capacity with six pumps operating at full flow; during winter operation, the predicted temperature rise is approximately 16.1 C (29 F). The higher predicted condenser temperature rise for Unit 3, compared to that for Unit 2, is due to the fact that Unit 3 has a higher generating capacity than Unit 2, but uses the same volume of cooling water.

### 1.2.3 ROSETON

The Roseton plant consists of two 600-MWe oil-fired units. Unit 2 began commercial operation on 14 September 1974, and Unit 1 on 31 December 1974. The two units operate independently but share a common intake and discharge cooling system.

Water for the once-through cooling system is withdrawn from the Hudson River at a shoreline intake structure (Figure 1.4). The intake contains four cooling-water pumps and eight 9.5-mm mesh vertical traveling screens. After passing through the condensers, cooling water is discharged into the river through a submerged jet diffuser. The area immediately in front of the intake and discharge structure has been dredged to a depth of 8.2 meters for a distance of 61 meters into the river (CHG&E 1978).

Various modes of pump operation are possible at the Roseton plant, depending on the number of units in operation. During 1983, two cooling-water pumps were usually in operation from January through June and from mid-September through December. During this mode of operation, peak cooling-water flow was approximately 26.4 m<sup>3</sup>/sec. Cooling-water pump operation was increased to three pumps in July, August, and early September with peak pumping rates of 35.4 m<sup>3</sup>/sec.

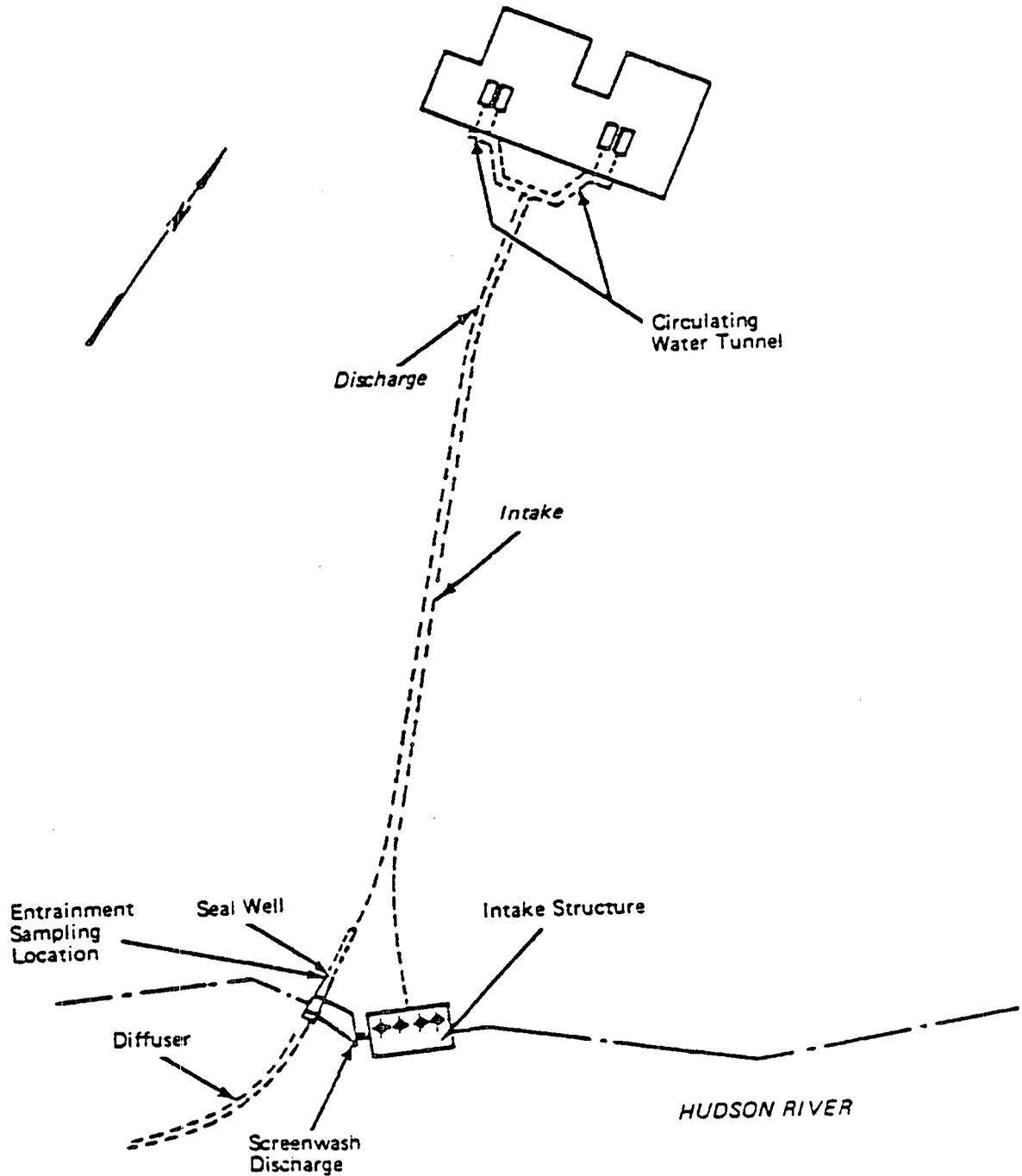


Figure 1.4. General overview of the Roseton Generating Station.  
(Reproduced from EA, 1984c)

### 1.3 FIELD COLLECTION PROCEDURES

The following general description of field sampling procedures has been taken from the 1983 Annual Reports for Bowline Point, Indian Point, and Roseton (EA, 1984a, 1984b, 1984c). Many passages have been quoted verbatim from these sources.

Ichthyoplankton samples were collected at Bowline Point Generating Station from 1 May through 13 August 1983, at Indian Point Generating Station from 3 May through 13 August 1983, and at Roseton Generating Station from May through July 1983. Sampling was scheduled to occur continuously throughout the period at Bowline and Indian Point. Some gear malfunctions resulted in some variation from the schedule at these two stations. Each 24-hour continuous collection consisted of eight discrete 3-hour samples. Sampling at Roseton was conducted weekly during early May, daily from 17 May through 27 May and 31 May through 22 June, and weekly during late June and July. Roseton ichthyoplankton samples were collected and analyzed as 24 distinct 1-hour intervals on each day of sampling. Locations of sampling points are shown in Figures 1.2, 1.3, and 1.4 for Bowline, Indian Point, and Roseton respectively.

Samples at Bowline and Indian Point were collected using the AUTOSAM sampler (Figure 1.5). The basic components of the sampler include a 7.6-cm electric pump, a cylindrical collection tank (1 m in diameter and 1.2 m in height) containing a cylindrical 500- $\mu$ m mesh plankton net, and a microcomputer control module. All components are housed in an enclosed trailer. The intake hose had a diameter of 10.1 cm. At Bowline Point, samples were collected from the plant cooling water discharge pipe; the intake hose was inserted into the discharge standpipe to a depth of about 1 m into the flow of water. At Indian Point, samples were collected from the discharge canal at Station D2; the intake hose inlet consisted of a 90 degree elbow fitting positioned at middepth (approximately 3 m below the water surface of the canal) and oriented into the discharge water flow. The 10.1-cm diameter hose was reduced to connect to the 7.6-cm diameter sampling pump. The pumping rate ranged from approximately 0.7 to 1 m<sup>3</sup>/min; however, the rate for most samples was 0.8 to 0.9 m<sup>3</sup>/min. Variation in rate occurred due to tidally induced changes in water level in the discharge canal. Average cross-sectional velocity at the mouth of the intake hose was approximately 2 m/sec.

Operational sequences of the AUTOSAM are controlled by the microcomputer module. During sampling, water is pumped into the net in the collection tank where primary concentration of the sampled organisms and detritus occurs. Filtered water passes out of the collection tank through a discharge drain pipe. Flow rate and volume are measured by an inline flowmeter mounted to the pipe that transports water from the AUTOSAM sampling pump to the collection tank. At the end of the programmed sampling interval, the following automated operations occur: (1) the pump shuts off and the collection tank drains; (2) the collection net is rinsed, concentrating the sample into the bottom of the collection net; (3) the sample is washed into the secondary concentrator and then into a collection container using chilled water (4.4 C) to reduce organism decomposition; and (4) formalin is automatically injected into the collection container

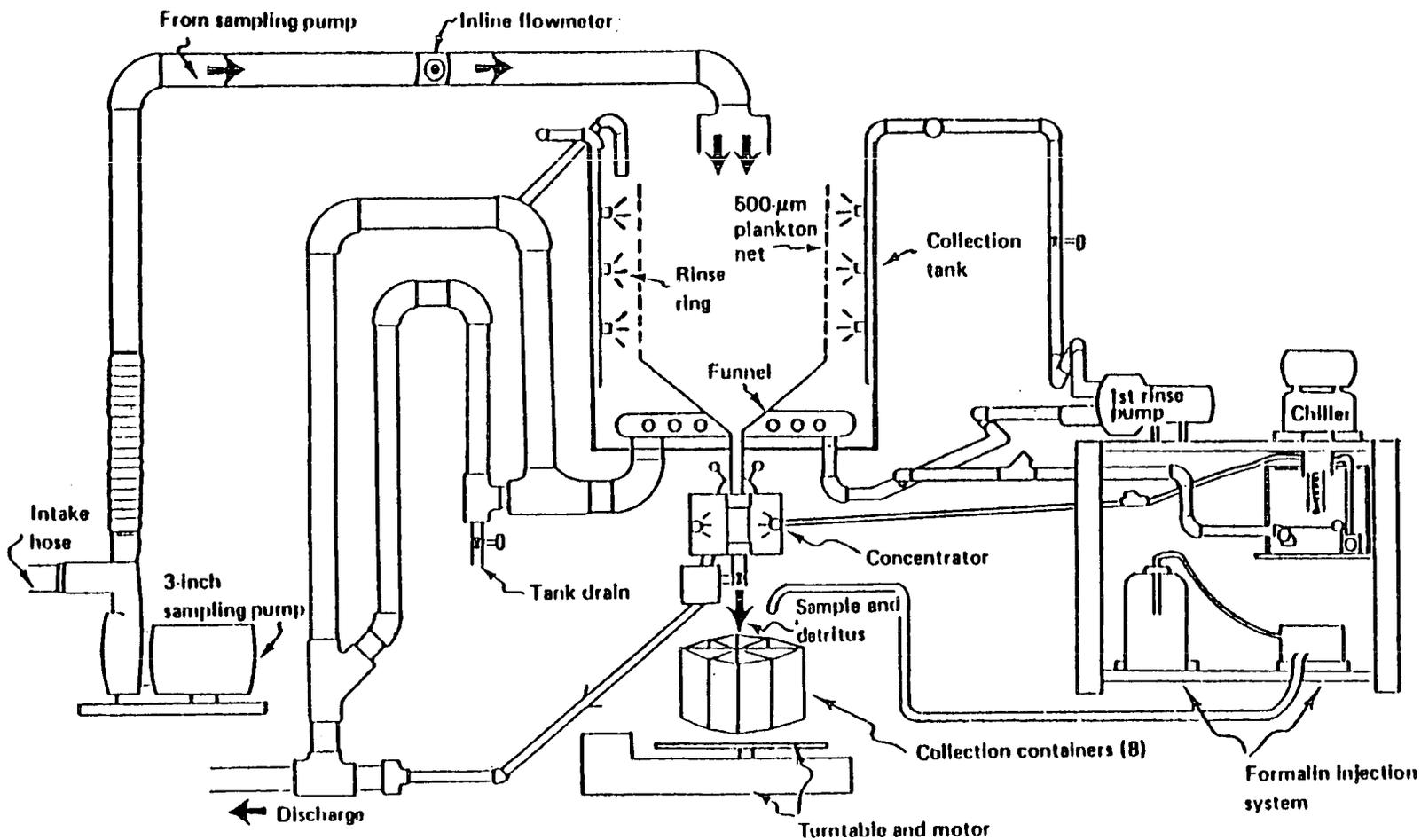


Figure 1.5. Schematic diagram of sampling components of EA's automated abundance sampling system - AUTOSAM (U.S. Patent No. 4, 145, 925).

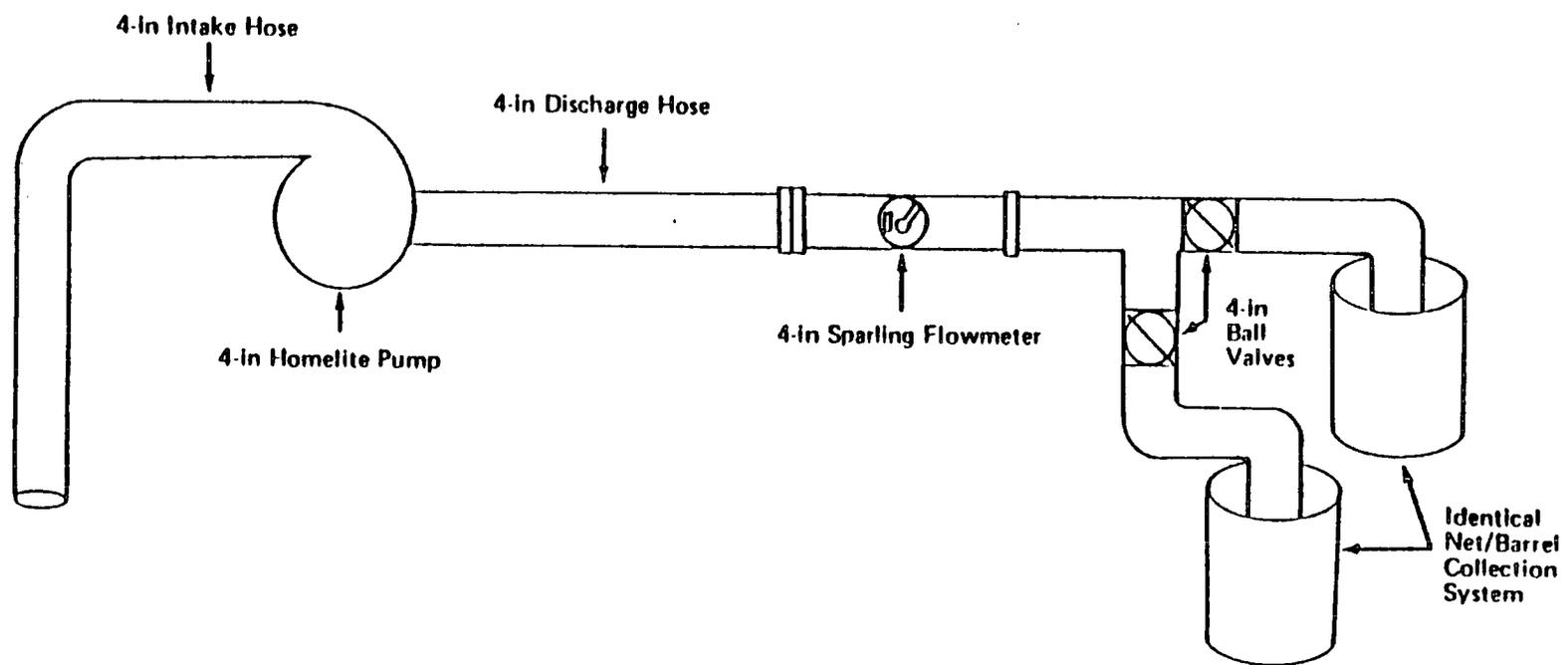
to achieve a 10 percent formalin to water solution. After each sample is collected, the turntable holding the collection containers rotates, and the sampling sequence automatically begins again.

Each sample can be comprised of many cycles; a cycle being defined as a sequence of pumping, net washdown, and transfer of sample contents to collection container. For the 1983 Entrainment Monitoring Program at the Bowline Point Generating Station, each sample was a 3-hour composite of 1-hour cycles (specifically, each cycle consists of 57 minutes of sample collection and three minutes of net wash and sample transfer). Because of the relatively long sampling period for each sample, the normal operational sequence was modified to inject formalin into the collection container after each 1-hour cycle to avoid potential predation and decomposition. In addition, the collection of samples in 1-hour cycles resulted in less damage to ichthyoplankton by reducing the times these organisms would be in the primary collection net.

At Indian Point from 3 May through 18 May each sample was a 3-hour composite, combining 1-hour cycles (specifically, each cycle consisted of 57 minutes of sample collection and 3 minutes of net wash and sample transfer). To reduce clogging as a result of heavy detritus, samples collected after 19 May were 3-hour composites consisting of six 27-minute collections with a 3-minute wash.

Samples at Roseton were collected with pump and net/barrel collection systems and were taken from the cooling water discharge seal-well. The location of the intake hose opening was fixed so as to collect samples from a constant depth throughout the season. At Roseton, this point was at the approximate midpoint of the discharge pipe, 1.8 meters off the bottom. The pump and net/barrel collection system is depicted in Figure 1.6. A Homelite 4-in. pump (Model 610 GPM) with 4-in. intake and discharge hose was used to direct the sample through an inline 4-in. Sparling flowmeter (Master Flo, Liters x 1000 Model) which measured total volume and flow rate. Prior to the sampling season, Sparling flowmeters used at Roseton were shipped to the Sparling Division of Envirotech where the meters were calibrated for use with 4-in. pipe. Two 4-in. ball valves enabled the operator to direct flow into one of two net/barrel systems so that sample collection and net-washdown/sample recovery could occur simultaneously. Each tank contained a 500- $\mu$  cylindrical collection net with a submerged area of approximately 15 square feet (Figure 1.6). The bottom of the net was attached to a polyethelene funnel to simplify transfer of the sample to a container.

Approximately 15 minutes prior to the initiation of sampling (scheduled for 0900 hours), the sampling pump was started and adjusted to provide a constant rate of 275 gallons per minute (1,040 liters/minute). When the start time was reached, sampling was initiated by recording the flowmeter value and by adjusting the ball valves to divert water to one barrel. The contents of the barrel filled during flow calibration were discarded. Sampling continued for 30 minutes with numerous checks conducted to assure a constant flow rate.



1.14

Figure 1.6. Schematic diagram of entrainment abundance collection gear, Roseton, 1983.

After 30 minutes, the total volume sampled was recorded and flow was diverted to the other barrel for the next 30 minutes of sampling. During this time, the net was washed down and the sample recovered from the barrel. The procedure was repeated until the entire 24-hour sampling event was completed. Successive 30-minute samples were consolidated into twenty-four 1-hour (2 x 30-minute) composite samples.

Field collection procedures required strict adherence to specific sampling volumes. Failure to maintain a flow of  $275 \pm 25$  gallons per minute and  $8,250 \pm 750$  gallons per 30 minutes for three consecutive 30-minute collections or during any four of the forty-eight 30-minute collections in a 24-hour period required the invalidation of all samples collected to that point and the reinitiation of a 24-hour sampling series at the nearest 1-hour interval.

All samples were preserved with 10 percent formalin and stored in jars labeled inside and out. All collection information was recorded by field crews onto standard data sheets.

During the 1983 sampling season, a study was conducted at two generating stations in order to examine the collection efficiency of the sampling gear. The tests were conducted at Roseton and Danskammer Point on 5 August and 8 August 1983, respectively. Because of the unavailability of hatchery-reared striped bass larvae in 1983, neutrally buoyant beads were used as the release object. The beads were introduced at the pump intake pipe and recovered after 30 minutes of sample collection. A total of 5 batches of 50 beads per batch were released into each net/barrel collection system at each of the two plants. Recovery rates were very high: at Roseton, 499 beads were recovered out of the total of 500 released; at Danskammer Point, 496 beads out of 500 were recovered (EA, 1984c, Section 5.6).

The sampling program at Roseton was supplemented by concurrent and additional sampling that was done at Danskammer Point. Sampling data from Danskammer Point can be used in the estimation of entrainment at Roseton during intervals when sampling was not done at Roseton, but this approach will not be considered in this report.

Replicate ichthyoplankton samples at Indian Point were also collected using the manual pump/net abundance sampler from 17 June through 8 July 1983. Sampling was scheduled to occur Monday, Wednesday, and Friday throughout that period. Similar to AUTOSAM collections, each sampling event was a 24-hour continuous collection effort separated into eight discrete 3-hour samples

Two manual pump/net samplers were used to collect samples from the discharge canal at Station D2 (Figure 1.7). The pumps were 4-in. gas-powered Home-lite trash pumps (Model 160-TP4-1). Inlets for the two 10.1-cm diameter intake hoses were located in the discharge canal, adjacent to the

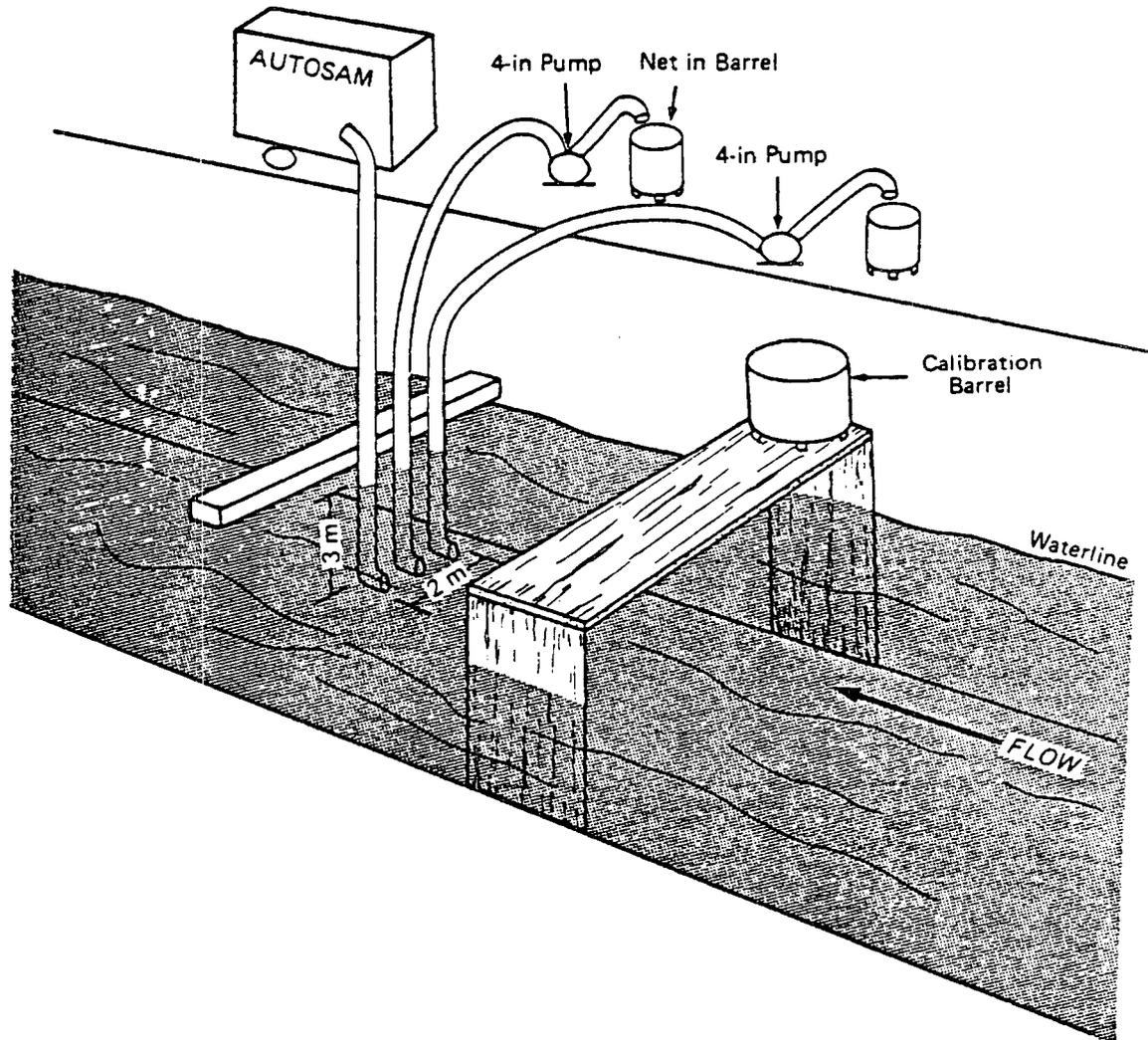


Figure 1.7. Generalized view of manual pump/net abundance and AUTOSAM collection systems used at D2 at the Indian Point Generating Station in 1983.

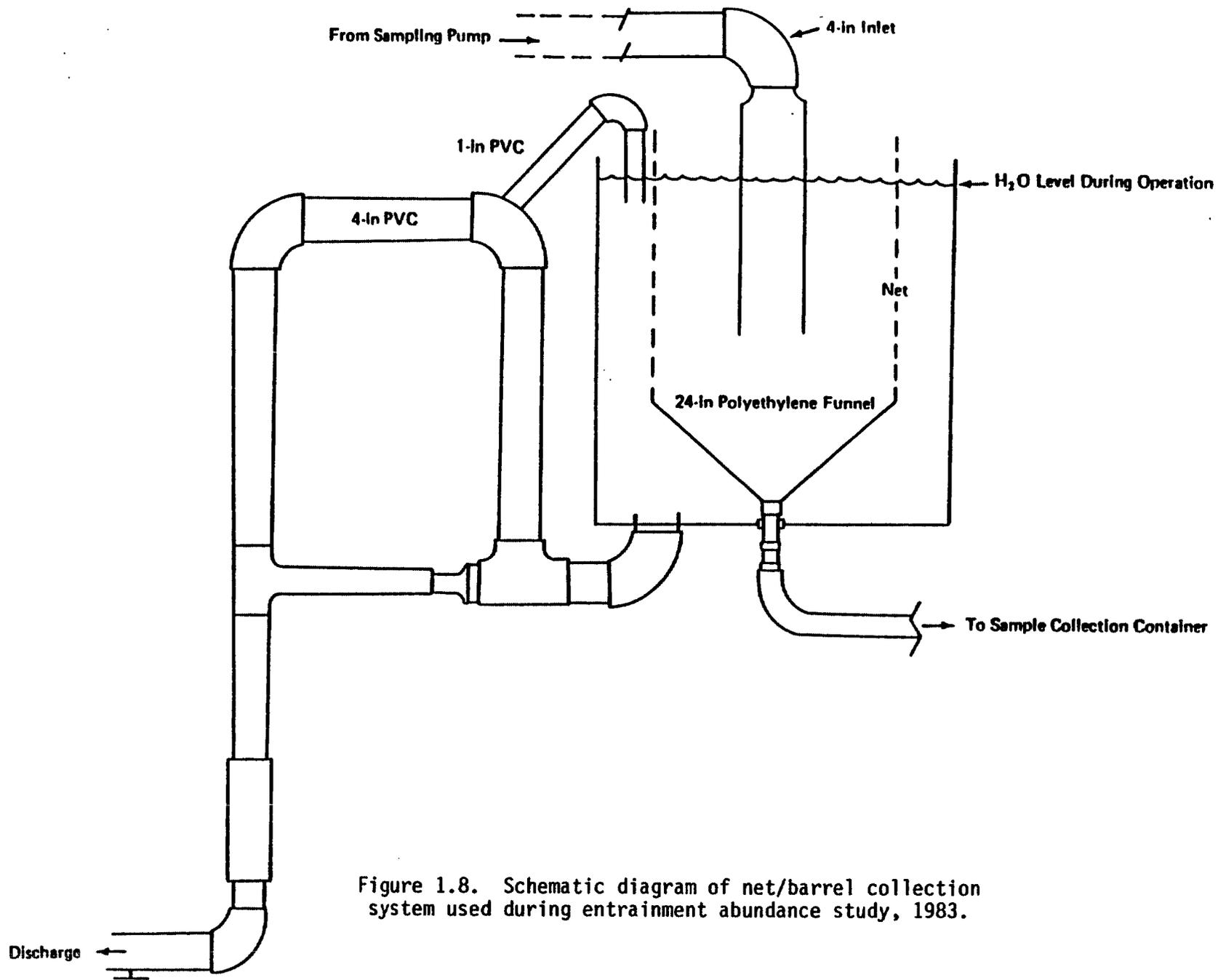


Figure 1.8. Schematic diagram of net/barrel collection system used during entrainment abundance study, 1983.

inlet of the AUTOSAM intake line. Inlets were at middepth and oriented into the discharge water flow. Each pump sampled a measured volume of water that was recorded by an inline 4-in. Sparling (Master Flo) flowmeter. The samples were pumped into a modified 500-um mesh collection net (0.6-m diameter, 1.2-m long) suspended in a collection barrel (Figure 1.8). (Samples collected prior to 1 July were collected with a 333-um mesh net.) At the end of the sampling interval, the pump was shut off, the collection barrel allowed to drain, and the ichthyoplankton and detritus were concentrated in a codend.

Similar to AUTOSAM collections, each sample was a 3-hour composite combining one-hour collections (for the manual pump/net system, each sample collection was approximately 50-55 minutes in duration with 5-10 minutes of net washdown). After each 1-hour collection, the sample was transferred to the sample jar having both inside and outside labels, and was preserved with 10 percent formalin. All collection information was recorded by field crews onto standard data sheets.

Juvenile fish were collected from 15 June through 9 August 1983 using the net sampler apparatus (Figures 1.8 and 1.9) located at Station D1 in the discharge canal at Indian Point (Figure 1.3). Throughout this period, net collections were scheduled three days per week on a random basis. Net collections also occurred continuously over a 24-hour period, and consisted of eight 3-hour samples.

Net samples were collected using a 0.5-m diameter conical plankton net (571-um mesh, 1.8-m long) mounted to a vertical frame which could be raised above or lowered beneath the water surface by an electric winch. During sampling the net was lowered to within 0.5 m of the bottom, then raised in a controlled step-wise manner to provide samples that were evenly distributed throughout the water column. Flow was recorded from two General Oceanics (GO) flowmeters (Model 2030), one positioned in the center of the net's mouth and the other located outside, but close to, the net. Flowmeters were changed between each sample in a 24-hour period.

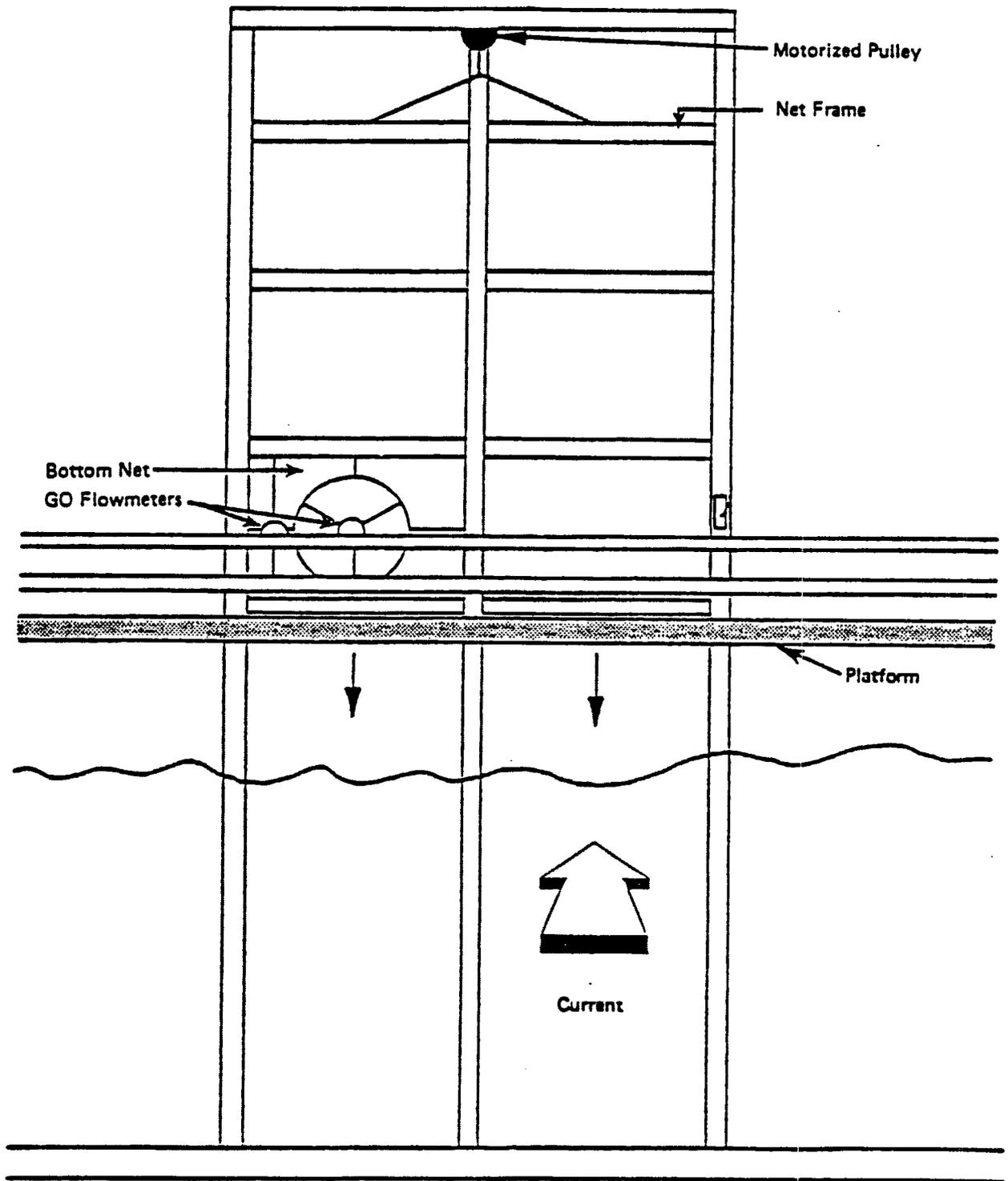


Figure 1.9. Net sampling apparatus at discharge canal Station D1, Indian Point Generating Station.

## 2. SUMMARY AND CONCLUSIONS

Entrainment abundance sampling designs for Bowline Point, Indian Point, and Roseton Generating Stations are examined in this report. The primary goal is to determine how efficient various sampling designs are in evaluating the effectiveness of plant outages in reducing entrainment impact. Total ichthyoplankton counts and counts of several specified taxa are treated. Estimates of entrainment abundance and the reduction in entrainment due to outages are developed for sampling designs of interest, including those designs currently in use at the generating stations.

The class of sampling designs under consideration is described. It includes two-stage and three-stage sampling designs. In two-stage sampling, the total volume of water passing through a plant is divided into primary sampling units or p.s.u.'s, which correspond to time intervals during the entrainment season. Samples are drawn from some or all of these p.s.u.'s and are analyzed for the presence of entrained ichthyoplankton. In three-stage sampling, these samples are subdivided into splits, which are analyzed for entrainment. The number of splits that are analyzed from a given sample may be either fixed or adaptive. With adaptive sampling, the number of splits analyzed from any sample can depend on the entrainment pattern observed in that sample.

The assumptions needed for a comparative analysis of these sampling designs are carefully stated. Formal statistical criteria by which sampling design performance will be measured are discussed. One of these is the absence or near absence of bias in entrainment estimates associated with a sampling design. Another is low variances for these estimates. A good sampling design will produce point estimates of entrainment having little or no bias and low variance, and it will be able to deal with rapid changes in the level of entrainment.

The distribution of ichthyoplankton in the discharge channel is a central aspect of the estimation problem. Bowline Point and Roseton discharge through a pipe; Indian Point discharges through an open channel. The distribution of organisms in the discharge channel is closely related to the sample counts observed; information from the investigation of distributions, through analysis of sample counts, is applied to the estimation of entrainment.

In work to date on the estimation of entrainment abundance, the variability of the estimates has not been properly addressed. This is a critical issue, because the estimated variance or standard error of any estimator provides essential information about the precision of that estimator. Estimated variances and standard errors of entrainment abundance and entrainment reduction estimates are not reported in the Indian Point and Roseton 1983 Annual Reports. These quantities are reported in the

Bowline Point 1983 Annual Report; however, they are computed using the assumption of a Poisson model for variation in the discharge channel. This model does not agree with data obtained in replicate sampling, as detailed in Chapter 4 below. Consequently, estimated variances and standard deviations of entrainment abundance estimates based on the highly specific properties of the Poisson model are only valid under special circumstances.

A different model that is consistent with the replicate sampling data has been determined. We show in Chapter 4 that the negative binomial model agrees very well with these data. This class of distributions is more general and flexible than the Poisson family. We have developed a method for estimating the variances of entrainment abundance estimates from the data under the negative binomial model. Thus, the variance and standard error of entrainment abundance estimates can now be estimated in a valid manner. This is a considerable advance over previous methodology; a point estimate of entrainment is of very limited value unless accompanied by an indication of its variability or margin of error.

The shift from the Poisson model to the negative binomial model is a logical evolutionary change. The Poisson model is the simplest model for random variation in the discharge channel. It was a biologically plausible and analytically convenient starting point. Only with the collection of replicate sampling data in 1982 and 1983 did it become possible to test whether the Poisson model provides an adequate explanation of variation in the discharge channel. It does not, but the negative binomial model does. The negative binomial family includes all Poisson distributions, so moving from Poisson-based to negative binomial-based analyses is a natural progression.

The effect of sample duration on the precision of entrainment estimates is treated. Shorter p.s.u.'s increase the precision of estimation, leading to a recommendation of one-hour to three-hour p.s.u.'s. A model is investigated that would explain the occurrence of sample counts fitting the negative binomial distribution if the true behavior of the organisms were Poisson. In this model, errors in classifying organisms having a Poisson distribution lead to observations that are not Poisson but are negative binomial. Examination of data in which classification error is virtually absent leads to the conclusion that this classification error model cannot be used to explain the negative binomial nature of the entrainment data. In other words, the hypothesis that actual Poisson counts are perturbed by classification errors, resulting in negative binomial data, is not supported by our analysis of the data.

The special case of juvenile data is discussed. The limited availability of replicate observations for juveniles makes it difficult to determine whether the negative binomial model is needed, that is, whether the Poisson model must be replaced by a more general distribution in order to obtain a satisfactory fit to the available juvenile data. Based on the small amount of data gathered, it appears that the Poisson distribution may tentatively be regarded as satisfactory. If this should be confirmed by future analysis of further replicate data, there will be little difference between the Poisson and the negative binomial approaches.

The estimation of entrainment abundance and entrainment reduction is then considered. Formulas for confidence intervals are derived based on the negative binomial distribution of sample ichthyoplankton counts. The effect of sample volume on the precision of estimation is examined. Methods are presented for both entrainment abundance and the effect of outages in mitigating entrainment. The standard assumption that ichthyoplankton entrainment densities are independent of the cooling water flow rate allows our method to be applied to projected as well as actual cooling water flows.

A large simulation study of the entrainment process is presented to show the behavior of entrainment estimators under a variety of conditions. Entrainment sampling is first examined over the course of a single day, for a wide range of organism densities and sample volumes and for uniform and diel daily variation. Entrainment over longer periods is then treated: 3 day, 5 day, one week, 18 day, and 34 day periods are simulated. An important conclusion is that sample volumes of 150 cubic meters per 3-hour period, or 1200 cubic meters per 24-hour day, are sufficient for purposes of entrainment estimation over the range of parameters considered; larger sample volumes produce only slight gains in accuracy of estimation.

A potentially valuable technique for laboratory analysis of field collections has been presented. This approach, known as sum-quota sampling, is a more sophisticated version of the splitting of field samples that was done on some Bowline Point 1982 data. Sum-quota sampling, an adaptive form of three-stage sampling design, is a more efficient sampling procedure than two-stage or fixed-number-of-splits three-stage sampling designs requiring comparable sampling effort. It would reduce the cost of laboratory processing of entrainment samples at the three generating stations, requiring less sampling effort to achieve the same accuracy in estimation as competing fixed-sample-size sampling designs. At present, sum-quota sampling cannot be implemented for two major reasons: first, the laboratory equipment currently available for sample splitting does not provide the necessary randomness in dividing the ichthyoplankton in the sample among the splits of subsamples; and, second, use of the data for other purposes in addition to estimation of entrainment abundance and reduction may require that sum-quota sampling not be used.

Noncontinuous sampling plans, in which sampling of the discharge flow is not scheduled during preselected periods within the entrainment season, present more difficulties than continuous sampling plans. The omission of these periods, ranging in length from one to six days, increases the variance of entrainment estimates. Even more important, the systematic nature of noncontinuous sampling results in estimates that are biased except under the assumption that certain special conditions hold. Neither the direction nor the magnitude of the bias can be inferred from the data. Negative bias is a clear possibility, stemming from the chance that a peak level of entrainment will occur during a sampling gap of one or several days; if this happens, entrainment will be underestimated. Entrainment estimators will be unbiased, however, if there is a linear trend in the daily entrainment during each sampling gap.

## 2.4

The specific features recommended for an entrainment abundance sampling design are: continuous sampling (or as nearly continuous sampling as possible); sampling fractions (that is, volume sampled in p.s.u. divided by discharge flow volume in p.s.u.) that are as nearly equal as possible for all p.s.u.'s; adequate total volume sampled to yield acceptable confidence intervals for the entrainment quantities of interest; sufficient replicate sampling to allow precise estimation of the quadratic variance parameter  $c^2$ , leading to shorter confidence intervals; and p.s.u.'s of one to three hours.

Additional studies related to entrainment estimation are proposed to examine the properties of replicate sampling, the relationship between entrainment and plant operating factors, and the use of entrainment estimates from previous years.

### 3. SAMPLING DESIGNS UNDER CONSIDERATION

#### 3.0 INTRODUCTION

The sampling design encompasses several phases of an entrainment abundance study that are often viewed as separate activities: the collection of field samples, the rules for determining what types of laboratory analysis are to be performed on which samples, and the method of deriving from laboratory data estimates of entrainment abundance and reduction. These aspects of the study must be viewed as a cohesive whole; a poor choice at any of these stages introduces inefficiency that cannot be removed from the estimation process, no matter how well the remaining stages are executed. Data collected from a field plan that is flawed cannot be redeemed by superior laboratory work and statistical analysis. Perhaps more surprisingly, a sound field collection plan can be offset by a pattern of laboratory analysis that fails to extract as much information as possible from the field samples. Finally, statistical estimation procedures that fail to make full use of the information emerging from the laboratory will produce inefficient estimates even from data of the highest quality.

The field sampling rules must provide full information on when sampling is to take place, how many samples (one or more) are to be drawn concurrently, where the discharge channel is to be sampled, and so on. The laboratory rules must provide full information on how the sample is to be processed, including any splitting of the sample into smaller parts and decisions on which of these parts to analyze. The statistical estimation procedures must provide appropriate, efficient point estimates and confidence intervals for entrainment abundance and reduction.

We shall continue to refer to the overall process of field collection, laboratory analysis, and statistical estimation as "sampling design," largely for reasons of historical precedent in previous entrainment studies. This process is more commonly referred to in the statistical literature as "experiment design," a phrase that reflects more clearly the concern with all aspects of the investigation.

Any sampling design must be considered as a complete process that includes field collection, laboratory analysis, and statistical estimation. Field collection plans cannot be rationally compared without assessing how laboratory and statistical analyses will be applied and what the consequences for estimation will be. Similarly, neither laboratory analyses nor statistical estimation procedures can be compared without considering the other aspects of sampling design.

This chapter describes the two-stage and three-stage sampling designs being considered for entrainment abundance studies in the future, including those currently in use. It also specifies the assumptions required for the derivation of entrainment abundance estimates appropriate for each of these designs.

In a three-stage sampling design, each sample is divided in the laboratory into several portions called splits, only some of which are processed. This allows us to allocate a specified level of sampling effort by dividing each sample into splits and analyzing a greater number of samples partially instead of analyzing fewer samples completely. This procedure gives somewhat less information about a particular partially-processed sample than would be available if processing were complete, but it allows more or larger samples to be (partially) processed for a given total cost. To determine whether splitting is beneficial, we must ascertain whether the process of field collection, laboratory analysis, and statistical estimation gives improved results--more accurate estimates--with or without splitting.

A fact emerging clearly from this chapter is the critical need for a model that accurately reflects the distribution of ichthyoplankton in the discharge channel. One reason for this is that replicate sampling, taking more than one sample in a discharge channel simultaneously, has been done very infrequently. The standard two-stage and three-stage sampling formulas for the variability of estimates contain terms that do not exist when replicate sampling is absent. Thus, estimated variances must be inferred from unreplicated samples by other methods, and the model approach is the natural choice. A second reason supporting the model approach is that even if replicate sampling has been performed, modeling the distribution is likely to provide more accurate estimation of entrainment abundance. Consider estimating entrainment abundance by using these two contrasting strategies:

- (1) Make minimal assumptions about the process, or no assumptions at all. Develop estimation formulas that are valid under the extremely general conditions that these minimal assumptions hold.
- (2) Make moderate or strong assumptions about the process, limiting the range of behavior the process is likely to exhibit. Develop estimation formulas that seek to perform with the greatest possible efficiency when the assumed conditions hold.

Making the assumptions referred to in the second strategy specifies a model for the process. This is the definition of a statistical model. When the assumptions do in fact hold, the estimation procedures produced by the second strategy can be expected to outperform the competing procedures produced by the first strategy. This is because the second strategy procedures have been selected to do better than any other

procedures under these conditions. If the entrainment data are consistent with a specified model, then estimation procedures derived from this model's properties should give more accurate results than more general, non-model-based estimation procedures. The extrinsic information supplied in the form of a model thus enhances the intrinsic information supplied by the data.

To state this another way, if an estimation formula is valid under extremely general conditions, this generality often entails a loss of efficiency for a specific situation. An example, which is not connected to entrainment estimation but which illustrates the point simply, is the following: it is desired to estimate the variance  $\sigma^2$  of the distribution from which the random sample  $x_1, \dots, x_n$  has been drawn. For any distribution with finite variance of  $\sigma^2$ , an unbiased estimate of  $\sigma^2$  is given by

$\sum_{i=1}^n (x_i - \bar{x})^2 / (n-1)$ . This estimator is clearly valid under extremely

broad conditions. However, if the distribution is known to be Poisson, then a second unbiased estimator of  $\sigma^2$  based on the Poisson model is given by  $\bar{x}$ . This estimator  $\bar{x}$  has lower variance than the first unbiased estimator when the distribution is actually Poisson, which makes it more efficient under the Poisson model. Thus, if the data  $x_1, \dots, x_n$  are consistent with the Poisson model, then  $\bar{x}$  is a better choice as an estimator of  $\sigma^2$ .

### 3.1 TWO-STAGE SAMPLING DESIGNS

In a two-stage sampling design, the entire volume of water passing through a generating station is divided into units called primary sampling units or p.s.u.'s. A set of these is selected for examination. It is logical to let p.s.u.'s correspond to disjoint, exhaustive time intervals, so a given p.s.u. is the cooling water that passes through the plant in a particular time period. For example, if the p.s.u.'s correspond to three-hour periods over a ten-day interval beginning at midnight, then there are 80 p.s.u.'s: Day 1 0000-0300, Day 1 0300-0600, Day 1 0600-0900, and so on up to Day 10 2100-2400. Some, and perhaps all, of these will be selected for subsequent examination.

Each p.s.u. is conceptually divided into subvolumes called subunits, some of which are selected at random for laboratory analysis. The need for subunits is clear, as a p.s.u. is much too large for laboratory analysis. Any subunit selected for laboratory analysis will be called a sample. The number of p.s.u.'s available, the number selected, the numbers of subunits available and selected within each p.s.u., and the counts of organisms entrained in all chosen subunits are known. Under very broad assumptions, this information yields an estimate of the total count of organisms entrained in the entire volume of water passing through the plant. (Although the exposition here treats the total overall count of organisms, it applies without change to any particular taxon and life stage.)

The usual estimation process for two-stage sampling is now outlined. Difficulties inherent in using two-stage sampling will then be discussed. The treatment here follows the notation of Cochran (1977), a standard reference on this material. For convenience, only the case of equal-sized p.s.u.'s is considered, as the complexities of the unequal-sized p.s.u. case do not alter the main features of this discussion.

The population quantities in two-stage sampling are

$N$  = total number of possible p.s.u.'s

$M$  = number of possible subunits in each p.s.u.

$Y_{ij}$  = number of organisms entrained in the  $j^{\text{th}}$  subunit of the  $i^{\text{th}}$  p.s.u.,  $i=1, \dots, N$ ,  $j=1, \dots, M$

$\bar{Y}_i$  = mean number entrained in the  $i^{\text{th}}$  p.s.u. (over the  $M$  subunits in this p.s.u.),  $i=1, \dots, N$

$\bar{Y}$  = overall mean number of organisms entrained (over all  $NM$  possible subunits)

$Y$  =  $NM\bar{Y}$  = total overall count of organisms entrained (over all  $NM$  possible subunits)

$S_1^2$  =  $\sum_{i=1}^N (\bar{Y}_i - \bar{Y})^2 / (N-1)$  = variance among the  $N$  p.s.u. means  $\bar{Y}_i$

$S_2^2$  =  $\sum_{i=1}^N \sum_{j=1}^M (Y_{ij} - \bar{Y}_i)^2 / N(M-1)$  = variance among subunits within p.s.u.'s

The sample statistics are

$n$  = number of p.s.u.'s selected

$m$  = number of subunits or samples selected in each p.s.u.

$f_1$  =  $n/N$  = fraction of p.s.u.'s selected

$f_2$  =  $m/M$  = fraction of subunits or samples selected

$y_{ij}$  = number of organisms entrained in the  $j^{\text{th}}$  (selected) subunit of the  $i^{\text{th}}$  (selected) p.s.u.,  $i=1, \dots, n$ ,  $j=1, \dots, m$

$\bar{y}_i$  =  $\sum_{j=1}^m y_{ij} / m$  = mean number of organisms in the  $m$  selected subunits of the  $i^{\text{th}}$  (selected) p.s.u.,  $i=1, \dots, n$

$$\bar{y} = \sum_{i=1}^n \bar{y}_i / n = \sum_{i=1}^n \sum_{j=1}^m y_{ij} / nm = \text{mean number of organisms in the } nm \text{ observed subunits}$$

$$y = nm\bar{y} = \text{total count of organisms entrained in the } nm \text{ observed subunits}$$

$$\hat{Y} = NM\bar{y} = (NM/nm)y = \text{product of the number of entrained organisms counted in the entire sample and inflation factors } N/n = 1/f_1 \text{ and } M/m = 1/f_2$$

$$s_1^2 = \sum_{i=1}^n (\bar{y}_i - \bar{y})^2 / (n-1) = \text{observed variance among the } n \text{ observed p.s.u. means } \bar{y}_i$$

$$s_2^2 = \sum_{i=1}^n \sum_{j=1}^m (y_{ij} - \bar{y}_i)^2 / n(m-1) = \text{observed variance among subunits within p.s.u.'s}$$

This approach to estimation in two-stage sampling divides the overall variation in the set of  $NM$  subunits into two components:  $S_1^2$  is the variance among the  $N$  means of the p.s.u.'s, and  $S_2^2$  is the variance among subunits within p.s.u.'s, that is, the variance among subunits within a single p.s.u. averaged over the  $N$  p.s.u.'s. The estimate  $\hat{Y}$  of the total entrainment  $Y$  is obtained by randomly selecting  $n$  p.s.u.'s, taking  $m$  randomly chosen subunits within each of these p.s.u.'s, calculating the mean number  $\bar{y}$  of organisms found in the  $nm$  observed subunits, and multiplying this mean by the total number of subunits in the entire volume of water,  $NM$ . This produces the estimator

$$\hat{Y} = NM\bar{y}.$$

The variance of this estimator is affected by  $S_1^2$ ,  $S_2^2$ , and the choices of  $n$  and  $m$ . It is given by the formula

$$\text{Var}(\hat{Y}) = N^2 M^2 [(1 - f_1)S_1^2/n + (1 - f_2)S_2^2/nm].$$

This variance increases as the variance among the  $N$  p.s.u. means,  $S_1^2$ , or the variance among subunits within p.s.u.'s,  $S_2^2$ , increases, or as both of these variances increase simultaneously. As the number of p.s.u.'s selected, which is  $n$ , approaches and finally equals  $N$ , the term  $1 - f_1$

decreases to 0, so the term in the variance of  $\hat{Y}$  involving the variance among the  $N$  p.s.u. means diminishes in importance as more p.s.u.'s are sampled. The corresponding decrease takes place in the  $S_2^2$  term as more subunits within each selected p.s.u. are selected and analyzed.

An unbiased estimator of  $Y$  is given by  $\hat{Y}$ , and an unbiased estimator of  $\text{Var}(\hat{Y})$  is given by

$$\hat{\text{Var}}(\hat{Y}) = N^2 M^2 [(1 - f_1) s_1^2 / n + f_1 (1 - f_2) s_2^2 / nm].$$

(The estimator  $\hat{\text{Var}}(\hat{Y})$  is derived by showing that  $s_2^2$  is an unbiased estimator of  $S_2^2$  and that  $s_1^2 - (1 - f_2) s_2^2 / m$  is an unbiased estimator of  $S_1^2$ .)

Two important issues must be dealt with in attempting to use these two-stage sampling design formulas to estimate entrainment abundance. The first of these, a major analytical problem with the current sampling programs, is that a single subunit is selected from each p.s.u., making  $m$  equal to 1. In other words, only one sample is drawn from the cooling water passing through the plant in each time period. This makes it impossible to calculate  $s_2^2$  and therefore  $\hat{\text{Var}}(\hat{Y})$ . Intuitively, the reason that this leads to technical difficulties is clear: when only one subunit is observed per p.s.u., there is no way to estimate the variability among subunits within a p.s.u. It is possible to use an externally derived estimator  $s_2^2$ , that is, an estimator of  $S_2^2$  obtained from sources other than the entrainment sampling considered here. Such an external estimator  $s_2^2$  would allow us to compute  $\hat{\text{Var}}(\hat{Y})$  when  $m = 1$ ; however, this approach will not be pursued in this study.

The other issue is that these two-stage sampling design formulas make no use of any information except the  $nm$  observed subunit counts  $y_{ij}$ . The formulas are thus valid under extremely general conditions: when  $n > 1$  and  $m > 1$ ,  $\hat{Y}$  is an unbiased estimator of  $Y$  whose estimated variance is  $\hat{\text{Var}}(\hat{Y})$ . However, such generality often entails a loss of efficiency for a specific situation, as the example at the end of Section 3.0 illustrated.

The generality of  $\hat{\text{Var}}(\hat{Y})$  suggests that if information about the process that produced the  $y_{ij}$ 's were available, it could be used to derive an improved estimate of  $\text{Var}(\hat{Y})$  whose variance is smaller than the variance of  $\hat{\text{Var}}(\hat{Y})$ .

As a further illustration that distributional information about a process can lead to improved estimation, we note that under certain circumstances, it is possible to find an unbiased estimator of  $Y$  that has smaller variance than  $\hat{Y}$ . Specifically, if the subunit counts  $y_{ij}$  were independent,

identically distributed Cauchy random variables (or equivalently, followed a t distribution with 1 degree of freedom), multiplying  $NM$  by the median of the observed subunit counts would give an unbiased estimator of  $Y$ . In this situation, neither the mean nor the variance of  $\hat{Y}$  exists; thus  $\hat{Y}$  is not an unbiased estimator of  $Y$ , and its variance can be interpreted as infinite. (See Mood, Graybill, and Boes, 1974, pp. 117,540.) We mention this example only in passing, because the circumstances do not pertain directly to the entrainment sampling situation; however, they dramatize the relevant point that knowledge of the model followed by a process can have enormous impact on the choice of estimation methods.

Information about the model followed by the entrainment process may be able both to overcome the lack of replicate sampling when  $m=1$  and to improve on the general estimation procedures when  $m$  exceeds 1. For example, if subunit counts within each p.s.u. are independent, identically distributed Poisson random variables, then  $S_2^2$  can be estimated by

$\bar{y}$  when  $m=1$  as well as for larger values of  $m$  (EA, 1983b, pp. E-13 to E-15). For this reason and because the turbulence within the discharge flow suggests a spatially random distribution of organisms in the flow cross-section, it is tempting to make the assumption that independently drawn samples from a p.s.u. will be a set of randomly drawn observations from a Poisson distribution. (The exact meaning of a spatially random distribution is discussed in Appendix I.) However, Chapter 4 of this report will show that the Poisson model does not fit replicate sampling data from the generating stations, but that a related model does.

Thus, estimates of the variability of  $\hat{Y}$  based on the assumption of Poisson structure among samples within each p.s.u. are not valid for these data.

In the past and present sampling designs used at the three generating stations,  $m$  has been restricted to the value 1 except in relatively infrequent replicate sampling experiments. For this reason, the estimation of entrainment abundance depends critically on the development of a model for the entrainment process. This provides not merely an opportunity to improve the estimator  $\hat{\text{Var}}(\hat{Y})$ , but a technique for deriving an estimator of  $\text{Var}(\hat{Y})$  under circumstances ( $m=1$ ) when  $\hat{\text{Var}}(\hat{Y})$  does not even exist.

The parameters of a two-stage sampling design are

- (1)  $N$ , the total number of p.s.u.'s in the overall time period of interest, the entire entrainment season. Equivalently, the time interval corresponding to a p.s.u. can be used as the parameter, since  $N$  can be found easily from this time interval and the length of the season.
- (2)  $n$ , the number of p.s.u.'s selected for examination. Equivalently, the proportion  $f_1 = n/N$  of p.s.u.'s selected can be used.
- (3)  $M$ , the number of possible samples or subunits contained in an entire p.s.u.

- (4)  $m$ , the number of samples or subunits selected per p.s.u. Equivalently, the proportion of  $f_2 = m/M$  of available subunits chosen can be used.

The first two of these parameters can vary over a wide range. The time interval corresponding to a p.s.u., which is the total number of hours in the entrainment season divided by  $N$ , can be very short (one hour or less), very long (24 hours or longer), or somewhere between these extremes. Intervals shorter than one-half hour seem undesirable, as do intervals longer than 24 hours. Very short intervals result in samples containing a great deal of random variability or "noise" relative to the amount of information or "signal" present. They also involve more administrative work to label, transport, process in the laboratory, enter in computer data bases, and analyze statistically. There appears to be no benefit in improved estimation derived from this extra effort. On the other hand, when the time interval spanned by each p.s.u. is very long, the estimation of overall seasonal entrainment is based on relatively few p.s.u.'s rather than on relatively many shorter p.s.u.'s. Consequently, the accuracy of the seasonal entrainment estimate decreases as the time interval of a p.s.u. increases. The length of time chosen to serve as a p.s.u. is treated fully in Section 4.3.

The number of p.s.u.'s selected for sampling can range from relatively few of the  $N$  p.s.u.'s to all  $N$  of them. The proportion of  $f_1 = n/N$  can range from near zero to one. Continuous sampling, in which every p.s.u. is sampled, was in effect at Bowline Point (EA, 1984a) and Indian Point (EA, 1984b) generating stations during the 1983 entrainment season. At Roseton, however, sampling was not continuous during the 1983 entrainment season (EA, 1984c). Between May 10 and June 30, sampling was performed on 31 out of 52 days, with gaps occurring on May 11-16, 28, 30, and June 4-6, 11-13, 23-27, and 29-30. As might be expected, continuous sampling produces much more accurate estimates of entrainment than sampling with many p.s.u.'s omitted, especially if the gaps between sampling periods are long. Continuous and noncontinuous sampling designs are compared in Chapter 8.

The number of possible samples contained in an entire p.s.u. is given by

$$M = \frac{\text{volume of cooling water flow past sampler during p.s.u.}}{\text{volume of cooling water drawn for (one) sample during p.s.u.}}$$

If replicate sampling is performed, the denominator is the volume of a single sample, not the total volume of all samples taken simultaneously.  $M$  is determined by the volumes of cooling water flowing past the sampling gear and into it during the p.s.u.;  $1/M$  is the fraction of flow in the discharge channel constituting a sample. Changing the value of  $M$  would entail altering the physical sampling apparatus, e.g., changing the 4-inch diameter intake hose on the AUTOSAM to another size and making corresponding adjustments in the sample pumping rate. There does not appear to be any reason why this should be considered.

The number of samples drawn per p.s.u. has generally been  $m = 1$ . There have been some replicate sampling data collected recently, though, with simultaneous sampling occurring at more than one location within the discharge channel (EA 1984a, 1984b).

### 3.2 THREE-STAGE SAMPLING

In a three-stage sampling design, the entire volume of cooling water passing through a generating station is divided into p.s.u.'s corresponding to disjoint, exhaustive time intervals. A set of these is selected for examination, and a random sample of subunits is drawn from within each chosen p.s.u. The chosen subunits are also referred to as samples. Each of these subunits, or samples, is then further subdivided into several portions called splits or aliquots, some of which are selected in random order for laboratory analysis. When certain conditions are met, the rules of the sampling procedure may specify that no further splits from a given sample are to be processed. Should this occur, laboratory analysis continues by moving to another sample, dividing it into splits, and selecting these in random order for processing.

The number of p.s.u.'s available, the number selected, the numbers of samples available and selected within each p.s.u., the numbers of splits available and selected within each sample, and the counts of organisms entrained in all chosen splits are known. Under very broad assumptions, this information yields an estimate of the total count of organisms entrained in the entire volume of cooling water passing through the plant. (The treatment here of the overall count of organisms applies without change to any particular taxon and life stage.)

The usual estimation process for three-stage sampling is now outlined. Difficulties in utilizing these standard three-stage sampling estimation methods will then be discussed. As with two-stage sampling, we follow the notation of Cochran (1977), where formulas omitted here are found, and treat the case of equal-sized p.s.u.'s.

The population quantities in three-stage sampling are

- $N$  = total number of possible p.s.u.'s
- $M$  = number of possible samples (subunits) in each p.s.u.
- $K$  = number of possible splits in each sample
- $y_{ijh}$  = number of organisms entrained in the  $h^{\text{th}}$  split of the  $j^{\text{th}}$  sample within the  $i^{\text{th}}$  p.s.u.,  $i=1, \dots, N$ ,  $j=1, \dots, M$ ,  $h=1, \dots, K$

$\bar{y}_{ij}$  = mean number entrained in the  $j^{\text{th}}$  sample from the  $i^{\text{th}}$  p.s.u. (over the K splits in this sample)

$\bar{y}_i$  = mean number entrained in the  $i^{\text{th}}$  p.s.u. (over the MK splits in this p.s.u.)

$\bar{y}$  = overall mean number of organisms entrained per split (over all NMK possible splits)

$Y$  = NMKY = total overall count of organisms entrained (over all NMK splits)

$S_1^2$  = variance among the N p.s.u. means  $\bar{y}_i$

$S_2^2$  = variance among sample means within p.s.u.'s

$S_3^2$  = variance among splits within samples

The sample statistics are

$n$  = number of p.s.u.'s selected

$m$  = number of samples selected in each p.s.u.

$k$  = number of splits selected in each sample

$y_{ijh}$  = number of organisms entrained in the  $h^{\text{th}}$  (selected) split of the  $j^{\text{th}}$  (chosen) sample within the  $i^{\text{th}}$  (selected) p.s.u.,  $i=1, \dots, N, j=1, \dots, M, h=1, \dots, K$

$\bar{y}_{ij}$  = mean number of organisms in the  $k$  (selected) splits in the  $j^{\text{th}}$  (chosen) sample within the  $i^{\text{th}}$  (selected) p.s.u.

$\bar{y}_i$  = mean number of organisms per split in the  $mk$  selected splits in the  $i^{\text{th}}$  p.s.u.

$\bar{y}$  = mean number of organisms per split in the  $nmk$  selected splits

$y$  =  $nmk\bar{y}$  = total count of organisms entrained in the  $nmk$  selected splits

$Y$  =  $NMK\bar{y}$  =  $(NMK/nmk)y$  = product of the number of entrained organisms in the entire sample and inflation factors  $N/n$ ,  $M/m$ , and  $K/k$

$s_1^2$  =  $\sum_{i=1}^n (\bar{y}_i - \bar{y})^2 / (n-1)$  = observed variance among the  $n$  observed p.s.u. means  $\bar{y}_i$

$$s_2^2 = \frac{\sum_{ij} (\bar{y}_{ij} - \bar{y}_i)^2}{n(m-1)} = \text{observed variance among sample means within p.s.u.'s}$$

$$s_3^2 = \frac{\sum_{ijh} (y_{ijh} - \bar{y}_{ij})^2}{nm(k-1)} = \text{observed variance among splits within samples}$$

The variance of  $\hat{Y}$  is

$$\text{Var}(\hat{Y}) = N^2 M^2 K^2 \left[ \left( \frac{1}{n} - \frac{1}{N} \right) s_1^2 + \frac{1}{n} \left( \frac{1}{m} - \frac{1}{M} \right) s_2^2 + \frac{1}{nm} \left( \frac{1}{k} - \frac{1}{K} \right) s_3^2 \right].$$

An unbiased estimator of  $Y$  is given by  $\hat{Y}$ , and an unbiased estimator of  $\text{Var}(\hat{Y})$  is given by

$$\hat{\text{Var}}(\hat{Y}) = N^2 M^2 K^2 \left[ \left( \frac{1}{n} - \frac{1}{N} \right) s_1^2 + \frac{1}{n} \left( \frac{1}{m} - \frac{1}{M} \right) s_2^2 + \frac{1}{nm} \left( \frac{1}{k} - \frac{1}{K} \right) s_3^2 \right].$$

The same issues that arose with two-stage sampling design formulas must be dealt with in estimating entrainment abundance from the three-stage sampling design formulas. Because  $m = 1$ , reflecting the selection of only a single sample (subunit) from each p.s.u.,  $s_2^2$  and therefore  $\hat{\text{Var}}(\hat{Y})$  cannot be calculated. The component of variance due to variation among sample means within p.s.u.'s cannot be estimated. Furthermore, even if it were true that  $m > 1$ , allowing us to compute  $\text{Var}(\hat{Y})$ , the opportunity to find a better estimator of  $\text{Var}(\hat{Y})$  than  $\hat{\text{Var}}(\hat{Y})$  would be present if we could model the process that generated the  $y_{ijh}$ 's.

In deciding how many splits to analyze from each sample, we may elect either of two different types of strategy, fixed or adaptive. If we choose to analyze a fixed number of splits per sample, as in the estimation process just described, the methods are an extension of the two-stage sampling design techniques. If instead we choose to analyze a number of splits that may vary from one sample to another, the processing of splits can be terminated adaptively; that is, the laboratory instructions can specify that when certain conditions occur, analysis of splits from a given sample will terminate, any remaining splits from this sample will be discarded, and analysis of splits from a different sample will commence. Analyzing a fixed number of splits per sample is, in fact, a special case of adaptive sampling design: laboratory instructions would be to select splits from a sample at random, stopping when the condition occurs that  $k$  splits have been analyzed. Other possible adaptive rules include sum-quota sampling, to be discussed in Section 7.1, in which instructions would be:

select splits from a sample at random, terminating whenever some split raises the total number of ichthyoplankton counted in the sample above a specified threshold (e.g., to 200 or more); if the total sample count remains below the threshold, then count all splits. The class of all adaptive subsampling rules includes all designs with constant  $k$ , in which the same number of splits is processed from every sample, as well as a variety of more flexible designs; optimal or nearly optimal adaptive sampling designs are therefore likely to outperform optimal designs with a fixed number of splits by a wide margin.

The parameters of a three-stage sampling design are

- (1)  $N$ , the total number of p.s.u.'s in the entrainment season. Equivalently, the time interval corresponding to a p.s.u. can be used.
- (2)  $n$ , the number of p.s.u.'s selected for examination. Equivalently, the proportion  $f_1 = n/N$  of p.s.u.'s selected can be used.
- (3)  $M$ , the number of possible samples or subunits contained in an entire p.s.u.
- (4)  $m$ , the number of samples selected per p.s.u. Equivalently, the proportion  $f_2 = m/M$  of available samples chosen can be used.
- (5)  $K$ , the number of possible splits contained in a sample.
- (6)  $k$ , the number of splits selected from each sample. If an adaptive sampling design is used,  $k$  will vary among samples; here, instead of a fixed value  $k$ , a rule must be provided that specifies the conditions under which analysis of splits from a particular sample terminates.

The ranges of  $N$ ,  $n$ ,  $M$ , and  $m$  to be considered are the same as for two-stage sampling designs. Full details are given in Section 3.1.

The number  $K$  of splits into which a sample is divided is limited by the capabilities of the splitting device and by the practical considerations involved in laboratory processing. These matters, especially the capabilities of the splitting device, have a major impact on the feasibility of three-stage sampling. They are discussed in Section 7.0.

The number  $k$  of splits per sample selected for analysis in a design with fixed  $k$  can range from  $k = 1$  to  $k = K$ . The choice  $k = 1$ , however, has drawbacks analogous to those mentioned in the discussion of  $m = 1$  in Section 3.1: this choice makes it impossible to calculate  $s_3^2$ . Because we then cannot use  $s_3^2$  to estimate the variance among splits within samples, we must estimate this variance by making some assumption about the behavior of split counts within subunits. This issue is treated in Section 7.1.

For adaptive sampling designs,  $k$  varies among samples. An almost unlimited variety of rules for terminating the analysis of splits from any sample could be proposed. Consideration here will be limited to the sum-quota type sampling rules defined in Section 7.1. This class of rules will be seen to improve greatly on the performance of sampling designs with a fixed value of  $k$ .

For sampling designs in which  $k$  is fixed, optimal designs are those for which the choices of  $N$ ,  $n$ ,  $M$ ,  $m$ ,  $K$ , and  $k$  reduce the variances of entrainment estimates as much as possible, consistent with a fixed level of sampling effort and practical limitations on sampling patterns. The general practice, except in relatively infrequent investigations of replicate sampling, has been to make  $m = 1$ ; a model for the distribution of ichthyoplankton in samples from the discharge channel is of crucial importance in this case, as it provides the only way to make inferences from the observed counts to the entrainment abundance in the discharge channel.

For adaptive sampling designs, optimal designs involve choices among  $N$ ,  $n$ ,  $M$ ,  $m$ ,  $K$ , and rules for termination of split analysis. In sum-quota sampling designs, a major part of this choice hinges on the tradeoff among the time period of a p.s.u. (that is, the time interval spanned by a sample), the number  $K$  of splits into which each sample is divided, and the quota  $Q$  that tells how many organisms must be counted before split analysis ceases. The same level of sampling effort is required by many different combinations of these. Optimal designs produce the lowest possible variances for entrainment estimates at a fixed level of sampling effort. Good choices of design parameters can be made on the basis of information currently available. Optimization requires developing a formula for estimator variance as a function of  $N$ ,  $K$ , and  $Q$  and finding the best combination of  $N$ ,  $K$ , and  $Q$  for a given level of sampling effort.

### 3.3 ASSUMPTIONS REQUIRED FOR ANALYSIS

The analyses in this report are valid only if certain assumptions about the entrainment process and the sampling of that process are correct. It is desirable to assume as little as possible so estimates of entrainment abundance and entrainment reduction will be valid under widely varying conditions. The assumptions on which any statistical sampling design depends should be stated explicitly, to allow them to be scrutinized and perhaps tested by sampling experiments specifically designed to examine their validity. We now list and discuss the assumptions made in this report.

Assumption 3.1. The relationship between an entrainment sample and the contents of the entire discharge channel can be modeled in some way.

This is necessary to ensure that the contents of entrainment samples are related to the contents of the whole discharge channel in a manner that is understood to some degree. In other words, the samples must be, in some sense, representative of the cooling water flow in the entire channel cross-section at the sampling location. Our knowledge of the relationship of the sample to the entire cross-section flow can be vague, specific, or somewhere between these extremes.

When our knowledge of a process is vague, we can describe the process and its behavior only at a general level. Our understanding of the process is not complete, but partial. Consequently, this understanding is embodied in mild, general assumptions. For instance, when considering a collection of observations (of entrainment abundance or any other phenomenon) from a process about which little or nothing is known, we may decide to assume only that the observations are independent and identically distributed. We may also decide to assume that the common distribution of the observations has finite mean and variance. These assumptions are very general: they are satisfied by a wide range of distributions.

In contrast, specific knowledge of a process is a detailed description of the mechanisms and relationships involved. This is usually stated as a parametric model. For instance, we may decide to assume that organism counts in an entrainment process behave as independent observations from a Poisson distribution whose intensity parameter or mean is given by a specified function of plant and river conditions.

An example of the use of vague knowledge is the treatment of two-stage and three-stage sampling in Cochran (1977, Ch. 10). The only assumptions are that the p.s.u.'s selected for sampling, the samples selected within each p.s.u., and the splits selected in three-stage sampling within each sample are all chosen at random from the set of all available units of each type. No parametric assumptions are made about the distributions of the counts in these units, so the results of this analysis are broadly applicable. Nevertheless, this method has drawbacks that were pointed out in Sections 3.1 and 3.2, including its inability to cope with situations where  $m = 1$ . The importance of the assumption of randomness in this method must be emphasized - if it is false because of systematic spatial nonrandomness in the discharge channel, then this approach will not perform well in estimating entrainment abundance. Systematic spatial nonrandomness occurs when a departure from spatial randomness is present: an organism is not equally likely to pass the location of the sampling gear at any point in the discharge channel cross-section. Spatial randomness is described in Appendix I. For example, systematic spatial nonrandomness occurs when the ichthyoplankton count of a sample is influenced by the depth at which the sample is drawn. When spatial nonrandomness is present, the sample(s) selected within a p.s.u. cannot be viewed as chosen at random from all potential samples, unless the location of the sampling gear intake pipe is chosen at random from the discharge channel cross-section. With fixed intake pipe location, the question of whether the selected sample(s) constitute a random sample from among all potential samples must be raised.

Past studies investigating whether the distribution of organisms in the discharge channel exhibits spatial randomness have come to different conclusions. A study of net and AUTOSAM densities for various striped bass life stages at Indian Point concluded that no significant depth effect was present for any life stage (EA, 1981, p. 5-67). However, a gear comparability study of entrainment densities of juveniles at Indian Point two years later concluded that sampling site (that is, depth) had a significant effect for five of the seven taxa examined (Normandeau and Con Edison, 1982, p. 30).

Neither of these two studies addressed the question of whether horizontal differences exist in the distribution of ichthyoplankton in the cross-sectional space of the discharge canal. The study of juvenile entrainment raised this issue and stated that the hypothesis of no horizontal differences would be assumed in its analysis (Normandeau and Con Edison, 1982, p. 18-19).

The assumption of spatial randomness in the discharge channel organism distribution cannot be made without caution; as a recent Indian Point entrainment report noted in discussing differences among sampling gear (EA, 1984b, p. 4-26),

The cause . . . could possibly be related to flow patterns within the discharge canal of D2 which resulted in differences in water velocities at the intakes of the sampling hoses. Flow patterns at D2 are quite variable and change substantially with changes in pump operation at Units 2 and 3. Thus, the difference may be due to location rather than type of sample.

An example of specific knowledge is a parametric model for the distribution of ichthyoplankton in the discharge channel. For instance, the Poisson model for sample counts in the discharge flow has often been adopted. The assumptions of such a model are more narrow than those of the general approach; however, when these assumptions are satisfied, the model-based analysis will give more accurate results than the general approach. Correspondingly, inferences based on a model that does not actually hold can be extremely inaccurate, so checking a model's consistency with observed data is a critical necessity. The failure of the Poisson model to agree with generating station data will be treated and a more suitable model will be introduced in Chapter 4.

Assumption 3.1'. In this study, random sampling will be assumed: one or more simultaneous samples from different locations in the discharge channel will be considered to be a random sample from the entire set of pump or net volumes of cooling water passing by the sampling gear during the p.s.u. In other words, the total volume of cooling water that flows by the sampling gear during a p.s.u. can be viewed as a collection of smaller volumes called pump or net volumes. Each of these is equal in volume to the amount of water that is withdrawn from the discharge channel to become a sample, passing through the sampling gear. The total flow of cooling water during the p.s.u. is divisible into a set of nonoverlapping (mutually exclusive) pump or net volumes, one or a few of which are withdrawn to become replicate samples; the remaining pump or net volumes are potential samples that turned out to be unrealized, because they were not withdrawn from the discharge channel by the sampling gear. It will be assumed that the sample or replicate samples actually obtained in any p.s.u. are a random sample from the conceptual set of all pump or net volumes that are present in the discharge during the p.s.u.

Three reasons can be given for making this assumption of random sampling. First, it is the simplest assumption possible, so it is preferable to more complex assumptions if it can be supported. Second, the available data do not allow us to propose and support any alternative assumption. Third, it will be seen in Chapter 4 that the ichthyoplankton counts of replicate samples within a p.s.u. are consistent with the hypothesis that these samples are independent observations from a distribution in a particular parametric family, the negative binomial family, to be discussed in Section 4.2. Consequently, Assumption 3.1' will be made rather than Assumption 3.1. Note that Assumption 3.1 is so broad that it is unworkable; it is stated and discussed here to emphasize the fact that different approaches to entrainment modeling exist, but some restriction to a more specific formulation must occur before progress can be made.

Assumption 3.2. At any instant of time, the number of organisms entrained is directly proportional to the volume of cooling water flow through the plant. In other words, the density of organisms entrained at a plant is identical under both the actual and full plant operating conditions. This relationship holds for each life stage of each taxon at each instant of time. Of course, densities may differ across time and among different taxa and life stages.

This assumption is currently made in entrainment estimation at Bowline Point, Indian Point, and Roseton generating stations. Recently gathered data from Bowline Point and Roseton, to be discussed in Section 4.7, raise questions about this assumption, but the evidence is not strong enough to refute it. Should such a refutation occur in the future, a different model relating ichthyoplankton density to flow volume will be required.

Assumption 3.3. The ratio R of the volume of cooling water passing the sampling gear to the volume of water sampled is known for each p.s.u.

This ratio is the expansion factor that is multiplied by the sample organism count to obtain estimated plant entrainment counts for each p.s.u. In practice, when cooling water flow is known only on a daily (24-hour period) basis, the volume passing through the discharge channel during a given p.s.u. can be approximated by

$$\text{daily volume} / \text{number of p.s.u.'s in a day} \quad (3.1)$$

This approximation depends on the fact that the plant cooling water flow rate is nearly constant across a single day.

The accurate measurement of the volume of discharge channel flow in each p.s.u. would improve the accuracy of entrainment estimates. Without this information, the approximation (3.1) or another similar approximation must be used in place of the ratio R for each p.s.u. The critical nature of R in entrainment estimation makes it clear that errors induced by this approximation can have an adverse impact on the accuracy of estimation.

Assumption 3.4. The relationship in three-stage sampling between split counts and the count of the sample from which these splits are drawn can be modeled as a random process. More specifically, when a sample is divided into splits, each organism in the sample is distributed at random among the splits, and all organisms are assigned to splits independently.

The evidence pertaining to this assumption and its consequences are discussed in Section 7.0. If the splitting device is not capable of dividing a sample into splits in a way that satisfies Assumption 3.4, the efficiency of three-stage sampling can be seriously impaired.

There is a need for further experimental investigation of the validity of these assumptions. Based on currently available data and analyses, Assumptions 3.1', 3.2, and 3.3 appear to be necessary and warranted. Assumption 3.4 or some other assumption about the variance of counts among splits within a sample is necessary for three-stage sampling plans. Such an assumption does not seem warranted for splitting devices currently available.

## 4. ASSESSMENT OF DISTRIBUTIONAL INFORMATION

### 4.0 INTRODUCTION

In order to assess the efficacy of a sampling design, we must first obtain the form of the variance of the proposed estimators. The sampling design will affect this variance, and, by understanding the relationship between them, an optimal sampling design (in terms of getting the smallest variance for the least effort) can be obtained.

Estimates of entrainment abundance in the discharge channel are obtained by either a one-step or a two-step approach within each time period or p.s.u., depending upon whether subsampling is used. In the one-step case, one or more samples are obtained from the discharge channel during each selected p.s.u., and each sample is sorted and identified in its entirety. In the two-step case, one or more samples are obtained from the discharge channel during each selected p.s.u. Each sample is divided into a number of splits (or aliquots), and these splits are then sampled according to a sum-quota sampling rule: individual splits are randomly selected and their contents are identified, until the sum-quota rule being used specifies that no further splits must be identified. Sum-quota procedures are treated in Chapter 7 of this report.

The variation in the entrainment abundance estimates is composed of many parts, in particular

- (i) variation in the discharge channel,
- (ii) variation in the subsampling process, and
- (iii) variation in the classification process.

In the one-step case only (i) and (iii) apply, while (i), (ii), and (iii) all apply in the two-step case. In this chapter we are mainly concerned with variation in the discharge channel, which leads us to consider both (i) and (iii). Subsampling variation will be treated in Chapter 7.

### 4.1 THE POISSON MODEL

If it is assumed that the ichthyoplankton have a spatially random distribution in the discharge channel, then with a few additional assumptions, a theoretical model can be developed which shows that the variation in the organism counts of the samples from the discharge channel should follow a Poisson distribution. (See Appendix I for details, including a discussion of spatial randomness.) Specifically, if we define for a given p.s.u.

## 4.2

$\lambda$  = true mean density of ichthyoplankton during the p.s.u.,  
 $v$  = volume of water sampled during the p.s.u.,  
 $X$  = observed number of ichthyoplankton in the sample,

then  $X$  has a Poisson distribution with intensity or mean  $\lambda v$ :

$$P(X=x) = e^{-\lambda v} (\lambda v)^x / x! \quad \text{for } x=0,1,2,\dots, \quad (4.1)$$

that is,  $X \sim \text{Poisson}(\lambda v)$ .

This process may be observed over  $n$  p.s.u.'s, with different organism density  $\lambda$ , sample volume  $v$ , and observed ichthyoplankton count  $X$  occurring in each of these p.s.u.'s. It may also happen that two replicate samples, each of volume  $v$ , were drawn independently in each p.s.u. The Poisson model is easy to extend to this situation; the notation would be

$$X_i, Y_i \sim \text{independent Poisson}(\lambda_i v_i) \quad \text{for } i = 1, 2, \dots, n, \quad (4.2)$$

where the  $n$  pairs  $X_i, Y_i$  are independent of each other and

$\lambda_i$  = true mean density of ichthyoplankton during p.s.u.  $i$ ,

$v_i$  = volume of water constituting a sample taken during p.s.u.  $i$ ,

$X_i$  = observed number of organisms in the first sample taken during p.s.u.  $i$ ,

$Y_i$  = observed number of organisms in the second sample taken during p.s.u.  $i$ .

In order to test the assumption of Poissonness, data that were obtained through replicate sampling were analyzed. For example, Table 8-2 in the Bowline Point 1983 Annual Report (EA, 1984a) gives data that were simultaneously sampled. These data consist of 49 pairs of observations; in each pair the two replicate observations were obtained simultaneously. We therefore have 49 pairs  $(X_i, Y_i)$ ,  $i=1, \dots, 49$ , each of which is independent of the remaining 48 pairs. In addition, for each  $i$  the two components  $X_i$  and  $Y_i$  of the  $i^{\text{th}}$  pair are independent, and under the Poisson model they satisfy (4.2).

To test the Poisson model (4.2) for  $n$  independent pairs of replicate observations  $X_i, Y_i$ , we can use the test statistic

$$S_p = \sum_{i=1}^n (X_i - Y_i)^2 / (X_i + Y_i) \quad (4.3)$$

When the Poisson model holds,  $S_p \sim \chi_n^2$ . Large values of  $S_p$  lead to rejection of the null hypothesis  $H_0$ : the data are from the Poisson model (4.2). The derivation of  $S_p$  is given in Section 4.1.1.  $S_p$  is discussed further in Section 4.2.1.

For the 1983 Bowline Point data on bay anchovy larvae, we obtain  $S_p = 431.692$  ( $p < 10^{-7}$ ), which is extremely strong evidence against the validity of the Poisson model.

Similar simultaneous data were available from the Roseton 1982 Annual Report (EA, 1983). Since these data were obtained in half-hour samples, it was possible to composite them in order to test the Poisson assumption for different sample durations. Data were composited into two sets of hourly data, one compositing on the hour, and one on the half-hour. From these two composites, denoted Composite I and Composite II, two- and three-hour samples were obtained. In all cases, the Poisson model was rejected by the data. A summary of all chi-square tests for Poissonness is given in Table 4.1.

A detailed reporting of the Bowline Point analysis is given in Table 4.2, including the values of the chi-square statistic for each data pair. Each of these values should be approximately equal to 1 if the null hypothesis that the data are from a Poisson distribution is true. The fact that many of these pairs are quite large is important: it shows that the Poisson model is not being rejected because of only a few influential "bad" data points. (One extremely large pairwise value could induce rejection, which would make us suspect that rejection may be due to one aberrant data value rather than to non-Poissonness. This is not the case here.) Details of the Roseton 1982 analysis are quite similar to those of the Bowline Point 1983 analysis. Because of their lengthiness, they are not reported in full here.

#### 4.1.1 DERIVATION OF THE POISSON TEST STATISTIC

Given  $n$  pairs of replicate observations  $X_i, Y_i, i=1, \dots, n$ , the Poisson model (4.2) specifies that each pair consists of two random observations independently drawn from a Poisson distribution. The distribution's underlying mean is the same for the two observations in a pair, but may vary arbitrarily from one pair to another, and every pair of observations is independent of all other pairs.

The test statistic  $S_p$  of (4.3) will now be derived as a test statistic of the null hypothesis  $H_0$ : the paired data  $X_i, Y_i, i=1, \dots, n$ , are from the Poisson model just described. This derivation is inevitably technical in nature.

Table 4.1

Summary of  $\chi^2$  tests to evaluate the  
Poisson and negative binomial models

<u>Bowline Point 1983</u>					
Species	Life Stage	Poisson $\chi^2$	Significance* Level	Neg. bin $\chi^2$	Significance* Level
Bay Anchovy	Larvae	431.69	<10 <sup>-7</sup>	46.86	.51
<u>Roseton 1982, Composite I</u> <u>1 hour samples (65 pairs)</u>					
Clupeids	Larvae	273.76	<10 <sup>-7</sup>	65.98	.41
Alosa spp.	Larvae	205.25	<10 <sup>-7</sup>	93.02	.01
White perch	Eggs	108.39	.00059	68.70	.32
White perch	Yolk-sac larvae	188.13	<10 <sup>-7</sup>	72.30	.22
White perch	Larvae	111.64	.00029	78.36	.11
Striped bass	Yolk-sac larvae	144.80	<10 <sup>-7</sup>	73.73	.19
Striped bass	Larvae	132.08	<10 <sup>-7</sup>	65.95	.41
Morone spp.	Larvae	299.11	<10 <sup>-7</sup>	75.17	.16
<u>2 hour samples (32 pairs)</u>					
Clupeids	Larvae	188.57	<10 <sup>-7</sup>	40.79	.11
Alosa spp.	Larvae	115.19	<10 <sup>-7</sup>	66.83	.00020
White perch	Eggs	41.52	.12	28.68	.58
White perch	Yolk-sac larvae	120.38	<10 <sup>-7</sup>	34.85	.29
White perch	Larvae	67.14	.00027	43.30	.070
Striped bass	Yolk-sac larvae	77.63	.000012	31.61	.44
Striped bass	Larvae	68.74	.00017	41.83	.093
Morone spp.	Larvae	262.95	<10 <sup>-7</sup>	34.35	.31
<u>3 hour samples (20 pairs)</u>					
Clupeids	Larvae	170.37	<10 <sup>-7</sup>	29.15	.064
Alosa spp.	Larvae	64.86	.0000012	40.85	.0025
White perch	Eggs	23.63	.26	16.79	.60
White perch	Yolk-sac larvae	121.08	<10 <sup>-7</sup>	27.70	.089
White perch	Larvae	32.02	.043	22.85	.24
Striped bass	Yolk-sac larvae	78.22	<10 <sup>-7</sup>	23.95	.20
Striped bass	Larvae	52.12	.00011	24.21	.19
Morone spp.	Larvae	255.98	<10 <sup>-7</sup>	19.84	.40

\*The significance level (also known as the p-value) indicates the amount of evidence against the null hypothesis. In the cases considered here, small values of the significance level are evidence against the fit of the model: the smaller the value, the more evidence against the fit of the model.

Table 4.1 continued

<u>Roseton 1982, Composite II</u>					
<u>1 hour samples (61 pairs)</u>					
Species	Life Stage	Poisson $\chi^2$	Significance* Level	Neg.bin $\chi^2$	Significance* Level
Clupeids	Larvae	229.61	<10 <sup>-7</sup>	61.45	.42
Alosa spp.	Larvae	282.62	<10 <sup>-7</sup>	88.86	.011
White perch	Eggs	78.06	.069	56.12	.62
White perch	Yolk-sac larvae	159.75	<10 <sup>-7</sup>	67.92	.23
White perch	Larvae	126.00	<10 <sup>-5</sup>	66.49	.26
Striped bass	Yolk-sac larvae	138.39	<10 <sup>-7</sup>	75.03	.092
Striped bass	Larvae	112.55	<10 <sup>-4</sup>	69.10	.20
Morone spp.	Larvae	286.38	<10 <sup>-7</sup>	70.32	.17
<u>2 hour samples (28 pairs)</u>					
Clupeids	Larvae	161.93	<10 <sup>-7</sup>	30.14	.31
Alosa spp.	Larvae	140.62	<10 <sup>-7</sup>	30.55	.29
White perch	Eggs	39.71	.071	31.59	.25
White perch	Yolk-sac larvae	114.78	<10 <sup>-7</sup>	32.05	.32
White perch	Larvae	50.31	.0059	32.62	.21
Striped bass	Yolk-sac larvae	86.71	<10 <sup>-7</sup>	28.80	.37
Striped bass	Larvae	60.17	.00038	33.86	.17
Morone spp.	Larvae	255.58	<10 <sup>-7</sup>	30.59	.29
<u>3 hour samples (18 pairs)</u>					
Clupeids	Larvae	132.21	<10 <sup>-7</sup>	21.33	.21
Alosa spp.	Larvae	83.95	<10 <sup>-7</sup>	30.71	.022
White perch	Eggs	25.91	.10	16.22	.51
White perch	Yolk-sac larvae	115.50	<10 <sup>-7</sup>	20.38	.26
White perch	Larvae	47.84	.00016	24.98	.095
Striped bass	Yolk-sac larvae	69.61	<10 <sup>-7</sup>	21.67	.20
Striped bass	Larvae	48.20	.00014	24.65	.10
Morone spp.	Larvae	232.64	<10 <sup>-7</sup>	18.05	.39

\*The significance level (also known as the p-value) indicates the amount of evidence against the null hypothesis. In the cases considered here, small values of the significance level are evidence against the fit of the model: the smaller the value, the more evidence against the fit of the model.

Table 4.2

Contributions to Model  $\chi^2$ 's from each Sample  
Bowline Point 1983 Table B2 Data

Sample	Larvae Count		Mean	Variance	Contribution	Contribution
	Obs 1	Obs 2			to Poisson $\chi^2$	to Negative Binomial $\chi^2$
1	158	147	152.5	60.5	.39	.01
2	143	72	107.5	2520.5	23.44	1.49
3	106	71	88.5	612.5	6.92	.52
4	57	74	65.5	144.5	2.20	.22
5	97	141	119.0	968.0	8.13	.47
6	80	85	82.5	12.5	.15	.01
7	109	119	114.0	50.0	.43	.02
8	109	96	102.5	84.5	.82	.05
9	350	142	246.0	21632.0	87.93	2.53
10	224	175	199.5	1200.5	6.01	.21
11	109	137	123.0	392.0	3.18	.17
12	214	80	147.0	8978.0	61.07	2.89
13	84	45	64.5	760.5	11.79	1.20
14	37	9	23.0	392.0	17.04	4.17
15	67	24	45.5	924.5	20.31	2.83
16	46	24	35.0	242.0	6.91	1.20
17	44	38	41.0	18.0	.43	.06
18	118	142	130.0	288.0	2.21	.11
19	30	48	39.0	162.0	4.15	.66
20	116	93	104.5	264.5	2.53	.16
21	137	59	98.0	3042.0	31.04	2.15
22	61	50	55.5	60.5	1.09	.12
23	49	76	62.5	364.5	5.83	.61
24	37	9	23.0	392.0	17.04	4.17
25	91	68	79.5	264.5	3.32	.28
26	55	99	77.0	968.0	12.57	1.09
27	91	63	77.0	392.0	5.09	.44
28	50	38	44.0	72.0	1.63	.23
29	63	69	66.0	18.0	.27	.02
30	28	34	31.0	18.0	.58	.11
31	41	38	39.5	4.5	.11	.01
32	44	26	35.0	162.0	4.62	.80
33	53	54	53.5	.5	.00	.00
34	46	13	29.5	544.5	18.45	3.70
35	72	100	86.0	392.0	4.55	.35
36	24	36	30.0	72.0	2.40	.47
37	19	8	13.5	60.5	4.48	1.61
38	32	25	28.5	24.5	.85	.17
39	29	34	31.5	12.5	.39	.07
40	13	31	22.0	162.0	7.36	1.86
41	8	20	14.0	72.0	5.14	1.80
42	12	43	27.5	480.5	17.47	3.71
43	16	24	20.0	32.0	1.60	.43
44	20	17	18.5	4.5	.24	.07
45	14	13	13.5	.5	.03	.01
46	17	10	13.5	24.5	1.81	.65
47	22	28	25.0	18.0	.72	.16
48	18	15	16.5	4.5	.27	.08
49	23	60	41.5	684.5	16.49	2.48

In the case of paired samples, the Poisson assumption may be tested either by an exact binomial test or by the chi-square approximation to the exact distribution of the Poisson variance test statistic. Unequal measured sample volumes, say  $v_i$  for the count  $X_i$  and  $v_i'$  for the count  $Y_i$ , with  $v_i \neq v_i'$ , can also be accommodated by these test procedures. For a given total  $t_i = X_i + Y_i$  the conditional probability distribution of  $(X_i, Y_i)$  is binomial,

$$P(X_i=x, Y_i=y | X_i+Y_i=t_i) = (t_i! / x_i! y_i!) [v_i / (v_i + v_i')]^x [v_i' / (v_i + v_i')]^y, \quad (4.4)$$

which reduces to the simple form

$$P(X_i=x, Y_i=t_i-x | X_i+Y_i=t_i) = \binom{t_i}{x} (1/2)^{t_i} \quad (4.5)$$

when  $v_i = v_i'$ . Letting

$$p_i = v_i / (v_i + v_i') \quad (4.6)$$

$$\hat{p}_i = X_i / (X_i + Y_i) = X_i / t_i,$$

we may then test for a significant discrepancy between the observed  $\hat{p}_i$  and the expected  $p_i$ .

The exact test could be implemented by entering a table of binomial confidence limits or Clopper-Pearson charts (Clopper and Pearson, 1934) to check whether the expected  $p_i$  is contained within the confidence limits determined by the observed  $\hat{p}_i$  and the sample size  $t_i$ . Alternatively, it is a straightforward matter to compute the exact probability of obtaining a binomial sample as discrepant as the one in hand. Measuring this discrepancy by the absolute difference between observed and expected values,

$$d_i = \text{discrepancy} = |X_i - t_i p_i| = |Y_i - t_i(1 - p_i)|, \quad (4.7)$$

and letting

$$x^* = t_i p_i - d_i \text{ (rounded downward to an integer)} \quad (4.8)$$

$$x^{**} = t_i p_i + d_i \text{ (rounded upward to an integer),}$$

then the significance level  $\alpha_i$  of this discrepancy is

$$\alpha_i = \sum_{x=0}^{x^*} \text{Bin}(x; t_i, p_i) + \sum_{x=x^{**}}^{t_i} \text{Bin}(x; t_i, p_i) \quad (4.9)$$

where

$$\text{Bin}(x; t_i, p_i) = [t_i! / x!(t_i - x)!] p_i^x (1 - p_i)^{t_i - x} \quad (4.10)$$

In the special case of equal volumes (where  $v_i = v_i'$ , so  $p_i = 1/2$ ) no rounding is needed to produce the lower and upper integers  $x^*$  and  $x^{**}$ , and the lower and upper tail probabilities in (4.9) are equal, so (4.9) reduces to the simpler expression for  $p_i = 1/2$

$$\alpha_i = (1/2)^{t_i - 1} \sum_{x=0}^{x^*} t_i! / x!(t_i - x)! \quad (4.11)$$

As an example of this latter calculation, consider paired sample 37 in Table 4.2 where  $X_{37} = 19$ ,  $Y_{37} = 8$ , so  $t_{37} = 27$ . Assuming equal sample volumes ( $p_{37} = 1/2$ ),

$$d_{37} = |19 - 13.5| = |8 - 13.5| = 5.5$$

$$x^* = 13.5 - 5.5 = 8$$

$$x^{**} = 13.5 + 5.5 = 19$$

$$\alpha_{37} = (1/2)^{26} \left[ \binom{27}{0} + \binom{27}{1} + \binom{27}{2} + \dots + \binom{27}{8} \right]$$

$$\begin{aligned}
 &= (1 + 27 + 351 + 2925 + \dots + 2,220,075)/2^{26} \\
 &= 3,505,699/2^{26} \\
 &= .052239
 \end{aligned}$$

Such calculations become subject to computational inaccuracies as  $X_i + Y_i = t_i$  becomes large, but the normal approximation to the binomial then becomes applicable (see Snedecor and Cochran, 1983, p. 120), giving the normal test statistic  $z_c$ , corrected for continuity,

$$\begin{aligned}
 z_c &= (d_i - .5) / \sqrt{t_i p_i (1-p_i)} \Big|_{p_i=.5} \\
 &= (|X_i - Y_i| - 1) / \sqrt{X_i + Y_i}.
 \end{aligned} \tag{4.12}$$

The square of  $z_c$  is approximately distributed under the null hypothesis as chi-square with one degree of freedom. In the above numerical example with  $p_i = .5$ , this gives

$$z_c = (|19 - 8| - 1) / \sqrt{27} = 1.9245,$$

which corresponds to a P-value of .054292 for a two-tailed normal test, as compared to the exact two-tailed probability .052239 calculated above.

For large  $X_i + Y_i$  the continuity correction has a negligible effect; the square of the uncorrected normal test statistic,

$$\begin{aligned}
 z^2 &= (X_i - t_i p_i)^2 / t_i p_i (1-p_i) \Big|_{p_i=.5} \\
 &= (X_i - Y_i)^2 / (X_i + Y_i),
 \end{aligned} \tag{4.13}$$

is approximately distributed as a chi-square with one degree of freedom. This chi-square analysis can be done independently for each of the  $n$  paired samples  $X_i, Y_i$ , and summing the  $n$  resulting test statistics gives

$$S_p = \sum_{i=1}^n (X_i - Y_i)^2 / (X_i + Y_i),$$

the statistic of (4.3), whose distribution under the null hypothesis is chi-square with  $n$  degrees of freedom.

For small  $X_i + Y_i$  the exact test given by (4.9) involves a small number of atoms of probability, each of relatively large mass. As a result, the test is not very powerful; it is capable of detecting only extreme departures from the null hypothesis.

The derivation of  $S_p$  is supported by the following intuitive view. If the pairs  $(X_i, Y_i)$  satisfy the Poisson model null hypothesis, then

$$E[(X_i + Y_i)/2] = (\lambda_i v_i + \lambda_i v_i)/2 = \lambda_i v_i$$

$$E[(X_i - Y_i)^2/2] = (\text{var}(X_i) + \text{var}(Y_i))/2 = \lambda_i v_i.$$

Thus, under the null hypothesis, we expect  $(X_i - Y_i)^2 / (X_i + Y_i)$  to be approximately equal to 1, which is the mean of the chi-square distribution with one degree of freedom. This is based on the approximation  $E[f(X,Y)/g(X,Y)] \cong E[f(X,Y)]/E[g(X,Y)]$  for functions  $f$  and  $g$  of the pair  $(X,Y)$ , which comes from the first-order Taylor series expansion of  $f/g$  about the point  $(E_f, E_g)$ . This expansion is sufficient for the informal nature of the present discussion.

## 4.2 THE NEGATIVE BINOMIAL MODEL

The rejection of the Poisson model in the discharge channel leads us to search for an alternative model. A richer class of models, which includes the Poisson model as a special case, is that based on the negative binomial distribution. This is a reasonable place to look for alternatives since it incorporates a set of assumptions similar to those of the Poisson, but allowing for a wider range of variability.

### 4.2.1 THE RATIONALE FOR THE NEGATIVE BINOMIAL MODEL

Cassie points out that samples from natural populations often exhibit overdispersion, which occurs when the variance in samples from a population exceeds the mean ( $\sigma^2 > \mu$ ): "Overdispersion is so common in natural populations that it will inevitably be one of the most important features to be observed in the planning and execution of any sampling program." (Edmondson and Winberg, 1971, p. 185). Noting that various mathematical models for overdispersion have been proposed, Cassie says

The general features of most models can be described approximately as follows: If a number of different

sets of samples are taken, each set representing the same natural population, but employing a different sample size, the relationship between the means and variances of the various sets will approximate to:

$$\sigma_i^2 = \mu_i + c\mu_i^2 \quad (*)$$

where  $c$  is a constant characteristic of the population and the subscript  $i$  refers to the  $i$ th set. The right-hand side of the equation may be regarded as having two components, the Poisson variance,  $\mu$ , arising from random variation, and the overdispersion variance,  $c\mu^2$ . It is difficult to generalize about the likely value of  $c$ . In natural plankton populations, probably the lowest value which can be hoped for is about 0.05, while the modal value may be 0.1 to 0.2 (Cassie, 1963). However, considerably higher values are not uncommon . . . . If the mean is small (say  $<1$ ) we find that the second-order term,  $c\mu^2$ , in (\*) is negligible and that the distribution approximates to Poisson. For this reason we should not be surprised if rare species show no departure from random distribution. On the other hand, when the mean is large (say  $>40$ ) the first-order term,  $\mu$ , is negligible and:

$$\sigma^2 \approx c\mu^2.$$

Cassie's advocacy of the quadratic variance model (\*) does not specifically mention the negative binomial distribution, but several considerations make this a logical choice for a model of overdispersion: (1) The negative binomial model is a generalization of the Poisson model, which is the standard model for random distribution; that is, the Poisson family is a special case of the negative binomial family of distributions. (2) The negative binomial model is the most commonly used model for overdispersion. It is a very broad, general class of distributions. (3) The negative binomial model satisfies the quadratic variance relationship of equation (\*). It is one of the few distributions to do so. (4) The negative binomial model is analytically tractable, allowing a wide range of theoretical and applied calculations. (5) The negative binomial model can be viewed as a mixture of Poisson distributions, in which a Poisson observation is drawn after the Poisson intensity parameter, the mean density  $\lambda$  of ichthyoplankton, is determined as a random observation from a gamma distribution; it is not necessary to view the negative binomial model as originating in this fashion, but this approach may be helpful to some, so we include it for completeness. We will discuss these points in greater detail after presenting an alternative argument, based on the nature of the entrainment process, in support of the quadratic variance model. This argument is directly related to the structure of entrainment sampling.

Ichthyoplankton organisms of a given species and life stage exhibit temporal peaks and troughs in their abundance. Furthermore, at a given point in time, these organisms are not, in general, uniformly distributed through the river's cross-section at the intake location: the abundance differs at various points in the water column (that is, at different depths) and at various cross-channel (that is, horizontal) positions in the river. In this sense, the ichthyoplankton entering the intake are spatially clumped, i.e., they exhibit spatial gradients. As the volume of water that on discharge will constitute a p.s.u. passes through the cooling system, the spatial pattern of ichthyoplankton densities is disrupted and fragmented; during the water's passage through the plant, it is divided into subvolumes that may be described as long, relatively thin threads, which correspond to the parallel tubes through which the subvolumes pass. Each thread originates from a specific cross-sectional location in the spatial density pattern that enters the intake; consequently, the density of organisms will differ among simultaneously formed threads. After the fragmentation of the intake spatial pattern of densities into threads, the plant cooling is performed, and then a tumultuous reunion of the threads takes place. This reunion might produce a cross-sectional mixture in the discharge channel that largely preserves the temporal distribution but reconstitutes the spatial distribution as a mixture of thread fragments. After passage through the plant, the density of organisms within each thread fragment may be expected to be virtually random; however, when the threads recombine in the discharge channel, each of them is located near new neighbors from different sectors of the original spatial pattern.

Only complete cross-sectional mixing, of the kind that occurs when there is total homogenization, would induce Poisson variation between replicate (simultaneous) samples from a p.s.u. in the discharge flow. The cross-sectional mixing resulting from the division into the threads just described is only incomplete, though: each thread, formed by a segment of the original spatial pattern in the intake, retains its integrity as it passes through the cooling system, because mixing takes place within each thread but not among different threads. This incomplete mixing leads to partial preservation of the incoming spatial heterogeneity. Such cross-sectional heterogeneity in the discharge would necessarily result in greater than Poisson variation among simultaneous replicate samples from the same p.s.u.

The physical mixing process of separation into threads followed by reunion in the discharge is a steady state stochastic process acting in common on all ichthyoplankton. The effect on individual species and life stages, though, would depend on the nature of their spatial pattern of density in the intake. Juveniles, for example, might be highly dispersed relative to the size of the thread fragments of water entering the discharge channel, because juvenile densities are low. Consequently, the tumultuous mixing in the discharge would effectively constitute homogenization, resulting in a Poisson distribution in spatial cross-section, i.e., spatial randomness in distribution. For the smaller and more abundant life forms, however, the mixing might be incomplete, since neighbors within a small fragment of water mass remain neighbors in the discharge. This clumping or incomplete mixing generates a cross-sectional spatial distribution that must be more variable than a random (Poisson) spatial distribution.

Within a small thread fragment of a p.s.u., the spatial distribution of an organism should be essentially random, but the density of organisms within the thread depends upon where the thread originated in the intake pattern. A collected sample is a cluster of such thread fragments yielding a sum of different Poisson counts, each of which is subject to Poisson variance. The variance between replicate samples, however, also depends on the between-cluster component of variance among such composites of Poisson density parameters (these parameters are the true underlying Poisson mean densities that the sample counts are used to estimate). This second, between-cluster component of variance depends on the magnitude of the density gradients, or differences in density from one location to another, in the intake spatial pattern for the organism in question. The component is also affected by the physical nature of the mixing process that fragmentizes and mixes the threads of cooling water independently of the organisms' presence.

If this process does produce a random mixture of fragments of the original spatial pattern (a necessary condition for unbiasedness of a fixed-position sampler), then this second component of variance constitutes a quadratic term in the variance between replicate samples. This component can be expressed as the product of the squared mean and the relative variance among randomly formed composites of fragments of combined volume  $v$  (the sample volume). The coefficient of the quadratic term in the variance corresponds to the parameter  $K$  of a negative binomial distribution.

This quadratic coefficient would now be representing a sum of three terms expressing the variance of a quota-sum of products of two independent random variables. One variable is the volume of a fragment, representing the sum-quota variable itself, and the other is the Poisson density parameter of that fragment. The three terms in such a sum-quota (sum-quota volume equals  $v$ ) relative variance comprise, first, the relative variance fragment volume; second, the spatial relative variance of density; and third, the product of these two relative variances. At a constant flow rate the relative variance of fragment size would remain constant through time. If spatial gradients in density are scaled by the overall density (mean density) of the organism at a given time, then spatial relative variance would likewise remain constant over time. Each kind of organism would then exhibit a quadratic variance function with a quadratic coefficient indexing the degree of clumping in the spatial distribution of that organism, as modified by the plant mixing process.

Either Cassie's general approach to sampling natural populations or the entrainment-based approach just given leads us to consider a model with a quadratic variance function. The negative binomial model is an obvious candidate for the reasons listed earlier in this section. We now discuss these points.

One way in which the negative binomial can be thought of as a generalization of the Poisson is through the variance function. If a random variable  $X$  is observed with mean  $\mu$ , then we would have

$\text{Var}(X) = \mu$  if  $X$  is Poisson

$\text{Var}(X) = \mu + (1/K)\mu^2$  if  $X$  is negative binomial,

where  $K$  is an additional free parameter. The value of  $K$  in this model, which must be positive, is constant for each species-life stage combination; it is not altered by changes in sample volume, organism density, etc. from one p.s.u. to another. These equations illustrate that the negative binomial model contains the Poisson (by taking  $K=\infty$ ), but also allows a much wider range of alternatives. In fact, the negative binomial model approaches the Poisson model as  $K \rightarrow \infty$ , not merely in the value of  $\text{Var}(X)$  but in the probability of any specified outcome. In other words, the negative binomial model approaches the Poisson model in distribution as  $K \rightarrow \infty$ . Thus the Poisson model is the subfamily of the negative binomial model obtained by setting  $K=\infty$ .

There are several probability models that have greater variance than the Poisson model, but the class of negative binomial models is the most commonly used one. A full treatment of the Poisson, the negative binomial, and related distributions can be found in Johnson and Kotz (1969, Chapters 4, 5, 8, 9); only a brief discussion will be given here. Historically, the negative binomial has long been used as a generalization of the Poisson (Anscombe, 1949; Bliss, 1953; Fisher, 1953) in situations characterized by dispersion or variability too great to be compatible with the Poisson model. Johnson and Kotz (1969, p. 125) point out the desirability of the negative binomial model "as alternative when it is felt that a Poisson distribution might be inadequate." The class of negative binomial distributions is a very rich class, that is, the distributions in this class cover a broad range of alternatives, so its use is quite nonrestrictive.

An interesting and relevant recent study of distributions whose variance is at most a quadratic function of the mean (Morris, 1982) lends further support to the choice of the negative binomial. Morris showed that if we limit consideration to distributions in the exponential class, an extremely broad category that includes most standard distributions and lies at the heart of modern statistical theory, there are only six classes of distributions whose variance is at most a quadratic function of the mean. Of these, three are continuous (the normal and gamma classes are among these), and one is bounded (the binomial class), making them unsuitable for modeling the counts in an entrainment situation. The two remaining classes with at most quadratic variance are the Poisson, whose variance is a linear function of the mean (in fact, the variance equals the mean itself), and the negative binomial. Morris demonstrated that the quadratic variance relationship is a pivotal property that greatly simplifies statistical analysis of these six classes of distributions. This analysis, combined with Cassie's treatment of overdispersion, provides a strong case for the negative binomial distribution.

Another aspect of the negative binomial that weighs in its favor is its analytical tractability. The negative binomial distribution allows a wide range of theoretical and applied calculations. Many of these are discussed in Johnson and Kotz (1969).

A recent article by Collings and Margolin (1985) examines tests for detecting negative binomial departures from the Poisson model. The authors caution against

the propensity of some investigators to assume Poisson sampling without any attempt at empirical verification of this assumption in situations where such verification with replicate observations is feasible at little or no additional cost.

They observe that the possible consequences of hyper-Poisson variability, or variation among observations that is greater than would be consistent with the Poisson model, include inefficient estimation and underestimation of the variances of parameter estimates.

Collings and Margolin point out that

the negative binomial plays the role of an attractive close alternative to the Poisson. As numerous authors suggest . . . . a reasonable test of fit against all close alternatives may be obtained by deriving the best test against a particular close alternative. The detection of a departure from Poisson sampling is not an end in itself; one is then faced with providing a scientifically credible alternative sampling model. . . . The negative binomial plays a prominent role here as well, possibly because of its interpretability as a gamma mixture of Poissons. Authors as early as Student (1907) and Greenwood and Yule (1920) have advocated its use for modeling data that noticeably depart from Poisson sampling.

Concerning the relationship between the quadratic variance model and the negative binomial distribution, they state

The negative binomial distribution is particularly attractive both as a testing alternative to the Poisson distribution and as a tractable sampling model when there is reason to expect that the variance  $\sigma^2(x)$  and the mean  $\mu(x)$  functions are related quadratically by

$$\sigma^2(x) = \mu(x) + c^2 \mu^2(x), \quad (*)$$

where  $x$  denotes possible covariates and  $c^2$  is constant with respect to  $x$ . The evidence for (\*) at times may be empirical . . . . Alternatively, the evidence for (\*) at times may be methodological.

Collings and Margolin cite a statistic employed by Gart (1964) to test for departures from the Poisson model when the data constitute a one-way layout. This situation is a more general version of replicate sampling, in which an arbitrary number of replicate observations is taken during each of  $n$  p.s.u.'s; the number of replicates may differ from one p.s.u. to another. Gart's statistic is a suitable modification of the standard "variance" or "index of dispersion" test for departure from the Poisson model in the random sample case (independent, identically distributed observations). Our test statistic  $S_p$  in (4.3) turns out to be equivalent to Gart's statistic.

We now give the standard derivation of the negative binomial distribution as a mixture of Poisson distributions. This is included for mathematical completeness. It may be helpful in visualizing the entrainment process; however, this mixture model will not be used either to derive or to justify any results in the rest of this report. The negative binomial model follows from the same assumptions as the Poisson model except that a different supposition is made concerning the Poisson parameter  $\lambda$ . In the Poisson model,  $\lambda$  (the mean density of ichthyoplankton) is assumed constant. In the negative binomial model,  $\lambda$  is allowed to vary according to a Gamma distribution with parameters  $K$  and  $\beta$ . The Gamma ( $K, \beta$ ) distribution is given by

$$f(\lambda) = \frac{\beta^K}{\Gamma(K)} \lambda^{K-1} e^{-\beta\lambda} \quad \text{for } \lambda > 0. \quad (4.14)$$

The parameters  $K$  and  $\beta$  are referred to as the shape parameter and scale parameter, respectively, of the Gamma distribution. For different values of  $K$  and  $\beta$ , the Gamma distribution can take on a wide variety of forms. The scale parameter,  $\beta$ , can be thought of as a variance, and controls the dispersion of the distribution. The shape parameter, as its name implies, controls the shape of the distribution, allowing it to change from strictly decreasing to unimodal, with varying degrees of skewness.

Thus, if a volume,  $v$ , of water is sampled with mean density  $\lambda$ , where  $\lambda$  varies according to (4.14), then the observed number of ichthyoplankton follows a negative binomial distribution with parameters  $K$  and  $\beta/(\beta+v)$ . That is,

$$P(X=x) = \binom{K+x-1}{x} \left(\frac{\beta}{\beta+v}\right)^K \left(\frac{v}{\beta+v}\right)^x \quad \text{for } x=0,1,2,\dots \quad (4.15)$$

(See Appendix II for details.) To see that this is, in fact, a generalization of the Poisson model, we note that if  $K \rightarrow \infty$ ,  $\beta \rightarrow \infty$ , and  $K/\beta \rightarrow \text{constant}$  in (4.14), then (4.15) becomes the Poisson distribution.

The Poisson-Poisson (or Neyman Type A) distribution has been suggested as an alternative model, which could be used rather than the negative binomial. The main point in this model's favor is a biological interpretation that seems plausible: the Poisson-Poisson distribution with parameters  $\lambda$  and  $\phi$  results from the assumptions:

- (1) female fish lay egg clusters that are distributed at random among the subunits within a p.s.u., so the number of clusters in a given subunit is a Poisson variable with mean  $\lambda$ ; and
- (2) the number of eggs surviving (or larvae hatching, etc. for other life stages) from each cluster is also a Poisson variable, with mean  $\phi$ .

The negative binomial distribution can be derived by the exact same line of reasoning, with (1) remaining unchanged but (2) replaced by

- (3) the number of eggs surviving (or larvae hatching, etc.) from each cluster is a random variable with the logarithmic distribution (Johnson and Kotz, 1969, Ch. 7 or Pielou, 1977, p. 119).

This type of distribution, which is called a generalized distribution, "arises if we suppose that groups or clusters of individuals (rather than single individuals) constitute the entities having a specified pattern, and that the number of individuals per group is a random variate with its own probability distribution." (Pielou, 1977, p. 118). In contrast, another type of distribution, known as a compound distribution, arises if we assume that the individual organisms are independent of each other (not clustered) and that subunits available to them are not identical (which would lead to a Poisson distribution of organisms) but dissimilar, with some having more favorable environments than others, so the parameter  $\mu$ , the expected number of organisms in a subunit, varies from one subunit to another. This approach, in which  $\mu$  is a random variable, is used to derive the negative binomial distribution in Appendix II.

The difference between these two approaches to combining distributions is more illusory than real, because every compound Poisson distribution corresponds to a generalized Poisson distribution and vice versa; this implies that we cannot reach conclusions about the underlying mechanism from data on the observed distribution of organisms per subunit (Pielou, 1977, p. 123). For example, the Poisson-Poisson distribution corresponds to a compound Poisson distribution, whose interpretation would be very different, and whose distribution of observed outcomes would be identical to that of the Poisson-Poisson.

The Poisson-Poisson has several unfavorable properties that cast doubt on its suitability for this entrainment work. For more detail on these, see Johnson and Kotz (1969, Ch. 9). It is extremely multimodal, that is, able to have arbitrarily many modes, making it difficult to deal with in practice. This excessive flexibility is a sharp contrast to the moment behavior of the Poisson-Poisson: the third and fourth standardized moments are connected by the near-constant behavior of the expression  $(\beta_2 - 3)/\beta_1$ , which always remains between 1 and 1.215. As Johnson and Kotz

note, "the narrow limits of the ratio restrict the field of applicability of the distribution." (1969, p. 219).

Estimation of the Poisson-Poisson parameters is difficult. Equations for the maximum likelihood estimators can be found, but solving them requires involved iterative computation. Method of moments estimators can be obtained, as can other estimators based on the sample mean, the proportion of 0's, the ratio of observed 1's to 0's, and combinations of these approaches. The choice among these methods is complicated by the fact that the relative performances of these estimators are influenced by the parameter values: as the parameters change, the relative performances of these estimators change.

In applied work, the Poisson-Poisson distribution has been used primarily in situations involving plants and plant distributions. Evans (1953) found that it did well for plant distributions, but that the negative binomial distribution did better for insect distributions. Generalizations of the Poisson-Poisson distribution to families with three and four parameters instead of two have been proposed. These generalizations alleviate the "severe restriction on the shape of distribution implied by the limits on the value of  $(\beta_2-3)/\beta_1$ " (Johnson and Kotz, 1969, p. 226) but introduce new difficulties because of the extra one or two parameters to be estimated.

A final issue is the applicability of the Poisson-Poisson distribution to the replicate sampling situation of the entrainment studies. Data are collected in the form of two, or at most three, simultaneous observations. We seek to estimate the parameters of the distribution for each p.s.u. observed, in a way that provides overall information about parameters that span the entire entrainment season. This is difficult to accomplish for the negative binomial distribution; it seems outside the realm of possibility to extend these methods, which rely on certain special properties and the simplicity of structure of the negative binomial, to the Poisson-Poisson. The intractability of the Poisson-Poisson model appears to preclude its use in this context.

#### 4.2.2 TESTING FOR THE NEGATIVE BINOMIAL MODEL

As with the Poisson model of Section 4.1, replicate data are needed to test whether observations within a p.s.u. follow the negative binomial distribution. Each observation is a sample ichthyoplankton count. It is hypothesized that observations within p.s.u.  $i$  are independent, each satisfying the relationship

$$\sigma_i^2 = \mu_i + (1/K)\mu_i^2 \quad (4.16)$$

for each p.s.u.  $i$ , where  $K$  is constant for each species-life stage combination. P.s.u.'s in which a single sample count is taken will not help in this test, because the mean  $\mu_i$  varies from one p.s.u. to another.

To test the appropriateness of the negative binomial model, a chi-square statistic can again be used. If there are  $n$   $(X_i, Y_i)$  pairs, define the statistic

$$S_{NB} = \sum_{i=1}^n \frac{(X_i - Y_i)^2}{(X_i + Y_i)} \left[ \frac{2\hat{K}^+ + 1}{2\hat{K}^+ + X_i + Y_i} \right], \quad (4.17)$$

where

$$\hat{K}^+ = 2 \sum_{i=1}^n X_i Y_i / \sum_{i=1}^n [(X_i - Y_i)^2 - (X_i + Y_i)]^+, \quad (4.18)$$

and "+" signifies positive part, i.e.,  $(x)^+ = \max(x, 0)$ .

The statistic  $S_{NB}$  can be used to test the tenability of the negative binomial model, where large values of  $S_{NB}$  lead to rejection of the negative binomial model. A derivation similar to, though somewhat more complicated than, the derivation of  $S_p$  (the Poisson test statistic) shows that  $S_{NB}$  has an approximate chi-square distribution with  $n-1$  degrees of freedom. The results of the negative binomial model evaluation are given in Tables 4.1 and 4.2. In all cases, the fit of the negative binomial model is a substantial improvement over that of the Poisson model, and in all but a very few cases it provides a very good fit to the data.

The statistic  $\hat{K}^+$  is an estimate of the parameter  $K$  appearing in the negative binomial variance function of (4.16),

$$\text{Var}(X) = \mu + (1/K)\mu^2.$$

The  $n$  pairs  $(X_1, Y_1), \dots, (X_n, Y_n)$  give rise to a single value of the statistic  $\hat{K}^+$ . The statistic is derived by using a method of moments approach. The positive part is used in the denominator, rather than the absolute value or something similar, for the express purpose of eliminating the negative contributions. The quantity  $[(X_i - Y_i)^2 - (X_i + Y_i)]$ , of which the

positive part is taken, is itself an estimate of Variance - Mean, which satisfies the inequality

$$\text{Variance} - \text{Mean} \geq 0$$

in all of the models considered. Therefore negative values of this quantity are outside the allowable range, and any reasonable estimation procedure will decree that the estimator must be pulled back into the proper, non-negative range.

To illustrate further the effectiveness of the negative binomial model, a bootstrap technique was employed. A bootstrap analysis addresses the following question: given that the model assumptions are correct, what should samples from the process look like? Does the sample actually observed appear to be a typical sample from this process? To answer these questions, samples are computer-generated according to the hypothesized model and the behavior of these samples is compared with the actual data.

Under the negative binomial model, we now ask how the  $(X_i, Y_i)$  pairs should behave. Standard statistical calculations (Appendix III) show that the distribution of  $X_i$ , conditional on the observed total of  $X_i + Y_i$ , follows a beta-binomial (or inverse hypergeometric) distribution. That is,

$$P(X_i = x | X_i + Y_i = t) = \frac{\binom{K+x-1}{x} \binom{K+t-x-1}{t-x}}{\binom{2K+t-1}{t}} \quad (4.19)$$

The Bootstrap technique is a relatively new statistical method (Efron, 1982). We use it here to provide some additional information about the tenability of our model, in the following way. The probability model believed to be governing the data is specified, along with some summary statistics from the observed data. Using this information, the computer is used to generate samples, and these generated samples are checked for agreement with the actual sample (usually graphical methods are used here). If there is agreement between the generated samples and the actual samples, this is evidence supporting the assumed probability model. It should be noted that we are using the bootstrap here mostly for illustrative purposes, since the chi-square tests have already given us strong evidence for the negative binomial model.

In this particular instance, the bootstrap was performed assuming that (1) the underlying distribution is negative binomial, and (2) the marginal totals are known, that is, the totals  $t_i = X_i + Y_i$  from the actual data were used. From this information, we arrive at (4.19) as the relevant distribution. For a given sample, the parameter  $K$  in (4.19) was estimated by  $\hat{K}^+$ . Then, for each value of  $t_i$ , a value  $X_i'$  was generated from the distribution in (4.19), and the corresponding  $Y_i'$  was calculated using the relationship  $Y_i' = t_i - X_i'$ . For an actual sample of size  $n$ ,  $n$  new pairs  $(X_i', Y_i')$  were generated in this manner, constituting a bootstrap sample.

One can generate as many bootstrap samples as desired, but since we are only using the bootstrap as an illustrative procedure, we only present three bootstrap samples. Graphical representations of these bootstrap samples, together with the actual sample, are given in Figure 4.1 for the Bowline Point 1983 data. It is easy to see that there is close agreement between the bootstrap samples and the actual data, although the bootstrap samples display less "within variation" than the actual data. This is, to a certain extent, to be expected, since the bootstrap data are simulated and, therefore, not subject to any additional sources of variation other than those specified by the model. It should be noted, however, that the bootstrap sample is from a negative binomial distribution and hence has more variability than a sample generated from a Poisson distribution. The fact that the bootstrap sample displays less within variability than the actual data can be interpreted as further evidence against the Poisson assumption.

As mentioned before, we are only using the bootstrap as an illustrative tool, and are not basing any inferences on it. Statistics can be calculated to compare the bootstrap samples to the actual sample, but for our purposes they would add nothing over the included graphs.

In Figure 4.2 the bootstrap samples are presented in histogram form, to allow an alternative method of comparison. The histograms allow comparison of only one variable at a time, so we have shown only the first variable ( $X_i$ ) from the pairs. (Histograms of the second variable are comparable, since the bootstrap pairs and the data pairs have the same sum.) Again, the histograms show close agreement between the observed data and the bootstrap sample. Table 4.3 lists the Bowline Point 1983 Table B2 data together with two typical bootstrap samples.

Figure 4.1: Plots of the observed data, Bowline Point 1983 Table B2, together with plots of the bootstrap samples

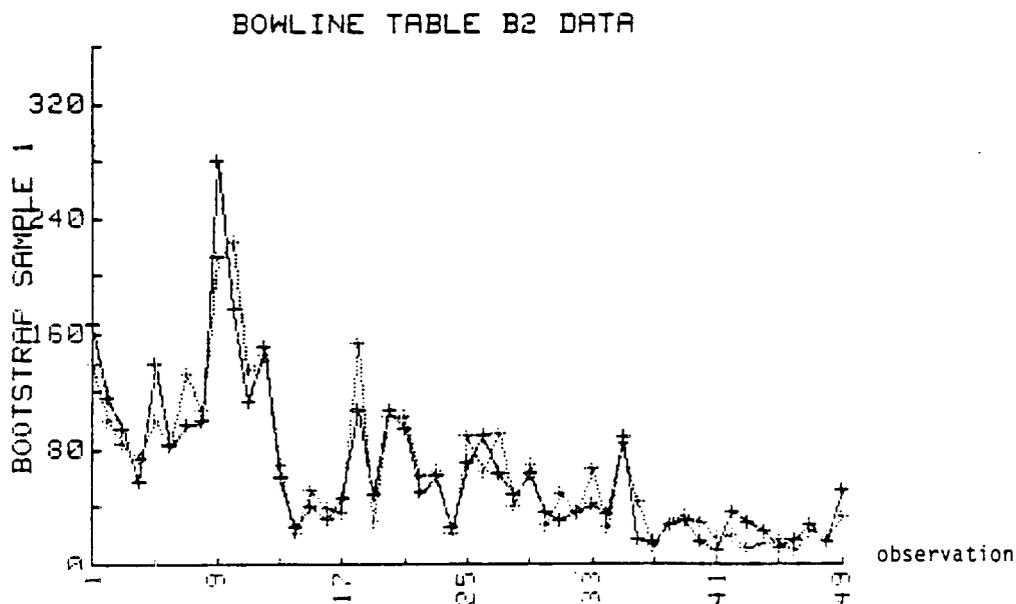
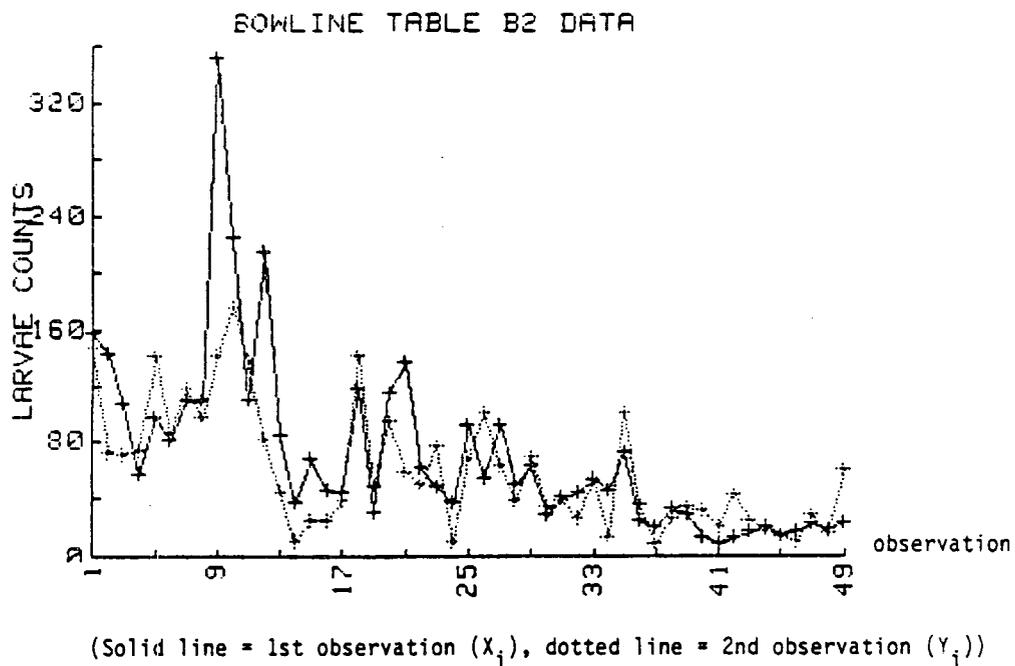
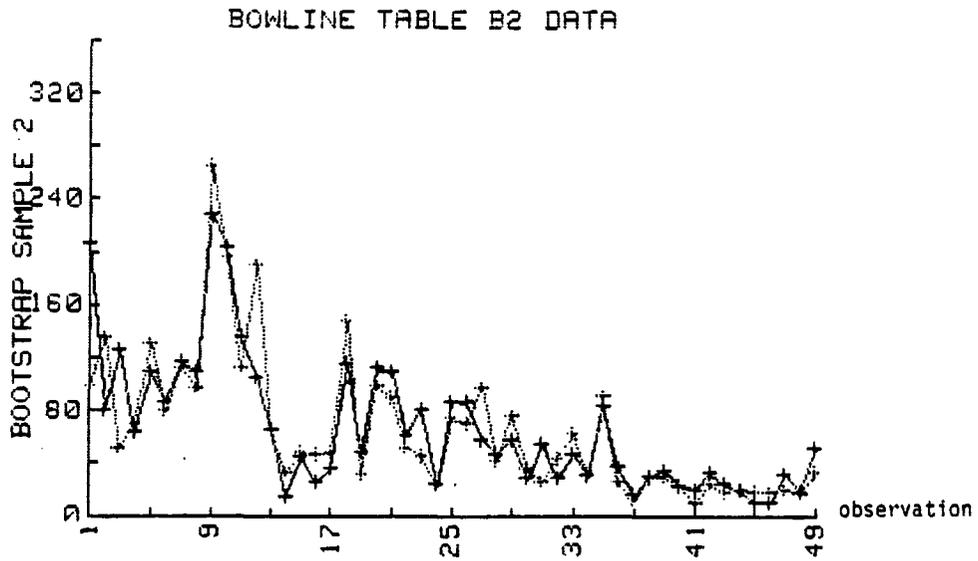


Figure 4.1 (continued)



(Solid line = 1st observation ( $X_t$ ), dotted line = 2nd observation ( $Y_t$ ))

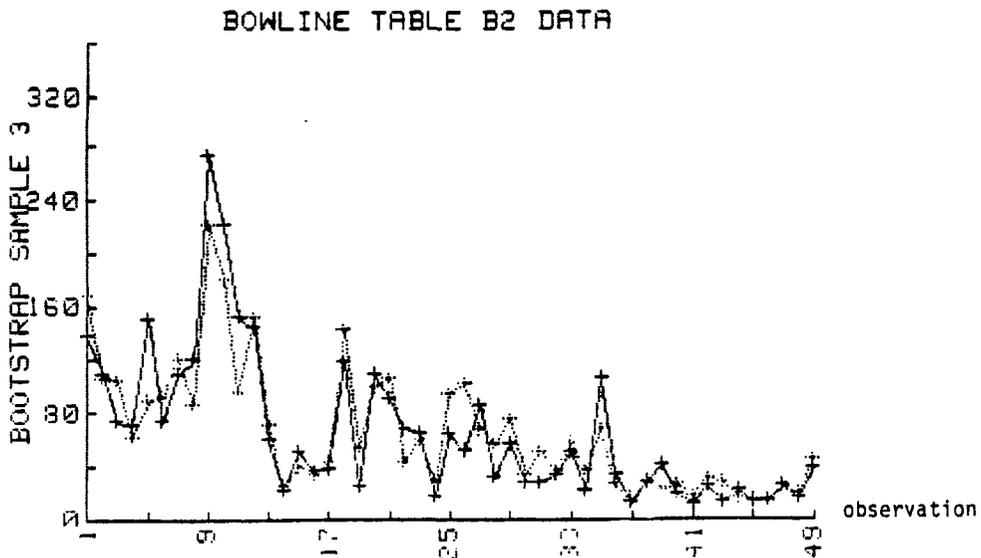


Figure 4.2: Histograms of the first observation from each pair in the Bowline Point 1983 Table B2 sample, and the first observation from each pair in three bootstrap samples

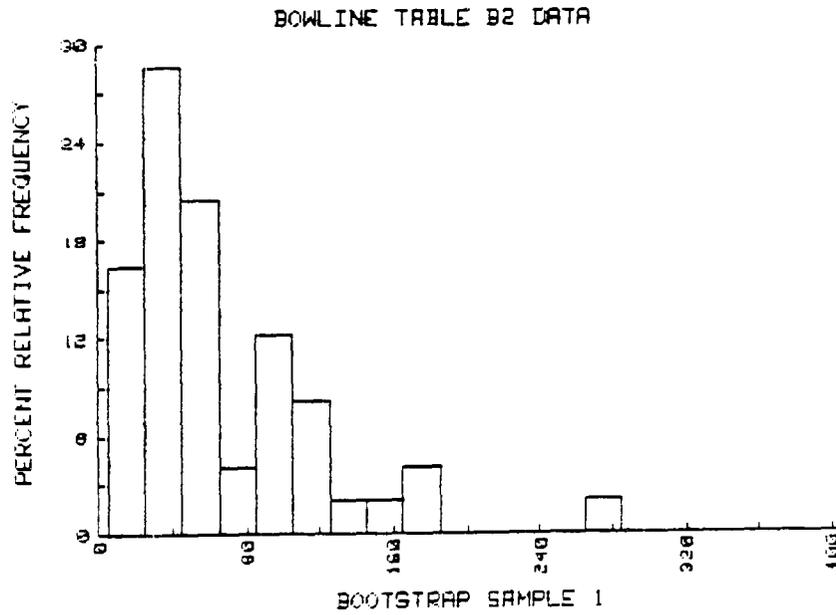
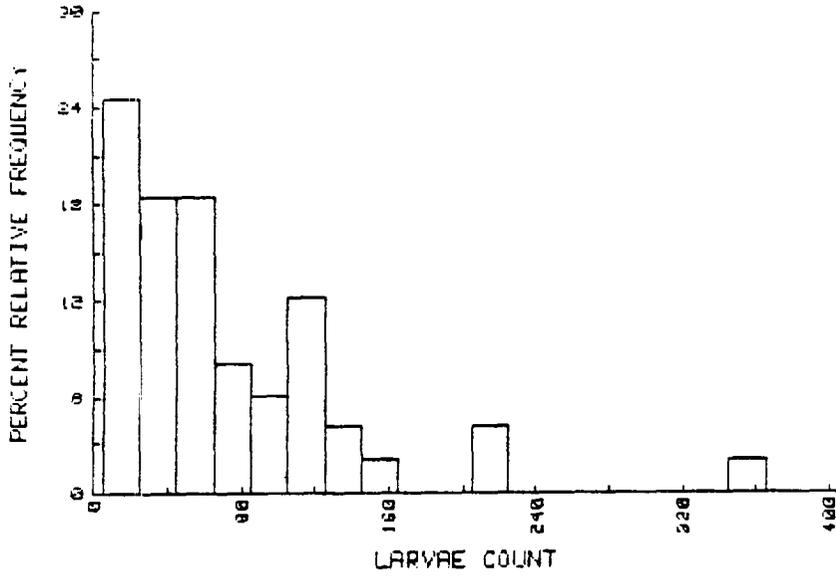


Figure 4.2 (continued)

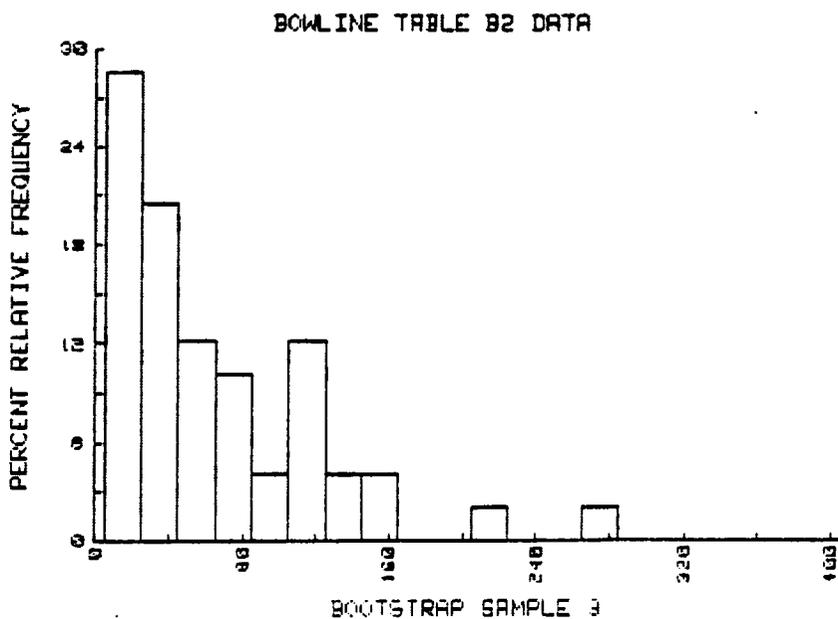
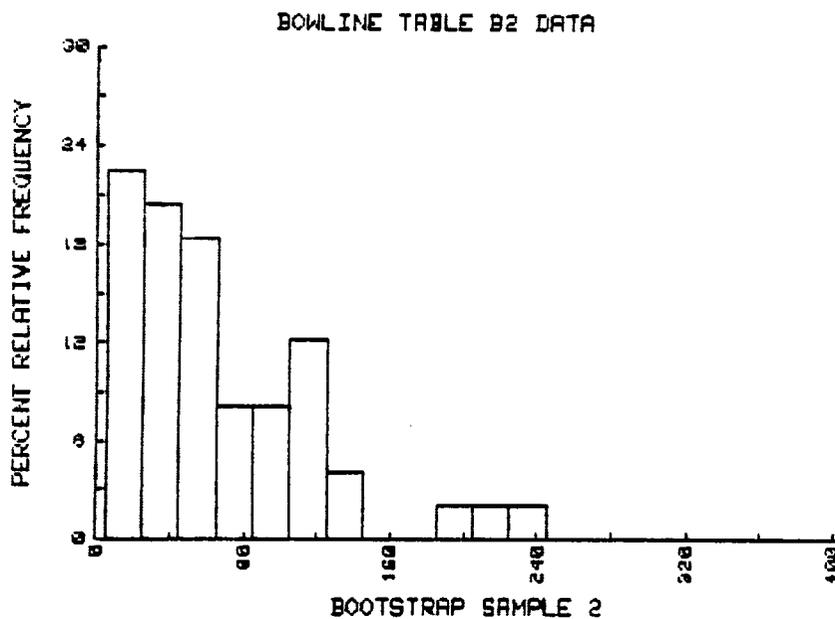


Table 4.3

Bowline Point 1983, Table B2 Data and Bootstrap Samples

<u>Data</u>		<u>Bootstrap Sample 1</u>		<u>Bootstrap Sample 2</u>	
<u>Obs 1:X<sub>i</sub></u>	<u>Obs 2:Y<sub>i</sub></u>	<u>Obs 1:X<sub>i</sub></u>	<u>Obs 2:Y<sub>i</sub></u>	<u>Obs 1:X<sub>i</sub></u>	<u>Obs 2:Y<sub>i</sub></u>
158	147	166	139	207	98
143	72	115	100	80	135
106	71	94	83	126	51
57	74	57	74	62	69
97	141	138	100	108	130
80	85	82	83	86	79
109	119	96	132	116	112
109	96	99	106	109	96
350	142	280	212	228	264
224	175	176	223	204	195
109	137	112	134	134	112
214	80	150	144	104	190
84	45	60	69	65	64
37	9	25	21	14	32
67	24	40	51	44	47
46	24	31	39	24	46
44	38	46	36	35	47
118	142	107	153	114	146
30	48	48	30	47	31
116	93	106	103	111	98
137	59	94	102	108	88
61	50	50	61	60	51
49	76	61	64	80	45
37	9	25	21	23	23
91	68	70	89	86	73
55	99	89	65	85	69
91	63	63	91	57	97
50	38	48	40	46	42
63	69	63	69	57	75
28	34	35	27	27	35
41	38	30	49	54	25
44	26	36	34	27	43
53	54	41	66	46	61
46	13	34	25	30	29
72	100	88	84	82	90
24	36	17	43	36	24
19	8	15	12	15	12
32	25	27	30	29	28
29	34	30	33	34	29
13	31	15	29	22	22
8	20	10	18	19	9
12	43	36	19	32	23
16	24	29	11	23	17
20	17	23	14	18	19
14	13	11	16	10	17
17	10	17	10	10	17
22	28	27	23	31	19
18	15	15	18	17	16
23	60	51	32	51	32

### 4.3 THE INDIAN POINT REPLICATE SAMPLING

The analysis in Sections 4.1 and 4.2 used data available from Bowline Point and Roseton. Replicate data are also available from Indian Point. However, because of their different nature, we treat them separately here. Ichthyoplankton samples were collected using two manual pump/net abundance samplers and an AUTOSAM at Indian Point Station D2 between June 17 and July 8, 1983. A description of the sampling is given in the Indian Point 1983 Annual Report (EA, 1984b). Data from the two pump samplers and the AUTOSAM were analyzed by the techniques used in Section 4.2 for data from Roseton and Bowline Point. The results of analyses will now be presented. An explanation of the methodology appears in Sections 4.1 and 4.2. Triplicate observations (Pump 1, Pump 2, and AUTOSAM) were obtained for 47 three-hour periods. Eight species-life stage combinations were present in numbers sufficiently great to make analysis worthwhile.

Tables 4.4, 4.5, and 4.6 give the comparisons of the two pumps to each other and of each pump to the AUTOSAM. Each of these comparisons (Pump 1 to Pump 2, AUTOSAM to Pump 1, and AUTOSAM to Pump 2) is treated by the methods of Section 4.2. For example, to compare AUTOSAM to Pump 1, the data from these two sampling devices are viewed as replicate observations from a series of p.s.u.'s and analyzed exactly as shown (for different data) in Table 4.1. (The three sampling devices were compared two at a time, rather than all three together, because the extension of the statistical methods here to triplicate observations is currently not known.) For each of the species-life stage combinations examined, the chi-square statistic testing the goodness of fit of the Poisson model, which was given in Section 4.1, is computed. In addition, the constant K is estimated as described in Section 4.2 and the chi-square statistic testing the goodness of fit of the negative binomial distribution is reported.

The assumption that each pair of replicate samples has equal volume is required by the analysis for the negative binomial distribution. Consequently, analysis of the Indian Point data was restricted to those of the 74 p.s.u.'s in which all three sampling devices were operating and all three resulting sample volumes differed by no more than 10 percent. There were 47 p.s.u.'s satisfying these conditions.

In all cases, the Poisson model was decisively rejected by the data. This is consistent with the results for Bowline Point and Roseton in Section 4.1. The fit of the negative binomial distribution is vastly better than the fit of the Poisson distribution, but not as good as it was for the data from the other two power plants. This is not unexpected for the comparisons of Pump 1 versus AUTOSAM and Pump 2 versus AUTOSAM; with two different sampling mechanisms involved, it seems reasonable that their simultaneously taken observations may not constitute independent, identically distributed draws from a negative binomial distribution. The difference in sampling mechanisms makes it plausible a priori that the manual pump/net sampler and the AUTOSAM may be generating observations from two distributions that are not identical. Conditions that seem necessary to assume equality of the sampling mechanisms would be equal area and same direction of the sampling orifice (pipe), equal velocity at the sampl-

Table 4.4

Indian Point 1983  $\chi^2$  Tests To Evaluate  
The Poisson And Negative Binomial Models:  
Pump 1 vs. Pump 2

(3 Hour Samples, 47 Pairs)

Species	Life Stage	$\hat{K}$	Poisson $\chi^2$	Significance Level	Neg. Bin. $\chi^2$	Significance Level
Clupeidae	Larvae	59.22	70.29	.015	59.79	.083
Alosa spp.	Larvae	6.94	85.38	.00052	62.33	.054
Engraulidae	Larvae	44.75	187.82	$<10^{-7}$	67.62	.021
Anchoa mitchilli	Eggs	1.28	184.96	$<10^{-7}$	63.49	.045
Anchoa mitchilli	Larvae	2.14	465.08	$<10^{-7}$	81.00	.001
Morone americana	Larvae	3.08	125.54	$<10^{-7}$	53.83	.20
Morone saxatilis	Larvae	.65	266.32	$<10^{-7}$	29.97	.97
Morone spp.	Larvae	3.45	112.05	$<10^{-7}$	45.39	.50

Table 4.5

Indian Point 1983  $\chi^2$  Tests To Evaluate  
The Poisson and Negative Binomial Models:  
Pump 1 vs. AUTOSAM

(3 Hour Samples, 47 Pairs)

Species	Life Stage	$\hat{K}$	Poisson $\chi^2$	Significance Level	Neg. Bin. $\chi^2$	Significance Level
Clupeidae	Larvae	24.01	92.00	$<10^{-4}$	63.53	.044
Alosa spp.	Larvae	2.64	127.31	$<10^{-7}$	58.97	.095
Engraulidae	Larvae	1.63	1499.51	$<10^{-7}$	56.00	.15
Anchoa mitchilli	Eggs	.02	613.27	$<10^{-7}$	808.21	$<10^{-7}$
Anchoa mitchilli	Larvae	.10	1694.01	$<10^{-7}$	138.48	$<10^{-7}$
Morone americana	Larvae	1.26	204.97	$<10^{-7}$	58.82	.097
Morone saxatilis	Larvae	.85	319.38	$<10^{-7}$	44.68	.53
Morone spp.	Larvae	1.52	174.21	$<10^{-7}$	55.51	.16

Table 4.6

Indian Point 1983  $\chi^2$  Tests To Evaluate  
The Poisson and Negative Binomial Models:  
Pump 2 vs. AUTOSAM

(3 Hour Samples, 47 Pairs)

Species	Life Stage	$\hat{K}$	Poisson $\chi^2$	Significance Level	Neg. Bin. $\chi^2$	Significance Level
Clupeidae	Larvae	16.22	114.08	$<10^{-6}$	69.56	.014
Alosa spp.	Larvae	2.74	147.78	$<10^{-7}$	68.30	.022
Engraulidae	Larvae	1.71	1565.58	$<10^{-7}$	58.44	.103
Anchoa mitchilli	Eggs	.05	420.81	$<10^{-7}$	320.92	$<10^{-7}$
Anchoa mitchilli	Larvae	.19	1688.75	$<10^{-7}$	99.75	$<10^{-5}$
Morone americana	Larvae	1.19	144.37	$<10^{-7}$	37.42	.81
Morone saxatilis	Larvae	2.67	215.98	$<10^{-7}$	55.39	.16
Morone spp.	Larvae	2.15	178.16	$<10^{-7}$	64.46	.037

ing orifice (intake velocity), and equal duration of pumping. Differences between sampling systems would cause differences to be expected in the sampling distributions observed, at least in an absolute sense. There are slight differences between the AUTOSAM and the pumps in duration of pumping and in velocity at the sampling orifice (the AUTOSAM's flow rate is about .8 to .9 cubic meters per minute, while the pumps' rate is about 1 cubic meter per minute; see EA, 1984b, p. 3-1), but the systems are quite similar in other respects. Note that the pump versus AUTOSAM negative binomial model is not rejected with overwhelming force, as the Poisson model is. However, the negative binomial model's goodness of fit can be fairly described as marginal.

It is more surprising that the comparison between the two identical pumps also results in a somewhat marginal level of agreement with the negative binomial model. For two identical pumps located one meter apart horizontally in the discharge channel, it is puzzling that the family of negative binomial distributions, which is much broader than the family of Poisson distributions, does not provide a better fit. A search of the observations for clues to what is happening turned up a series of anomalous observations. One such set of observations is the following collection of triplicate sample counts of Engraulid larvae:

	<u>Pump 1</u>	<u>Pump 2</u>	<u>AUTOSAM</u>
	39	44	282
	125	185	0
	54	68	203
	139	187	14
	240	239	62
	46	102	40
	835	858	266
	474	386	567
	244	153	169
	178	217	317
	<u>98</u>	<u>155</u>	<u>179</u>
Coefficient of Variation(%)	105.9%	95.9%	87.6%

Since Engraulids other than bay anchovy are very uncommon in the Hudson River (EA, 1984b, p. 4-10), it is appropriate to treat the lumped counts of bay anchovy and anchovies (i.e., all Engraulids other than bay anchovy) combined. This is because ichthyoplankton identified as anchovies are virtually certain to be bay anchovy. Using lumped Engraulid data has the effect of replacing counts that are distorted due to incomplete identification at the species level with undistorted, honest bay anchovy counts.

These counts reveal a great deal of fluctuation, as evidenced by the very large coefficients of variation. The variation observed in these replicate observations is greater than negative binomial variation.

The fact that the negative binomial model fits so well in all other cases leads us to retain it and, for the time being, to consider these Indian Point data as outliers or aberrant observations. Since there is some doubt that these data are true replicates (EA, 1984b, pp. 4-24 to 4-26), there is no reason to go beyond the negative binomial model. The observations listed above are not atypical of the Indian Point data; similar lists can be given for the other species-life stage combinations. It is clear that these Indian Point data are characterized by a greater degree of inherent variability than the data from the other power plants analyzed previously. Several factors could be responsible for this increased random fluctuation, including the fact that the discharge channel is an open channel rather than a closed one. Another source of difficulty is the great variation in the volumes of the samples, which introduces substantial complexities to the analysis. The extreme disparity among simultaneous counts indicates that a very broad, flexible class of distributions may be needed in order to obtain a better fit to the observed data.

Two further analyses have been made to supplement the goodness-of-fit tests. The first is a set of plots of the observed data for the eight species-life stage combinations treated. These plots are given in Figure 4.3. They confirm the pattern of highly discrepant triplicate simultaneous observations illustrated by the counts listed above. The second is a bootstrap analysis of the type described in Section 4.2. This analysis is Table 4.7.

The bootstrap values of  $\hat{K}^+$  show a large amount of variability, reiterating the evidence that these data show variation beyond negative binomial.

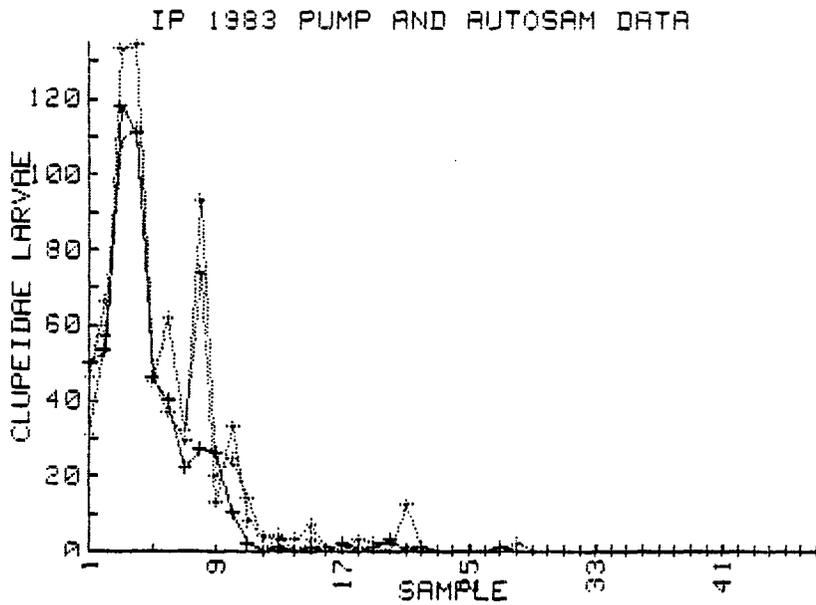
#### 4.4 SAMPLE DURATION

The sample duration, or equivalently the length (in time) of a p.s.u., is important because it bears directly on the amount of effort expended in sampling and the accuracy attainable in estimation. We first focus on the question of whether the negative binomial model is necessary for longer sample durations. If such samples display reduced variability, resulting in a large value for  $K$ , then the Poisson model may become applicable for these longer samples.

As sample duration increases, we might expect a "smoothing" effect to occur; that is, while ichthyoplankton counts taken on an hourly basis could show relatively large variability, we might expect counts taken over longer periods, such as daily counts, to show less variability. Three reasons can be advanced in support of this expectation. First, diurnal variation will have less of an effect on counts over longer periods, and no effect on daily counts.

Second, assume that the one-hour count  $X_i$  in p.s.u.  $i$  has mean  $\mu_i$  and variance  $\mu_i + (1/K)\mu_i^2$ , where each  $\mu_i$  is positive. Let  $\Sigma X_i$  denote (for concreteness) the sum of 24 one-hour counts. Because it is a sum of independent random variables,  $\Sigma X_i$  has mean  $\Sigma \mu_i$  and variance  $\Sigma \mu_i + (1/K)\Sigma \mu_i^2$  (where all

Figure 4.3: Plots Of Observed Data,  
Indian Point 1983 Pumps vs. Autosam



Dotted lines = Pump Samples, Solid Lines = AUTOSAM Samples.

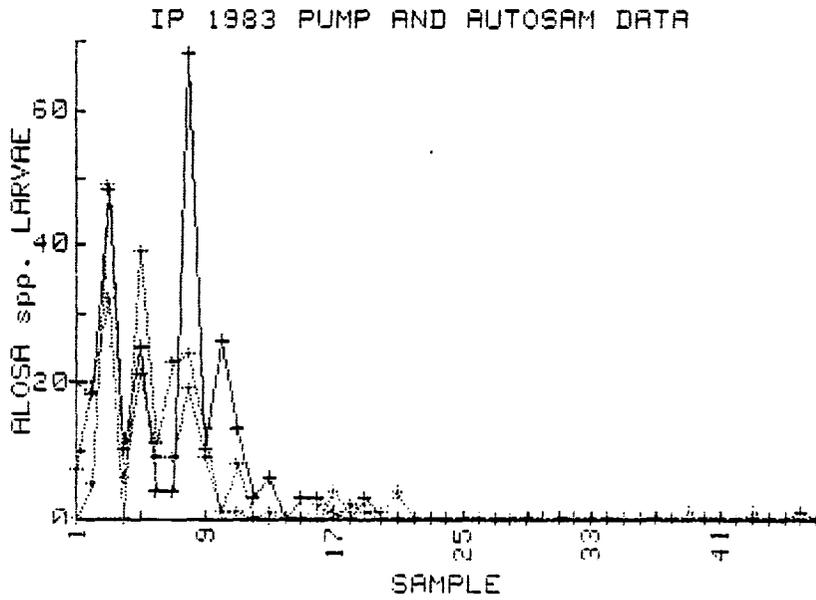
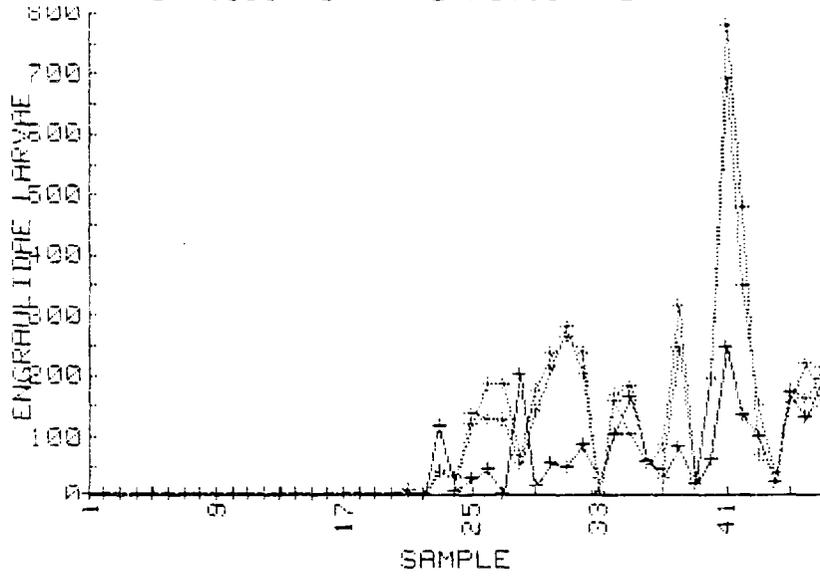


Figure 4.3 (Continued)

IP 1983 PUMP AND AUTOSAM DATA



Dotted Lines = Pump Samples, Solid Lines = AUTOSAM Samples.

IP 1983 PUMP AND AUTOSAM DATA

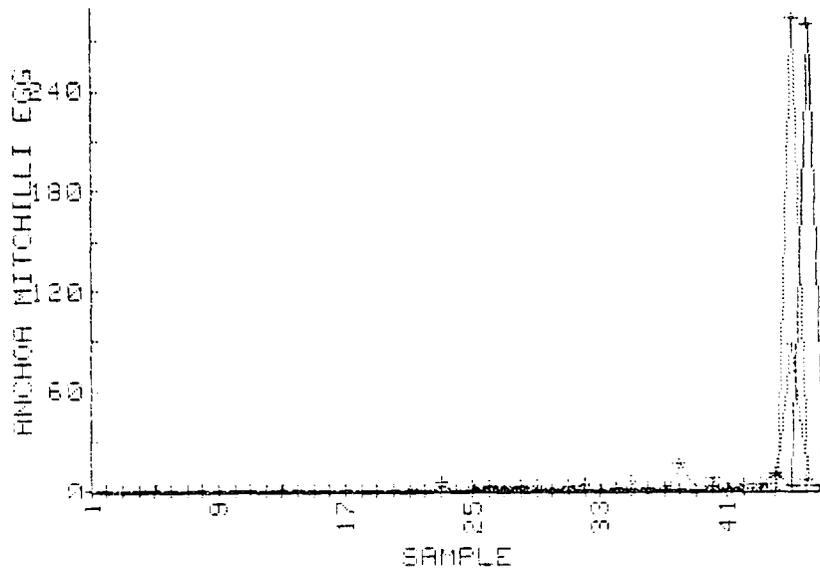
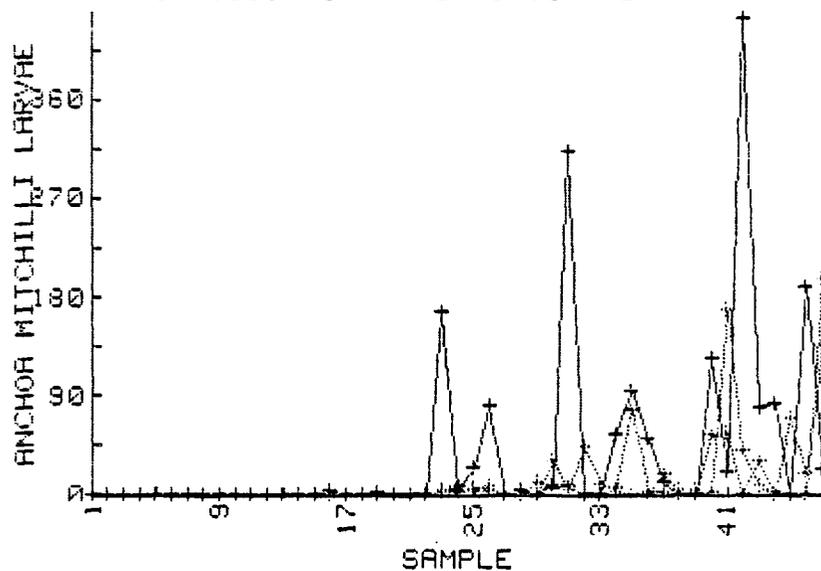


Figure 4.3 (Continued)

IP 1983 PUMP AND AUTOSAM DATA



Dotted Lines = Pump Samples, Solid Lines = AUTOSAM Samples.

IP 1983 PUMP AND AUTOSAM DATA

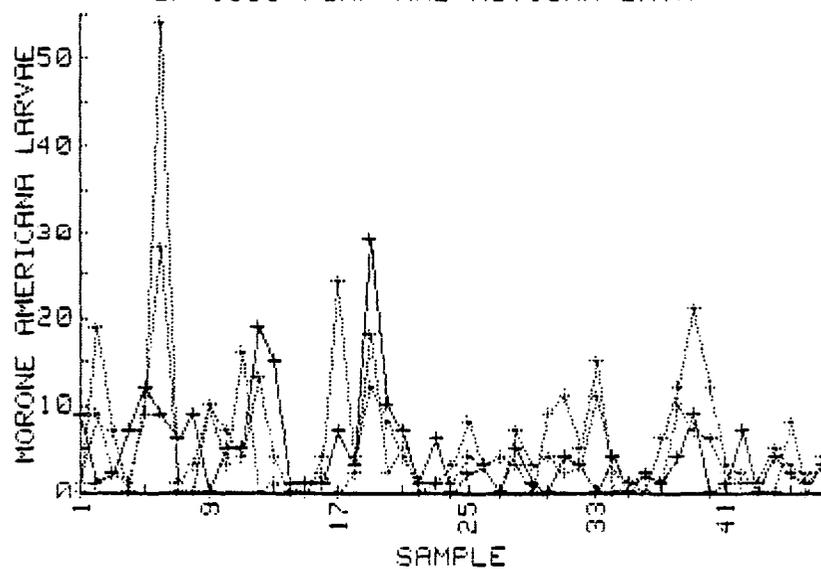
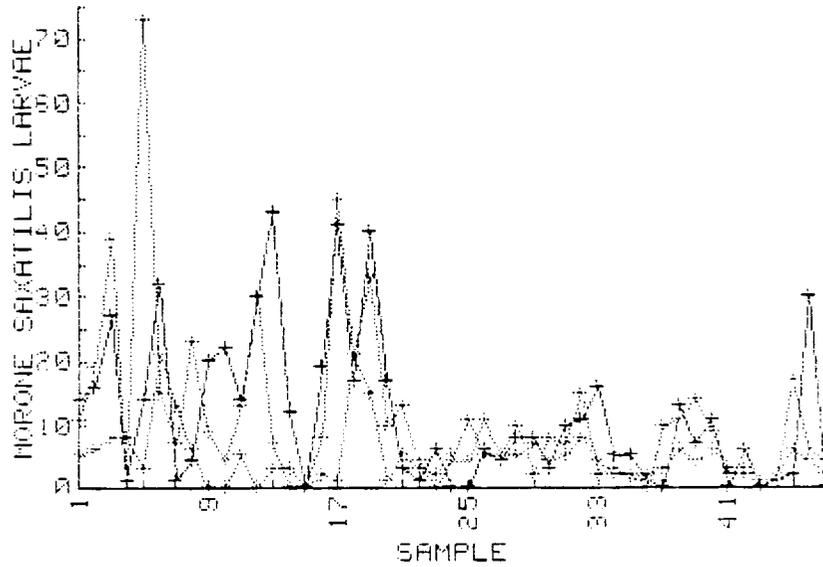


Figure 4.3 (Continued)  
 IP 1983 PUMP AND AUTOSAM DATA



Dotted Lines = Pump Samples, Solid Lines = AUTOSAM Samples.

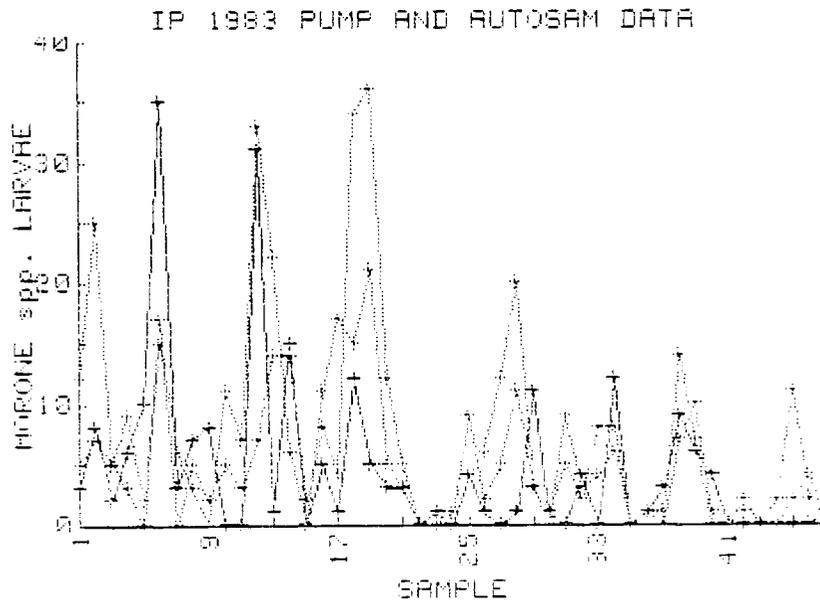


Table 4.7

Estimated  $\hat{K}^+$  Values From Indian Point 1983  
Paired Pump Data And 10 Bootstrap Samples\*  
 (47 3 Hour Samples)

$\hat{K}^+$	59.22	6.94	44.75	1.28	2.14	3.08	.65	3.45
B	81.76	5.95	17.27	1.73	9.85	2.85	.82	2.19
O	100.48	10.60	29.31	1.12	2.59	4.89	.56	2.03
T	59.98	37.82	126.15	1.25	1.68	1.28	.56	2.55
S	21.32	2.83	20.04	496.68	10.50	3.76	.80	3.83
R	81.03	9.36	107.67	.44	2.18	2.57	.45	3.71
A	55.24	10.88	89.53	1.19	3.77	4.36	.53	1.63
P	117.73	4.32	59.75	25.90	.74	6.13	.87	6.46
V	188.48	4.82	28.19	16.40	1.36	3.91	.52	7.06
A	93.69	5.49	112.37	45.43	6.54	1.54	.26	2.62
L	44.55	9.47	41.24	.92	10.84	6.19	.84	2.72
U								
E								
S								
O								
F								
$\hat{K}^+$								
Mean	82.13	9.86	61.48	53.85	4.74	3.69	.62	3.48
Std. Dev.	44.61	9.65	40.25	147.58	3.94	1.64	.19	1.77

\*The columns of this table correspond to the eight species-life stage combinations entrained in sufficient numbers for analysis. Each column contains the value of  $\hat{K}^+$  estimated from the data and 10 values calculated from bootstrap samples.

<u>Column</u>	<u>Species</u>	<u>Life-Stage</u>
1	Clupeidae	Larvae
2	Alosa spp.	Larvae
3	Engraulidae	Larvae
4	Anchoa mitchilli	Eggs
5	Anchoa mitchilli	Larvae
6	Morone americana	Larvae
7	Morone saxatilis	Larvae
8	Morone spp.	Larvae

sums are over the 24 one-hour p.s.u.'s). However, if we instead view  $\Sigma X_j$  as the count from a single 24-hour p.s.u., the negative binomial model gives the variance of  $\Sigma X_j$  as  $\Sigma \mu_j + (1/K')(\Sigma \mu_j)^2$ , where  $K'$  corresponds to 24-hour p.s.u.'s rather than one-hour p.s.u.'s. This implies that

$$(1/K)(\Sigma \mu_j^2) = (1/K')(\Sigma \mu_j)^2, \quad (4.20)$$

and it then follows from

$$(\Sigma \mu_j)^2 > \Sigma \mu_j^2, \quad (4.21)$$

which is easily shown, that  $K' > K$ . Of course, the amount by which  $K'$  exceeds  $K$  depends on the one-hour p.s.u. means  $\mu_j$ .

Third, under ideal theoretical conditions, in particular constant flow rate in the discharge channel and constant ichthyoplankton mean density, we would expect the variation in the channel to become more like Poisson variation as the sample duration increases (See Appendix IV). In terms of our parameters, we would expect  $K \rightarrow \infty$  as sample duration  $\rightarrow \infty$ . However, the empirical evidence, presented below, suggests that this is not the case.

Table 4.8 shows values of  $\hat{K}^+$  for the Roseton 1982 data, for the eight species-life stage combinations considered and for sample durations of 1, 2, and 3 hours. While there is some evidence that  $\hat{K}^+$  is increasing as a function of sample duration, it is not increasing quickly enough to support the hypothesis that the distribution of the data approaches the Poisson. In fact, examination of Table 4.1 shows strong evidence that the Poisson distribution does not fit the three-hour samples, while the negative binomial still does quite a good job.

Ten bootstrap samples were generated by the mechanism explained in Section 4.2, in order to get some idea of the variability in the estimation of  $\hat{K}^+$ . Since there are no actual data available to do this (it would require at least two sets of simultaneous paired samples, that is, four simultaneous samples), the bootstrap was the only available mechanism. The estimated  $\hat{K}^+$  values resulting from these bootstrap samples appear in Table 4.8. We chose to generate 10 samples because it was felt that this would give enough information about the variability of  $\hat{K}^+$ . Of course, more samples could have been generated, but then the question of computer time would have become an issue. Furthermore, since these are simulated data, they are not giving information about the variability of  $\hat{K}^+$  calculated from data in the discharge channel, but rather about  $\hat{K}^+$  calculated from data that follow the assumed model.

Table 4.8

Estimated  $\hat{K}^+$  Values from Roseton 1982 Data and 10 Bootstrap Samples  
 Roseton 1982 Data - 1 Hour Samples\*

$\hat{K}^+$	5.04	34.24	25.94	1.77	17.65	8.85	15.97	.83
B	1.86	62.74	19.02	1.25	28.07	13.17	39.04	.34
O	11.16	40.68	16.2	1.87	21.17	9.24	56.83	1.79
T	4.5	87.42	16.31	.9	19.35	19.67	43.83	.58
S	4.74	104.8	41.88	3.66	15.37	4.76	21.23	.88
T	8.29	10.82	37.27	1.03	19.54	4.16	11.05	1.06
R	4.11	105.48	28.16	1.14	20.06	28.58	65.03	.91
A	6.96	100.15	33.23	1.37	12.14	5.67	15.27	1.81
P	7.86	99.4	17.27	1.99	19.64	5.75	22.4	1.22
V	2.1	28.19	27.71	1.38	15.37	8.1	14.26	.33
A	9.66	31.64	16.02	1.99	26.73	12.92	13.69	.72
L								
U								
E								
S								
O								
F								
$\hat{K}^+$								
Mean	6.03	64.14	25.36	1.67	19.55	10.99	28.96	.95
Std. Dev.	3.01	36.17	9.21	.76	4.7	7.42	19.05	.5

\*The columns of this table correspond to the eight species-life stage combinations entrained in sufficient numbers for analysis. Each column contains the value of  $\hat{K}^+$  estimated from the data and 10 values calculated from bootstrap samples.

Column	Species	Life Stage
1	Clupeids	Larvae
2	Alosa spp.	Larvae
3	White perch	Eggs
4	White perch	Yolk-sac larvae
5	White perch	Larvae
6	Striped bass	Yolk-sac larvae
7	Striped bass	Larvae
8	Morone spp.	Larvae

Table 4.8 continued

<u>Roseton 1982 Data - 2 Hour Samples</u>								
$\hat{K}^+$	6.61	40.83	61.69	1.89	49.98	9.01	27.77	.93
B	2.62	74.84	28.60	2.09	51.29	5.87	49.90	1.57
O	2.45	34.65	46.35	1.74	36.14	5.85	48.80	1.24
T	5.69	39.05	20.07	1.87	14.01	10.88	23.75	.45
T	6.12	21.67	34.24	4.08	35.61	16.65	26.74	1.15
R	2.99	42.09	92.26	1.85	50.16	8.72	41.46	2.37
A	2.25	34.29	47.25	2.20	27.80	5.47	46.68	1.84
P	7.42	125.93	171.85	3.24	15.54	4.72	76.27	.32
V	16.13	50.17	27.75	1.12	11.02	8.41	29.12	3.36
A	7.79	92.37	26.55	5.89	21.15	21.12	24.75	2.98
L	9.14	15.29	148.78	1.24	40.58	13.07	167.53	.56
L								
U								
E								
S								
O								
F								
$\hat{K}^+$								
Mean	6.29	51.92	64.13	2.47	32.12	9.98	51.16	1.52
Std. Dev.	4.05	33.01	51.96	1.42	15.16	5.16	41.68	1.02

Table 4.8 continued

<u>Roseton 1982 Data - 3 Hour Samples</u>								
$\hat{K}^+$	7.86	107.25	52.81	1.55	48.31	10.97	33.08	.91
B	30.02	57.23	34.19	2.47	27.97	12.67	122.31	.73
O	31.97	34.88	124.65	4.8	64.59	5.78	28.09	.49
T	6.82	317	72.75	1.69	40.82	21.49	20.55	2.21
S	10.13	645.96	104.56	.92	71.86	21.34	123.05	1.45
T	5.2	770.97	49.71	1.6	34.02	12.86	24.34	.53
R	15.26	109.78	48.09	2.94	17.42	4.79	71.12	3.24
A	7.26	145.72	58.31	1.47	41.49	62.45	33.92	.7
P	9.4	318.78	17.96	1.82	130.13	31.14	85.25	.34
V	36.13	627.28	115.15	1.81	36.76	6.91	47.02	1.42
A	4.87	281.46	28.5	.96	17.26	34.28	67.76	.87
L								
U								
E								
S								
O								
F								
$\hat{K}^+$								
Mean	14.99	310.57	64.24	2	48.24	20.43	59.68	1.17
Std. Dev.	11.8	259.76	35.92	1.1	32.07	17.1	37.55	.87

At present, adequate data are not available to test the Poisson vs. negative binomial models for longer sample durations. This is because the compositing process drastically reduces the number of paired replicate samples available for analysis. For example, the Roseton 3-hour composite analysis contains only 18 observations. Furthermore, compositing must be done over consecutive sampling periods. In most of the cases examined there were gaps in the sampling process, that is, time periods in which no sampling occurred. This fact also contributes to the small numbers of paired replicate samples entering the analyses of composited data. Compositing beyond 3-hour periods would reduce the number of paired replicate samples below the point where analysis would be worthwhile.

Investigation of p.s.u. lengths greater than three hours does not seem profitable for several reasons. These include both statistical and non-statistical considerations pertaining to the estimation of entrainment, as well as the needs of other related analyses. The data collected in entrainment abundance sampling will be used in mortality and similar studies; the need to provide sufficiently detailed information for these studies makes short p.s.u.'s seem more appropriate than long ones.

The main issue is the effect of p.s.u. length on the standard deviation and the coefficient of variation of the estimate of entrainment over the entire season. We will now show that when the negative binomial model holds, making the p.s.u. shorter results in a lower CV of seasonal entrainment. To avoid unnecessary complications, some simplifying assumptions will be made.

Assume that the entrainment season consists of  $d$  days indexed by  $i=1, \dots, d$ , each of which is divided into  $r$  p.s.u.'s indexed by  $j=1, \dots, r$ . Define

$V_{ij}$  = the volume of water in p.s.u.  $j$  on day  $i$ ,

$v_{ij}$  = the volume of water sampled from p.s.u.  $j$  on day  $i$ ,

$\lambda_{ij}$  = the true mean density of ichthyoplankton in p.s.u.  $j$  on day  $i$ ,

$X_{ij}$  = the observed ichthyoplankton count in p.s.u.  $j$  on day  $i$ ,

$T_{ij}$  = the true number of ichthyoplankton in p.s.u.  $j$  on day  $i$ .

Then  $\hat{\lambda}_{ij} = X_{ij}/v_{ij}$  and the variance of  $X_{ij}$  is given by (4.16), so

$$\hat{\tau} = \sum_{i=1}^d \sum_{j=1}^r (V_{ij}/v_{ij})(v_{ij}\hat{\lambda}_{ij}) = \sum_i \sum_j \hat{T}_{ij} \quad (4.22)$$

$$\begin{aligned}\text{Var}(\hat{T}) &= \sum_i \sum_j (V_{ij}/v_{ij})^2 [v_{ij}\lambda_{ij} + (v_{ij}\lambda_{ij})^2/K] \\ &= \sum_i \sum_j (V_{ij}/v_{ij})(v_{ij}\lambda_{ij}) + v_{ij}^2\lambda_{ij}^2/K\end{aligned}\quad (4.23)$$

Assume that all sample volumes  $v_{ij}$  have a common value  $v$  and that all p.s.u. volumes  $V_{ij}$  have a common value  $V$ . Then

$$\begin{aligned}\text{Var}(\hat{T}) &= (V/v)\sum T_{ij} + \sum T_{ij}^2/K \\ &= (V/v)T + \sum T_{ij}^2/K \\ &= T^2[(V/v)(1/T) + \sum T_{ij}^2/(\sum T_{ij})^2K] \\ &= T^2[(1/drv)(drV/T) + \sum T_{ij}^2/(\sum T_{ij})^2K]\end{aligned}\quad (4.24)$$

$$\text{CV}(\hat{T}) = [(1/drv)(drV/T) + \sum T_{ij}^2/T^2K]^{1/2}\quad (4.25)$$

Assume that water is sampled from the discharge channel at a constant rate per unit time, regardless of the length of a p.s.u. Consider the behavior of  $\text{Var}(\hat{T})$  as a function of  $r$ , the number of p.s.u.'s in a 24-hour day. The annual sample volume, that is, the total amount of cooling water sampled during the entire entrainment season, is given by  $drv$ , and the annual discharge volume is given by  $drV$ . The overall seasonal mean density of organisms in the discharge flow is  $T/drv$ . The quantities  $drv$ ,  $drV$ ,  $T$ , and  $T/drv$  are not affected by the value of  $r$ , the number of p.s.u.'s per day. The only term in the expression for  $\text{CV}(\hat{T})$  affected by  $r$  is  $\sum T_{ij}^2$ , which decreases as  $r$  increases.

A numerical example will illustrate the effect of p.s.u. length, or  $r$  (which equals  $24/\text{p.s.u. length in hours}$ ), on the coefficient of variation of  $\hat{T}$ . Let the entrainment season consist of  $d = 5$  days, and let sampling take place at the rate of 150 cubic meters per 3-hour period, or 1,200 cubic meters per 24-hour day. Assume that the total number of organisms entrained is 2,400,000 and for computational convenience assume that they occur at a uniform rate of 20,000 organisms per hour for the entire five-day season. Finally, let the cooling water flow through the plant equal 100,000 cubic meters per 3-hour period. Then for any  $r$ ,

$$v = 1,200/r$$

$$V = 800,000/r$$

and

$$(1/dr)(drV/T) = (1/6,000)(4,000,000/2,400,000) = .0002777,$$

so the first term on the right hand side of (4.25) is negligible. And

$$\sum T_{ij}^2/T^2K = (5)(1)(480,000)^2/(2,400,000)^2K = .20(1/K) \text{ for } r = 1,$$

$$= (5)(8)(60,000)^2/(2,400,000)^2K = .025(1/K) \text{ for } r = 8,$$

$$= (5)(24)(20,000)^2/(2,400,000)^2K = .00833(1/K) \text{ for } r = 24,$$

so if  $K = 2$

$$CV(T) = [.0002777 + .10]^{1/2} = .3167 \text{ for } r = 1,$$

$$= [.0002777 + .0125]^{1/2} = .1130 \text{ for } r = 8,$$

$$= [.0002777 + .004166]^{1/2} = .0667 \text{ for } r = 24.$$

The differences among estimates based on 24-hour p.s.u.'s ( $r=1$ ), 3-hour p.s.u.'s ( $r=8$ ), and 1-hour p.s.u.'s ( $r=24$ ) will not be of this magnitude when the entrainment pattern is more complex, but they will show the same direction. In particular, the presence of diel patterns and steep peaks in entrainment density will make these differences smaller.

A further drawback of samples of longer duration is that they introduce other sources of error, such as nonsampling errors due to sorting and identification. With samples of longer duration, it will be necessary to increase the size of the sample in order to achieve desirable accuracy. Thus, larger batches of material must be sorted and identified.

Although the choice of short p.s.u.'s rather than long ones is clear, the costs of extra processing and data handling must be weighed. More importantly, it is necessary to remember the point, mentioned at the beginning of this section, that extremely short p.s.u.'s result in a great deal of variability. It can be difficult to use effectively the high-in-noise, low-in-signal information produced by p.s.u.'s that are extremely short.

#### 4.5 AN ALTERNATE MODEL FOR THE ESTIMATION PROCESS

The model described in Section 4.2, which ascribes negative binomial variation to the discharge channel, does a very good job of explaining the observed variation in the data. However, based on theoretical considerations, such as the derivation given in Appendix I, it could be considered somewhat surprising that the variation observed in the channel does not conform to the Poisson model. More precisely:

1. Assume that individual ichthyoplankton organisms have independent spatially random distributions (described in Appendix I) within the discharge channel; this physical characteristic of the process leads to the conclusion that the data should display Poisson variation, but
2. All empirical findings strongly reject the Poisson model as tenable.

This situation leads to the question: Is there a reasonable physical model for which the variation in the channel can be Poisson, but the variation in the observed data can be other than Poisson? The answer to this question has led to an alternative model that not only fits the data adequately, but also contains parameters that have meaningful interpretations and show how the observed data may not be Poisson.

It should be noted at this point that we are not advocating the model developed in this section as the one model that fits the data. We are only seeking to explain the observed variation without rejecting Poissonness in the discharge channel, and the model developed below does a credible job. The turbulence observed in the discharge channels leads to the belief that the distribution of larvae is Poisson, i.e., a spatially random distribution. The fact that the Poisson model is not tenable may lead one to believe that there is some sort of clumping in the channel, which can lead to negative binomial variation (Quenouille, 1949), or else to believe that additional variation is encountered during the data collection process. It is this latter alternative that we consider here.

The model developed in this section should perform well in analyzing the data treated in Section 4.5.3 if the model gives the sole or major cause of the data's failure to conform to the Poisson distribution. The model's poor performance in Section 4.5.3 leads us to dismiss it as an explanation for the observed negative binomial data; the model is not a credible explanation for this phenomenon.

We also note that with observational data it is impossible to test the adequacy of a model, that is, to find out if there is significant unexplained variation that is not attributable to random error, but can be attributed to inadequacy of the model. This is a very different question than that of whether a model explains a significant portion of all variation, which we can test with the available data. Therefore it is impossible to justify the following model on data-based arguments; we can

only answer whether or not the model explains a significant portion of the observed variation. Tests of adequacy (also known as lack of fit) must be based on designed experiments.

The treatment of the remainder of this section is for a single life stage of ichthyoplankton. Separate analyses can be performed for the different life stages.

#### 4.5.1 THE CLASSIFICATION MODEL

Suppose that classification into genus (e.g., *Morone* spp) can be made correctly, but that there is possible misclassification into species within the genus. Let  $X^*$  be the number of ichthyoplankton of a particular genus collected during a specified sampling period. These  $X^*$  organisms are later identified according to species. Let  $X$  denote the number of these organisms that are classified as being of a particular species, say species A. Note that the number of organisms of species A collected may differ from  $X$ , the number of organisms classified as species A; the two numbers will be equal only if there are no errors, or exactly offsetting errors, made during the identification process. The classification at this stage is a difficult task, and the assumption of no classification error is probably unrealistic.

Assume therefore that an organism from this genus is classified as species A with probability  $p$ . Then the distribution of  $X$  conditional on  $X^*$  (that is, given the value of  $X^*$ ) is binomial,

$$X \sim \text{Binomial}(X^*, p). \quad (4.26)$$

(More will be said later about  $p$ , the probability of classifying a member of the genus as belonging to species A.) Assume also that  $X^*$  has a Poisson distribution with mean  $\lambda$ ,

$$X^* \sim \text{Poisson}(\lambda). \quad (4.27)$$

Theoretical arguments of a biological nature, described in point 1 at the beginning of Section 4.5, support this assumption, which says that the variation in the channel is Poisson variation.

$X$  is the observed quantity in our data, the number of specimens classified as species A. The unconditional distribution of  $X$ , the observed count of species A, is

$$X \sim \text{Poisson}(\lambda p). \quad (4.28)$$

Now we come to the key point: what is the variation among the observed species counts? In Sections 4.1 and 4.2, we saw that the Poisson model did not fit the observed data, while the negative binomial model did. The motivation leading to the latter was the fact that in the Bowline Point 1983 paired observations from replicate sampling (EA, 1984a, Table B-2), the within-pair sample variance  $s^2$  substantially exceeded the pair mean on average. This suggested that the equality of the population variance and mean,

$$\sigma^2 = \mu, \quad (4.29)$$

which holds for any Poisson distribution, did not hold for these data. A more promising relationship was the possibility that the population variance could be a quadratic function of the mean,

$$\sigma^2 = \mu + c^2 \mu^2, \quad (4.30)$$

where  $c$  is an arbitrary nonnegative constant. This characteristic led us to the negative binomial distribution, which includes the Poisson distribution as a special case (when  $c = 0$ ).

The model now being considered is

$$X^* \sim \text{Poisson}(\lambda), \quad (4.31)$$

$$X \sim \text{Binomial}(X^*, p).$$

If under this model we observe  $n$  replicate counts  $X_1, X_2, \dots, X_n$  of species A from  $n$  simultaneously drawn samples (we usually have  $n = 2$ ), the model becomes

$$X_i^* \sim \text{Poisson}(\lambda) \quad \text{for } i = 1, \dots, n, \quad (4.32)$$

$$X_i \sim \text{Binomial}(X_i^*, p_i) \quad \text{for } i = 1, \dots, n,$$

where  $X_1^*$  to  $X_n^*$  are independent,  $X_1$  to  $X_n$  are independent, and  $p_i$  is the probability that an organism from the genus being examined is classified in the  $i^{\text{th}}$  sample as species A. Notice that  $p_1$  to  $p_n$  may differ according to the level of the sorter's skill, the working conditions, the time of day at which the sorting is done, and other factors. Define

$$\mu = (1/n) \sum_{i=1}^n \lambda p_i = \lambda \bar{p}, \quad (4.33)$$

$$c^2 = \frac{1}{n-1} \sum_{i=1}^n (p_i - \bar{p})^2 / \bar{p}^2 = \sigma_p^2 / \bar{p}^2.$$

It can be shown that

$$E \left[ \frac{1}{n} \sum_{i=1}^n X_i \right] = E(\bar{X}) = \mu, \quad (4.34)$$

$$E \left[ \frac{1}{n-1} \sum_{i=1}^n (X_i - \bar{X})^2 \right] = E(S^2) = \mu + c^2 \mu^2.$$

This shows that the variance among the simultaneous observed counts is a quadratic function of the mean, which is not the case when these counts are independent, identically distributed Poisson observations.

Therefore, under this model, which allows for possible misclassification of species, it is possible to have Poisson variation in the discharge channel and yet not to have Poisson variation in the observed data. The chi-squared tests done in Section 4.2, which show that the negative binomial model is a good fit to the data, will also serve to test this model. This is because the chi-squared test is based on fitting a variance that is a quadratic function of the mean, which is characteristic of this model. This model, however, enjoys many advantages in terms of the interpretation of the parameters.

#### 4.5.2 INTERPRETING THE PARAMETERS

The quantity  $c^2$  actually measures how far the observed data are from the Poisson model. That is, if  $c^2 = 0$ , then the observed data would be Poisson, and the data move away from the Poisson distribution as  $c^2$  increases. If data are classified by  $n$  sorters with respective species A classification probabilities  $p_1, \dots, p_n$ , the parameter  $c^2$  measures the level of agreement among the sorters, since  $c$  equals the coefficient of variation of the  $p_i$ 's.

It is clear from the definition that  $c^2 \geq 0$ . It is also true that  $c^2$  is bounded above, as will now be shown. From (4.33) we have

$$\begin{aligned} c^2 &= \frac{1}{n-1} \sum_{i=1}^n (p_i - \bar{p})^2 / \bar{p}^2 = \frac{n^2}{n-1} \frac{\sum p_i^2 - \frac{1}{n} (\sum p_i)^2}{(\sum p_i)^2} \\ &= \frac{n^2}{n-1} \left[ \frac{\sum p_i^2}{(\sum p_i)^2} - \frac{1}{n} \right]. \end{aligned} \quad (4.35)$$

Since  $p_i \geq 0$   $i=1, \dots, n$ , it follows that  $(\sum p_i)^2 \geq \sum p_i^2$ , because

$$\left( \sum_{i=1}^n p_i \right)^2 = \sum_{i=1}^n p_i^2 + \sum_{i \neq j} \sum p_i p_j \geq \sum_{i=1}^n p_i^2. \quad (4.36)$$

Therefore  $\left[ \sum p_i^2 / (\sum p_i)^2 \right] \leq 1$ , and from (4.35),

$$c^2 \leq \frac{n^2}{n-1} \left[ 1 - \frac{1}{n} \right] = n. \quad (4.37)$$

This upper bound is attained when one of the  $n$   $p_i$ 's equals 1 and all of the  $n-1$  other  $p_i$ 's are zero. We therefore have

$$0 \leq c^2 \leq n \quad (4.38)$$

for  $n \geq 2$ . If  $n=1$  formula (4.33) cannot be used, and an analytic bound cannot be used. The reasonable extrapolation is to take the upper bound as 1 if  $n=1$ . (The fewer sorters, the less the amount of variation due to classification.) Note that  $c^2$  can be identified with  $1/K$  in the negative binomial model (Section 4.2), since we have from (4.34) and Appendix II

$$\text{Classification variance: } \mu + c^2 \mu^2 \quad (4.39)$$

$$\text{Negative Binomial variance: } \mu + (1/K) \mu^2.$$

It is important to realize that the bounds on  $c^2$  can only be derived within the interpretations of the classification model. Without these interpretations no bounds can be derived on  $c^2$ .

If we define the "relative skill of the  $i^{\text{th}}$  sorter" to be

$$p_i^* = p_i / \sum_{j=1}^n p_j, \quad (4.40)$$

we can think of  $p_i^*$  as the species A classification probability of the  $i^{\text{th}}$  sorter relative to the other sorters. We can then express  $c^2$  as

$$c^2 = \frac{n^2}{n-1} \sum_{i=1}^n (p_i^* - \bar{p}^*)^2, \quad (4.41)$$

where  $\bar{p}^* = (1/n) \sum p_i^* = 1/n$ . Thus, if all of the sorters classify as species A with the same probability, as would occur if one person were to do all of the sorting under identical conditions, we would have  $c^2 = 0$ , and the observed data would follow a Poisson distribution. As the relative probabilities  $p_i^*$  become more discrepant,  $c^2$  will increase to its maximum value,  $c^2 = n$ .

A model with  $c^2 > 0$  does not imply that the persons identifying the organisms are unskilled, or that there are gross errors in classification. The quantity  $c^2$  measures relative, not absolute, consistency in classification. It is possible that all classifiers are highly skilled but not equally skilled and not absolutely consistent within and between workdays. Thus, on a relative scale, there can be differences in classification accuracy among samples, leading to a nonzero  $c^2$ . There is good reason to believe that there is variation among sorters and identifiers and/or among times and working conditions. Sorting and identification are difficult tasks, particularly in certain cases where two or more closely related species are hard to differentiate.

During the laboratory analysis of entrainment abundance data, two operations are performed: sorting, or separating the ichthyoplankton; and identification, or counting the number of each species-life stage combination. Several types of technicians are involved:

analyzers, who process all samples;

quality control checkers, who process samples determined according to a quality control plan; and

resolvers, who settle any discrepancies between analyzers and quality control checkers.

In sorting, the processing of a sample is considered defective or erroneous if the analyzer misses more than 10% of the ichthyoplankton in the sample. In identification, the processing of a sample is considered defective or erroneous if either of the following occurs on one or more species:

- (a) for the species or one of its life stages, the QC (quality control checker) count is 20 or less and the analyzer and QC counts differ by 3 or more;
- (b) the sum of the species-life stage percent errors over all life stages with QC counts above 20 is 10% or more, where the species-life stage percent error is defined as

$$(\text{analyzer count} - \text{QC count})/\text{QC count}.$$

Noticeable differences can exist between analyzer counts and QC counts without leading to a conclusion of defective processing. For example, consider a sample in which the analyzer counts for a species are 100 eggs, 20 yolk-sac larvae, and 6 post yolk-sac larvae, and the QC counts are 109 eggs, 18 yolk-sac larvae, and 8 post yolk-sac larvae; the processing of this species in this sample is considered nondefective. The margin of error allowed in sample processing without resulting in a conclusion of defective processing makes it clear that a degree of disagreement among analyzers processing the same sample is expected. This supports the position that there are differences in sorting and identification accuracy among samples.

The classification probability,  $p$ , of a particular sorter is actually composed of two pieces, and examination of these pieces can be helpful in coming closer to an optimal sampling scheme. For purposes of illustration, we will concentrate on one particular genus, *Morone*, and its two dominant species, striped bass (SB) and white perch (WP).

The classification model assumes that organisms can be classified into the correct genus without error. Suppose now that we are interested in the classification of SB. In the model described above, the quantity  $p$  is

$$p = \text{probability of classifying a } \textit{Morone} \text{ spp. as a striped bass.}$$

This probability is composed of two parts: the sorter's skill in identifying striped bass, and the abundance of striped bass in the sample. Define

$\pi_{SB}$  = proportion of sample of Morone spp. that are striped bass.

Then

$$p = \text{Prob}[\text{classifying SB as SB}] \pi_{SB} + \text{Prob}[\text{classifying WP as SB}] (1 - \pi_{SB}), \quad (4.42)$$

so we see that the probability  $p$  is a function of not only the skill of the sorter, but also the composition of the sample. If we assume that the skill of the sorter is constant over time, then the only variation in  $p$  comes from variation in  $\pi_{SB}$ .

The interpretation of the classification model leads to the bounds  $0 \leq c^2 \leq 1$ . From (4.34), we see that  $c^2$  is the coefficient of the quadratic variance term discussed in Section 4.2. While the quadratic variance model does not lead to an analytic bound on this coefficient, Cassie's comments (quoted in Section 4.2.1) and the empirical results for  $K = 1/c^2$  in this chapter support the view that  $c^2$  is generally less than 1, and is often substantially less than 1. Because  $c^2 \leq 1$  is true except on rare occasions, it will be useful to take  $c^2 = 1$  as an empirical conservative upper bound from time to time. This is not a true bound that can be established deductively, but only a value that the actual  $c^2$  usually falls below. The empirical bound  $c^2 = 1$  will be used as a value giving greater variance in (4.34) than almost all values of  $c^2$  observed in practice. Although this empirical conservative upper bound of 1 is a convenient reference point against which to measure an actual  $c^2$ , this use of  $c^2 = 1$  does not mean that an analytic bound for  $c^2$  exists. Both here and in later chapters, the empirical bound  $c^2 = 1$  will reflect a distribution of greater-than-usual variability, but its use will not mean that values of  $c^2$  exceeding 1 are impossible. Setting  $c^2 = 1$  can be described informally as a conservative approach to variability because the true value of  $c^2$  will most often be less than this. If a lack of replicate sampling data for a species-life stage combination leaves us unable to estimate  $c^2$ , using the empirical conservative bound  $c^2 = 1$  will be a reasonable, tentative first step.

## 4.5.3 EVALUATING THE CLASSIFICATION MODEL

The classification model hypothesizes that errors in the sorting and identification of ichthyoplankton have a major impact: they are responsible for transforming the sample counts from the Poisson distribution, which would be observed under error-free processing, into the negative binomial distribution. The appropriateness of this model can be examined by investigating the behavior of counts in situations known to be virtually free of errors in classifying specimens. If counts of this kind appear to be Poisson distributed, then the classification model gains credibility; however, if such counts do not show Poisson behavior, then they strengthen the case against the classification model.

By amalgamating, or lumping, observed counts to the family level, we can ensure that errors in classification are virtually absent. This is because a specimen from one family will almost never be mistaken for a member of another family. It is more common for a specimen to be misclassified as belonging to a species that is incorrect but is within the correct family. Another frequent outcome of identification is the labeling of a specimen as a member of a family without specifying its species. For example, a blueback herring (*Alosa aestivalis*) could be identified as an American shad (*Alosa sapidissima*), herring species (*Alosa spp.*), or herring family (Clupeidae). Such errors occur with some regularity, especially the failure to classify a specimen at the species level; an identifier may find it difficult to decide on the species of a particular organism, perhaps because of damage to the organism during entrainment. For example, as was noted in Section 4.3, Engraulids other than bay anchovy are extremely uncommon in the Hudson River, but many specimens are identified as anchovies (Engraulidae) rather than as bay anchovy (*Anchoa mitchilli*).

Using data amalgamated to the family level, therefore, has the effect of replacing counts that result from incomplete or inaccurate identification at the species level with complete precise counts of organisms in specified families. To evaluate the classification model, three families were examined:

Clupeids, consisting of herring family (Clupeidae), blueback herring (*Alosa aestivalis*), alewife (*Alosa pseudoharengus*), American shad (*Alosa sapidissima*), and herring species (*Alosa spp.*);

Engraulids, consisting of anchovies (Engraulidae) and bay anchovy (*Anchoa mitchilli*); and

Morone, consisting of white perch (*Morone americana*), striped bass (*Morone saxatilis*), and temperate bass species (*Morone spp.*).

Replicate lumped counts in these families were examined for the 1982 data from Roseton and the 1983 data from Indian Point described earlier in this chapter. The Poisson goodness-of-fit statistic  $S_p$  was computed, then  $\hat{K}^+$  was found and  $S_{NB}$  for the negative binomial distribution was computed. The results are shown in Table 4.9. Counts for each p.s.u. are plotted in Figures 4.4 for Indian Point and 4.5 for Roseton.

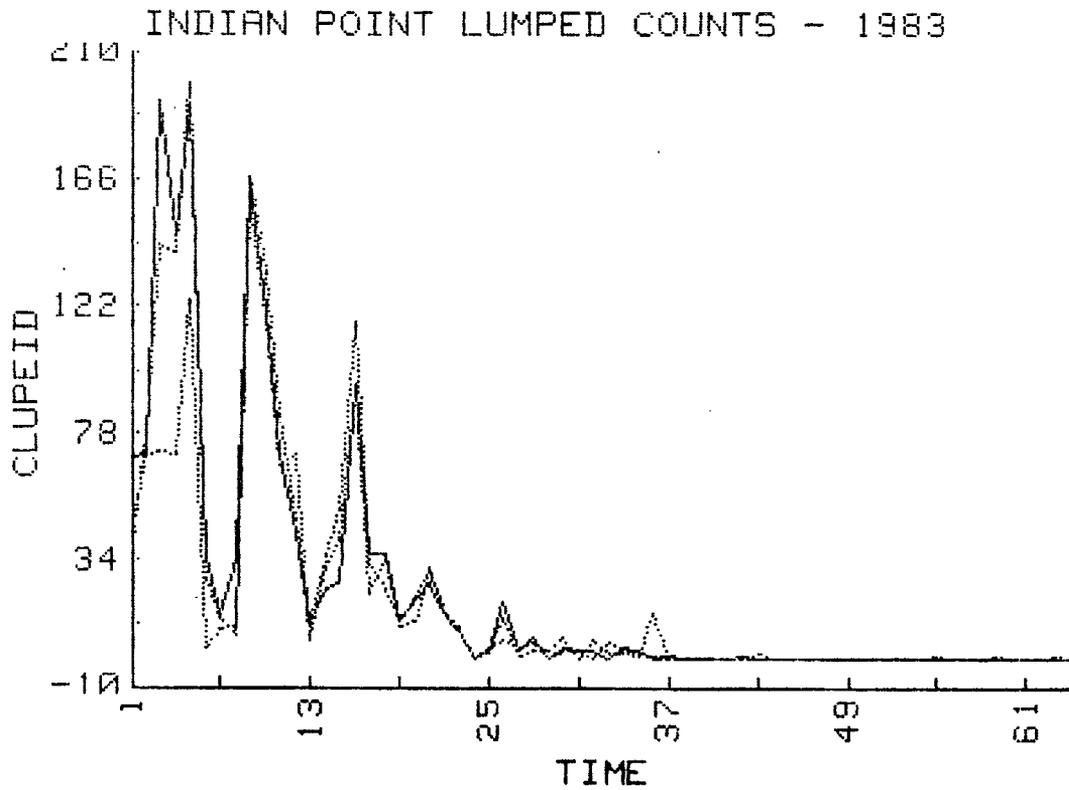
Table 4.9

Indian Point 1983 and Roseton 1982  $\chi^2$  Tests to  
Evaluate the Poisson and Negative Binomial Models

	<u>Indian Point</u>		
	<u><math>\hat{K}</math></u>	<u>Poisson <math>\chi^2</math></u> <u>(64 d.f.)</u>	<u>Neg Bin <math>\chi^2</math></u> <u>(63 d.f.)</u>
<u>Pump 1 vs Pump 2</u>			
Clupeid Larvae	14.26	169.76	79.51
Engraulid Larvae	64.52	205.63	93.45
Morone Larvae	4.28	398.93	60.31
<u>Pump 1 vs AUTOSAM</u>			
Clupeid Larvae	8.82	204.33	66.94
Engraulid Larvae	3.66	1368.57	85.26
Morone Larvae	2.96	521.61	67.15
<u>Pump 2 vs AUTOSAM</u>			
Clupeid Larvae	67.43	113.69	90.82
Engraulid Larvae	2.89	1576.78	83.88
Morone Larvae	7.79	314.43	85.72
<u>Roseton</u>			
	<u><math>\hat{K}</math></u>	<u>Poisson <math>\chi^2</math></u> <u>(22 d.f.)</u>	<u>Neg Bin <math>\chi^2</math></u> <u>(21 d.f.)</u>
Clupeid Eggs	21.88	24.81	22.54
Clupeid Larvae	571.07	52.14	38.46
Morone Eggs	78.73	29.48	21.22
Morone Yolk-Sac Larvae	6.86	155.50	31.96
Morone Larvae	12.09	175.98	16.25

Figure 4.4

Plots of Observed Lumped Data, Indian Point  
1983 Pumps vs. AUTOSAM



Dotted lines = pumps      Solid line = AUTOSAM

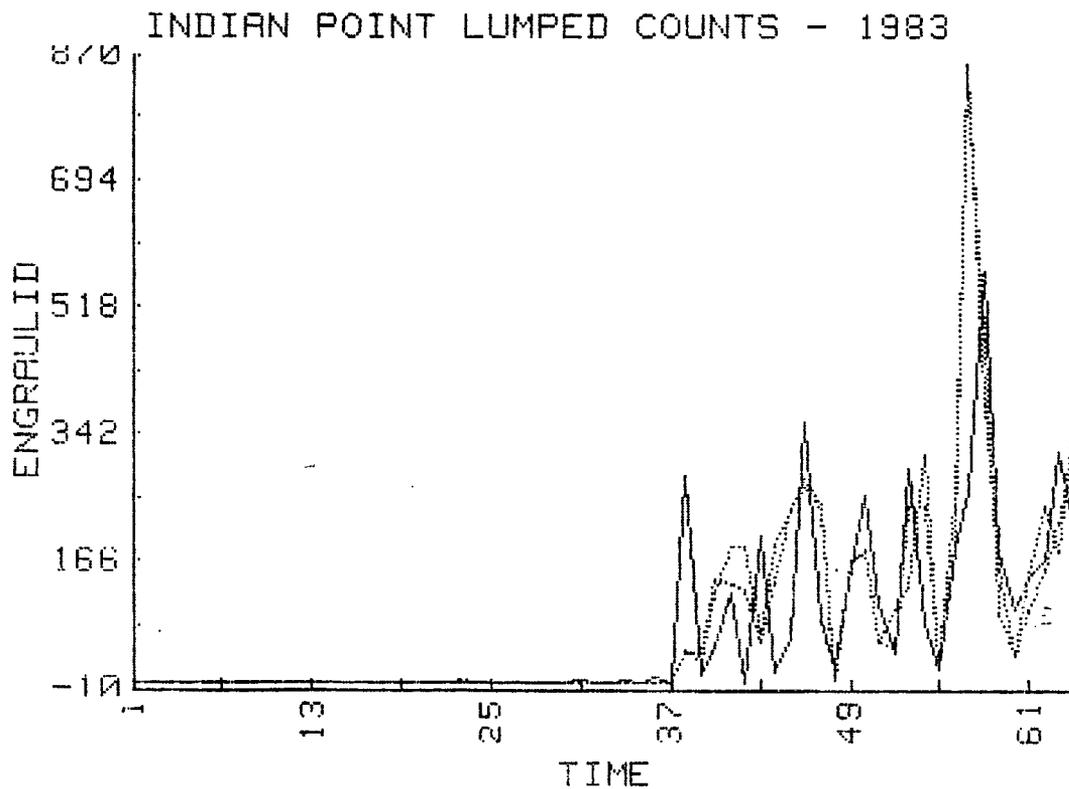
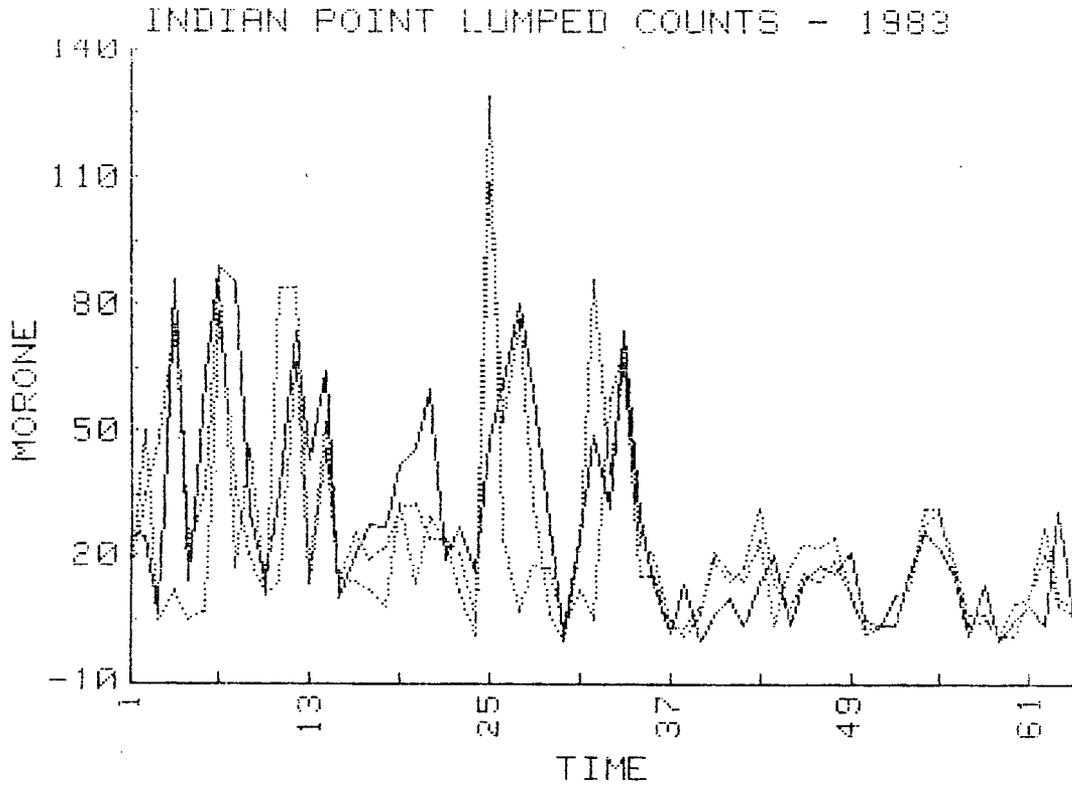


Figure 4.4 (continued)



4.57

Figure 4.5

Plots of Observed Lumped Data, Roseton 1982

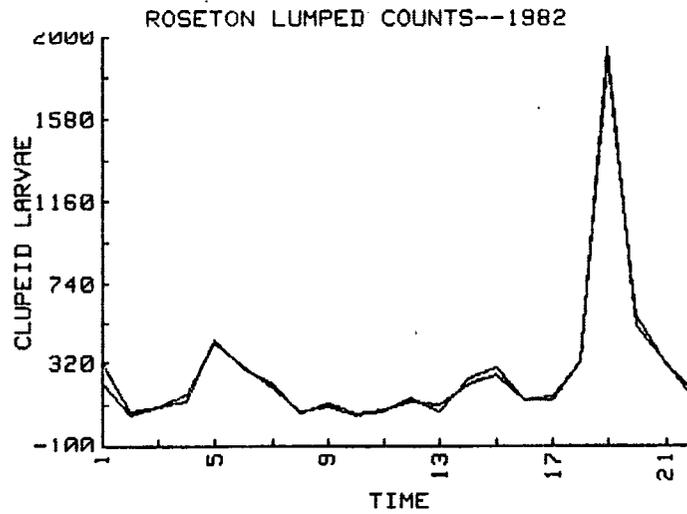
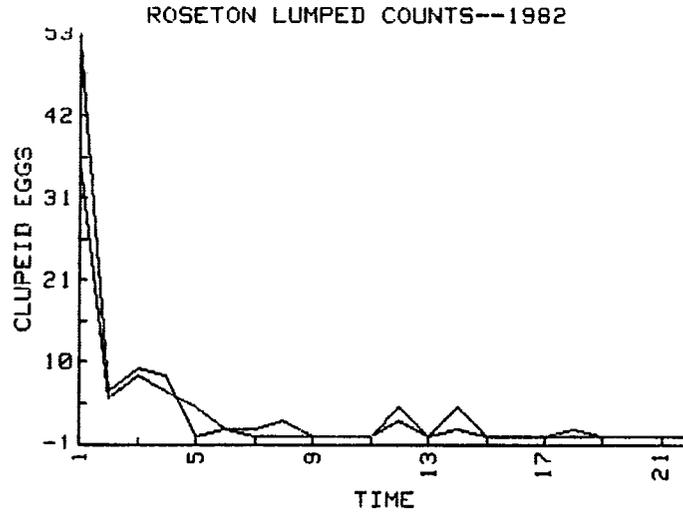
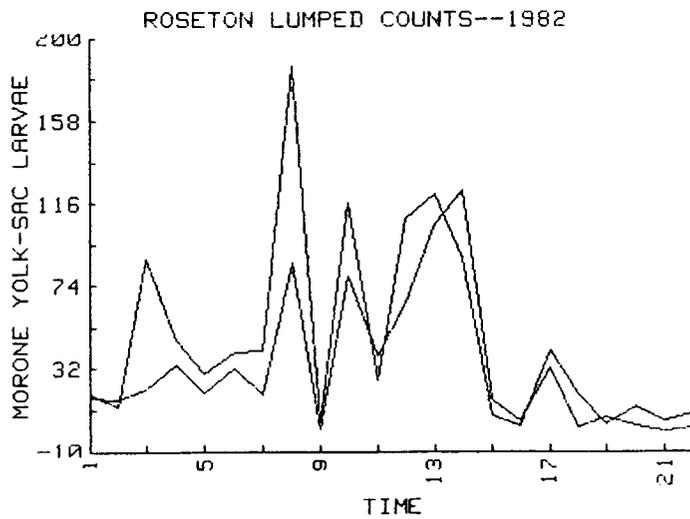
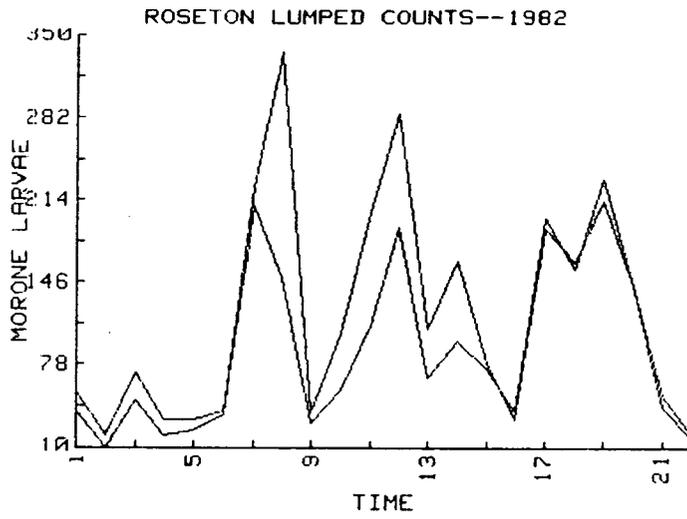
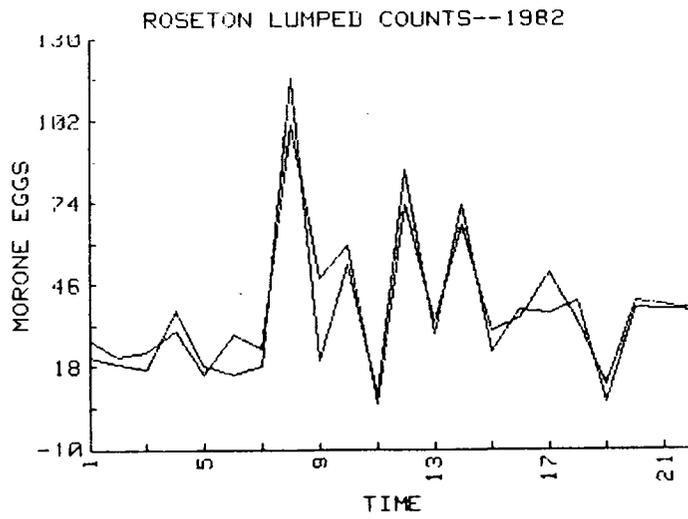


Figure 4.5 (continued)



The Indian Point triplicate data, analyzed for each pair of sampling locations (Pump 1 vs. Pump 2, Pump 1 vs. AUTOSAM, Pump 2 vs. AUTOSAM), decisively reject the hypothesis of replicate Poisson observations, as shown by the very large values of  $S_p$ . The negative binomial distribution's goodness of fit is marginal: about half of the values of  $S_{NB}$  lead to rejection of the negative binomial hypothesis at the .05 level. For the Roseton data, the Poisson distribution is rejected for three of the five family-life stage combinations examined; the negative binomial is rejected for one of these five.

The evidence against the Poisson distribution is very strong. Even at the family level, where classification is not causing any difficulty, the data do not conform to the Poisson distribution. Thus some problem other than classification must be largely or wholly responsible for the failure of the data to fit the Poisson distribution. We conclude that the lumped data do not support the classification model.

#### 4.5.4 THE PARAMETERS $K$ AND $c^2$

As has been pointed out in (4.39) and elsewhere,  $K$  and  $c^2$  are connected by the relationship  $c^2 = 1/K$ . We can benefit from this. For example, we could estimate  $c^2$  by using (4.18) to obtain  $\hat{K}^+$  and then taking  $1/\hat{K}^+$  as an estimate of  $c^2$ . Similarly, we could estimate  $K$  by  $1/\hat{c}^2$ , using  $\hat{c}^2$  from (VI.5) of Appendix VI. It is clear that little would be lost if we were to select either  $K$  or  $c^2$  for sole use, dropping the other.

It was not always evident during the development of this work, however, that  $K$  and  $c^2$  would turn out to be essentially interchangeable. It seemed at times that one of these might have substantial advantages over the other, based on some property of the classification model or similar considerations. For this reason, both  $K$  and  $c^2$  were retained and used in different parts of the study. For example,  $\hat{K}^+$  appears in the test of the negative binomial model in (4.17) and (4.18), while  $\hat{c}^2$  appears in the confidence interval given by (5.14) and (5.15). It would be routine to substitute  $c^2$  for  $1/K$  or  $K$  for  $1/c^2$  and to make the necessary adjustments if one wanted to do so. We have chosen not to go to this trouble.

#### 4.6 MODEL CHOICE FOR ENTRAINMENT OF JUVENILES

The entrainment of ichthyoplankton of the juvenile life stage is of particular interest. To some extent, this is because juveniles have survived through the earlier life stages, making them more valuable both ecologically and economically. The numbers of juveniles entrained at Bowline Point, Indian Point, and Roseton are lower than the numbers of early life stage ichthyoplankton entrained. (EA, 1984a, 1984b, 1984c). Nevertheless, estimation of juvenile entrainment is an important problem.

One point that is worth stressing is the fact that when a sample is analyzed in the laboratory and is found to contain no juveniles of a particular species, a useful piece of information about juvenile entrainment has been obtained. Zero counts, for juveniles or any other life stage for that matter, are informative about entrainment just as counts greater than zero are. In other words, zero counts should not be confused with a lack of information, such as might occur in the absence of sampling.

The methods of estimation to be treated in Chapter 5 can be applied to juveniles. These involve the derivation of confidence intervals for total entrainment under the negative binomial model and the Poisson model, which is obtained as the negative binomial model with the parameter  $c^2 = 0$ . If the use of the Poisson model can be justified, Poisson-based confidence intervals will be appropriate. These will be beneficial because they are shorter than the confidence intervals for any other negative binomial distribution. (Chapter 5 will contain more details on this point.)

The statement by Cassie, cited in Section 4.2.1, that the negative binomial distribution approximates to Poisson when the mean is small, suggests that the negative binomial and Poisson models may give similar results for juveniles.

To investigate whether this theory holds, we can test whether replicate sample counts of juveniles satisfy the Poisson model of Section 4.1 if appropriate data are available. The low level of juvenile entrainment makes it difficult to obtain suitable data, but recent replicate sampling has produced three such data sets. All three support the Poisson model for juvenile counts. The first data set, Table 4.10, is paired counts of bay anchovy juveniles from Bowline Point in 1983. When the Poisson test of Section 4.1 is applied to the 36 nonzero pairs (a paired count of (0,0) is called a zero count and is discarded for purposes of this test), the statistic  $S_p = 38.80$ , which is much smaller than the upper 5% point of a  $\chi^2_{36}$  distribution. In the Indian Point 1983 paired pump observations discussed in Section 4.4, paired observations of juveniles were obtained for white perch:

Table 4.10

Summary of Data Collected to Evaluate the Assumption  
of Random Sampling at the Bowline Point Plant, 1983  
(Reproduced from EA, 1984a: Table B-2)

Date	Replicate 1 Bay Anchovy					Replicate 2 Bay Anchovy				
	Vol. (m <sup>3</sup> )	Eggs	Larvae	Juv.	Other	Vol. (m <sup>3</sup> )	Eggs	Larvae	Juv.	Other
08 August	62.7	5	158	4	1	63.7	10	147	4	1
	45.8	0	143	5	2	46.9	2	72	2	1
09 August	58.0	0	106	2	0	57.4	0	71	0	1
	59.8	0	57	1	1	58.9	0	74	2	0
	60.0	0	97	1	2	62.6	0	141	3	0
	59.3	7	80	2	0	63.3	10	85	0	2
	67.4	13	109	0	1	69.4	7	119	2	1
	64.8	0	109	2	0	58.8	0	96	1	0
10 August	62.8	1	350	0	2	63.8	0	142	1	0
	63.1	5	224	0	2	65.2	4	175	0	1
	63.2	2	109	1	0	64.7	2	137	0	3
	62.1	22	214	0	2	61.2	4	80	0	1
11 August	58.6	12	84	0	1	55.5	14	45	1	0
12 August	77.6	0	37	1	1	71.4	1	9	1	2
	54.1	1	67	2	2	51.6	2	24	0	0
	53.9	4	46	2	1	54.4	1	24	2	0
13 August	76.4	1	44	0	0	77.8	0	38	0	1
15 August	55.6	0	118	0	1	56.9	0	142	0	0
	52.6	0	30	0	0	51.5	1	48	0	0
	77.5	0	116	1	0	77.9	0	93	1	1
16 August	64.0	0	137	1	4	66.1	0	59	0	4
	67.9	0	61	0	0	71.6	0	50	0	1
	66.2	0	49	0	0	67.1	0	76	0	1
17 August	64.3	0	37	0	2	61.3	0	9	0	0
	57.2	0	91	1	1	59.8	0	68	0	0
18 August	76.2	0	55	1	1	76.5	0	99	3	2
	75.5	0	91	0	1	77.2	0	63	0	1
	61.5	0	50	2	2	58.8	0	38	0	0
19 August	66.9	0	63	1	1	63.3	0	69	2	1
	64.1	0	28	1	0	63.4	0	34	2	0
	63.1	0	41	3	0	69.3	0	38	1	1
	73.1	0	44	2	1	71.1	0	26	0	1
22 August	69.5	0	53	3	0	64.8	0	54	5	0
	62.2	0	46	8	1	66.0	0	13	1	1
23 August	80.1	0	72	5	2	79.8	0	100	6	0
	60.0	0	24	4	0	59.5	0	36	3	0
24 August	56.9	0	19	1	0	60.5	0	8	1	0
	69.2	0	32	0	0	70.2	0	25	1	1
	70.7	0	29	2	0	73.8	0	34	0	0
25 August	69.6	0	13	0	1	68.2	0	31	1	0
	66.2	0	8	0	0	66.3	0	20	1	0
	77.0	0	12	0	1	70.5	0	43	0	2
26 August	55.0	0	16	0	1	52.7	0	24	0	0
	71.8	0	20	1	1	66.9	0	17	0	0
	61.0	0	14	0	0	64.6	0	13	0	0
	75.5	0	17	3	0	76.9	0	10	0	0
27 August	68.3	0	22	0	0	67.2	0	28	0	1
	75.5	0	18	1	0	74.1	0	15	0	0
	72.4	0	23	1	0	72.9	0	60	1	1
Total Count	—	73	3,483	65	39	—	58	2,922	48	54
Mean	65.23	1.49	71.1	1.33	0.80	65.17	1.18	59.6	0.98	0.67
Median	64.10	0.0	50.0	1.00	1.00	64.80	0.0	48.0	0.0	0.0
Standard Deviation	7.78	4.11	64.5	1.66	0.89	7.69	2.97	43.4	1.39	0.87
Coefficient of Variation (X)	11.9	275.8	90.7	124.8	111.2	11.8	251.7	72.8	141.8	129.8

1	1	1	2	4	1	3	5	2	1
1	0	0	2	1	3	5	3	1	1

$$S_p = 6.133$$

and for striped bass:

1	1	2	0	3	0	4	0
1	1	4	1	1	1	0	1

$$S_p = 8.667$$

The test statistic  $S_p$  was used here because the volumes of the paired samples satisfied  $v_i \approx v_i'$  for each p.s.u.  $i$ . Neither observed value of  $S_p$  was close to the corresponding critical value. Thus all three of these data sets are consistent with the Poisson model for juvenile counts. At Roseton juveniles were not collected in sufficient quantity to perform this test on any species.

The data sets of paired juvenile counts are all quite small, and the power of  $S_p$  to detect various departures from the Poisson distribution is unknown. Consequently, the support shown here for the Poisson structure of juvenile counts must be regarded as tentative. Further replicate sampling data must be gathered before a final conclusion can be reached.

#### 4.7 MODELING ENTRAINMENT ABUNDANCE USING PLANT OPERATING FACTORS

The task of modeling entrainment abundance as a function of plant out-ages has received little attention in this study for two reasons. First, the modeling of the discharge channel required a much greater effort than was originally expected. This effort has been well spent, though, for without accurate entrainment estimates it would be foolhardy to attempt any further modeling.

The second, and more important, reason that this task has not been given attention is the lack of sufficient data to test a model against. To see the severity of this problem, consider the following: The desired end result is a model relating entrainment abundance (ea) to plant operation (po) through a functional relationship

$$ea = f(po).$$

(4.43)

However, entrainment abundance is a function of both species abundance (sa) and flow rate in the discharge channel (fr), where species abundance results from a complex pattern involving the density of organisms in the plant's immediate vicinity, diurnal variation, weekly variation, and other factors. Thus a more realistic model is

$$ea = f(sa, fr). \quad (4.44)$$

Any attempt to model entrainment abundance as a function of plant operation while ignoring other variables cannot possibly lead to a realistic model. To remedy this situation, entrainment abundance must be measured over a variety of plant operating conditions while there is an attempt to hold the other variables constant. Part of this problem can be overcome by using these other variables as covariates; however, with the data presently available, the worth of such an endeavor is questionable. (Such an attempt should be based on a designed rather than an observational data set.) Therefore, although a model can be fitted with the data presently available, we feel that such a model can be given little credence due to the large number of uncertain relationships.

The model used to relate flow rate  $f$  to entrainment  $T$  over a time interval  $(0, t)$  is

$$T = \int_0^t \lambda(x) f(x) dx = f \int_0^t \lambda(x) dx = tf\bar{\lambda} \quad (4.45)$$

where  $\lambda(x)$  is the ichthyoplankton density in the discharge channel at the instant  $x$ ,  $0 < x < t$ , and  $\bar{\lambda}$  is the average density over this period  $(0, t)$  of constant flow rate  $f$ . The question at issue is whether

$\bar{\lambda}$  depends on  $f$ . This is critical for the estimation of entrainment reduction due to outages. Let  $T+T'$  denote the entrainment that would occur under full flow conditions, so  $T'$  is the entrainment reduction due to outage during the time interval  $(0, t)$ . Similarly, let  $f+f'$  denote the flow rate that would occur under full flow conditions. If  $\bar{\lambda}$  and the flow rate are independent, then

$$T' = tf'\bar{\lambda}; \quad (4.46)$$

$T'$  can be estimated from the known values of  $t$  and  $f'$  and the estimated value of  $\bar{\lambda}$ , which is easily obtained from the estimated true entrainment, as

$$\hat{T}' = tf'\hat{\bar{\lambda}} = tf'(\hat{T}/tf) = (f'/f)\hat{T}. \quad (4.47)$$

If, on the other hand,  $\bar{\lambda}$  is a function  $\lambda(f)$  of the flow rate, then (4.46) must be replaced by

$$T' = t[(f+f')\bar{\lambda}(f+f') - f\bar{\lambda}(f)]; \quad (4.48)$$

to estimate  $T'$  here, the average density of organisms at full flow,  $\bar{\lambda}(f+f')$ , must be estimated from the known  $f$  and  $f'$ , the estimated value of  $\bar{\lambda}(f)$ , and the functional relationship connecting  $\bar{\lambda}$  to the flow rate. This is a much more complex estimation process than (4.47).

In the face of such uncertainty, it is usually the case that the simplest solution is the most reasonable. Thus, the linear extrapolation of (4.47) seems to be the most reasonable approach until experimental data from a designed study become available.

An attempt was recently made to perform a designed study of this kind at the Roseton Generating Station (EA, 1984c, Sec. 5.7). To examine the relationship between entrainment densities and cooling water flow rates, continuous sampling was scheduled immediately before and after planned flow rate changes. A problem with the Roseton flow rate study is that it took two hours for the flow rate to change from its high setting to its low one (or vice versa); in two hours the river conditions can change a great deal, which makes it difficult to draw inferences from the comparison of densities before and after the flow rate change. If the change could be made over a shorter transition period, say 15 minutes instead of two hours, this difficulty would not be as great.

Another study of the relationship between plant flow and ichthyoplankton density was recently performed at the Bowline Point Generating Station (EA, 1984a, Sec. 3.4, 4.6). Samples were collected at night, when densities were greatest. Analysis centered on bay anchovy, the only species expected to occur in large numbers. Two patterns of pumping were tested: 2 pumps to 3, then back to 2; and 3 pumps to 2, then back to 3. Samples were collected on four of the scheduled ten sampling dates. The study's results "seem to indicate that flow and density are inversely related, [although] this conclusion should not yet be too strongly embraced." (EA, 1984a, p. 4-17).

In order to address this question directly, it would be desirable, but impossible, to measure entrainment simultaneously at two or more different flow rates. Recommendations for special studies to determine whether entrainment densities and cooling water flow rates are related will be made in Section 9.3.

## 5. ESTIMATION OF ENTRAINMENT ABUNDANCE AND ENTRAINMENT REDUCTION

### 5.0 INTRODUCTION

The performance of a sampling design can be assessed by statistical criteria that evaluate how well the design achieves its objectives. The main objective of a sampling design is the estimation of two quantities: the total entrainment abundance at a plant over the entire entrainment season and the effect of outages in reducing entrainment. Statistical properties of estimators can therefore be used to make comparisons among the estimators associated with different sampling designs. A good sampling design will lead to estimators of entrainment abundance and entrainment reduction that have low bias and low variance, relative to the estimates from other designs, over a variety of conditions. In particular, a good sampling design must be able to deal with rapid changes in the level of entrainment over a short period of time.

Our goal is to obtain sampling designs that give the most accurate estimates possible for a fixed level of sampling effort. Changing the cost of sampling judged acceptable would lead to adjustment of the design, resulting in a different design appropriate for the new level of sampling effort.

### 5.1 PERFORMANCE CRITERIA CONSIDERED

Performance criteria will be discussed from the perspective of estimation. Our objective is to estimate both entrainment abundance and the reduction in entrainment due to outages, over the entire entrainment season. Estimation is to be as accurate as possible for a fixed level of cost, over a wide range of conditions. The performance of a sampling design will therefore be judged on the basis of accurate estimation. This makes it crucial to keep in mind, as Section 3.0 pointed out, that a sampling design must be viewed as an overall process that includes field collection of samples, laboratory analysis of these samples, and statistical estimation based on the laboratory data. In this framework it makes sense to investigate the quality of estimation resulting from a given sampling design.

Low bias is one desirable feature of a design. Bias in a design may produce estimates having a systematic tendency either to be too high (positive bias) or to be too low (negative bias). A positively biased design gives estimates of entrainment that are, on the average over a long series of entrainment seasons, higher than the (unknown) actual level of entrainment. A negatively biased design does the opposite. A design is called

## 5.2

unbiased if its entrainment estimates are correct on the average. For an unbiased design, entrainment will be overestimated in some years and underestimated in others, but over a long series of seasons the average estimate will approach the average true level of entrainment. Formally, bias is defined in terms of the expected value of the estimator  $\hat{T}$  of the true entrainment level  $T$ :

$$\begin{aligned} E[\hat{T}] > T & \quad \text{is positive bias} \\ E[\hat{T}] = T & \quad \text{is zero bias or unbiasedness} \\ E[\hat{T}] < T & \quad \text{is negative bias.} \end{aligned} \tag{5.1}$$

A possibility that must be considered is the presence of bias in entrainment estimates due to the physical characteristics of the sampling gear. Potential sources of bias are in the location of the intake orifice for the pumping samples, the velocity of water at the intake orifice, and the direction of the intake orifice. Intakes for the pumps were more or less fixed and sampled from the same point in the plant discharge. The assumption of random mixing of the ichthyoplankton in the discharge water is necessary in order to consider the sampling to be representative. Accurate estimates of the entrained ichthyoplankton are also dependent upon the plant discharge water velocity matching the water velocity at the sampler intake. There could conceivably be a positive bias if the water velocity at the sampler intake exceeds the plant discharge water velocity, a negative bias if the opposite is the case. The direction of the sampler intake orifice will most likely cause a negative bias unless facing directly into the plant discharge flow. Open-ended pipes pointing directly downward into the discharge would result in underestimation of the ichthyoplankton entrainment. Details are given by Martin Marietta (1981).

Low variance of entrainment estimates is another important feature that any good design must have. Variance measures the degree to which an estimator is likely to differ if estimation takes place many times under identical conditions, so high variance indicates a lack of precision in the estimation procedure. Low variance means that if several independent samples were drawn from a single random process and each sample were used independently to obtain an estimate (e.g., for entrainment abundance), the several independent estimates of the same underlying quantity would be very close to each other. This allows us to have confidence that the one sample we actually draw in practice will give an estimate that is highly consistent with other samples we might have drawn. Conversely, high variance in an estimator means that several independent samples from a process are likely to produce estimates differing greatly from one another. This dispersion implies that the estimator cannot do a precise job of estimating the quantity of interest.

It is sometimes convenient to consider the standard deviation of an entrainment estimator, which is the square root of the variance of the estimator. The standard deviation is expressed in the same units of measurement as the observations, which often puts it on a scale that is appropriate for interpreting its magnitude. The variance and standard deviation are equivalent pieces of information, since knowing either one makes it easy to calculate the other.

Another useful measure of variation is the coefficient of variation or CV, which is the ratio of standard deviation to mean. This measure of relative dispersion is often expressed as a percentage,  $100(\text{standard dev})/(\text{mean})\%$ . It measures variability, as expressed by the standard deviation, but is scaled by the size of the mean. Thus the CV gives the size of the standard deviation as a fraction (or multiple) of the mean. One particularly useful property of the CV is that it enables us to calculate the probability of erring by any specified percentage. For example, if the CV of an entrainment estimator is 25 percent, then we can be 95 percent certain that this estimator will not err by more than 50 percent, and 80 percent certain that the estimator does not differ from its target by more than 33 percent. In general, the probability of erring by at most  $100\epsilon\%$  is (approximately)

$$1 - 2\Phi(-\epsilon/\text{CV}), \quad (5.2)$$

where  $\Phi(z)$  is the standard normal distribution. Since  $1 - 2\Phi(z) = .95$  for  $z = -1.96 \approx -2$ , then with 95 percent certainty the fractional error of the estimate will not exceed twice the CV.

The coefficient of variation can be viewed as a scaled version of the standard deviation; instead of expressing the level of variability in the same units as the observations, however, the CV expresses variability relative to the mean. This is useful as a criterion for comparing estimators and sampling designs because, for distributions such as the Poisson and negative binomial, the standard deviation increases as the mean increases. The fraction by which an estimator is likely to be in error, which can be found from (5.2), is often more important than the absolute magnitude by which the estimator is likely to be in error, which can be found from the standard deviation.

Both bias and variance should be considered in evaluating an estimator, as it is possible for an estimator to behave well on one of these criteria but poorly on the other. For example, if we are concerned only with achieving zero bias and we ignore variance, an estimator based on data gathered on a single randomly chosen day during the entrainment season can be unbiased, but any such estimate will have an enormous variance, making it practically useless. Conversely, if we are concerned only with minimizing variance and we ignore bias, an estimator that ignores the data and gives a fixed value, e.g., the previous year's estimated entrainment level, has a variance of zero--any sample collected this year leads to exactly the same estimation result--but has a huge bias because of year-to-year fluctuation in entrainment. As these examples show, it is easy for an estimator to perform well in terms of either bias or variance by ignoring the other criterion; to be useful, an estimator must perform well in both of these areas.

A standard approach to estimation that combines variance and bias is to introduce the mean-squared error of an estimator. As the name suggests, this is the average value, over a long series of replications, of the squared difference between the estimator and the quantity it seeks to estimate. This is the long-run average of the square of the distance between the estimator and its target. The relationship connecting the mean-squared error (MSE), variance, and bias of an estimator is

$$\text{MSE} = \text{variance} + (\text{bias})^2. \quad (5.3)$$

Thus an estimator that has low MSE is performing well as measured by a combination of variance and bias.

An unavoidable complication is that variance, bias, and MSE are affected by entrainment conditions. That is, the general level of entrainment, the distribution of entrainment across the season, and other similar factors are involved in determining the variance, bias, and mean-squared error of estimation for any design. If we knew the values of such factors, we could use this knowledge in comparing designs, but unfortunately these values are almost always unknown. It is therefore necessary to seek designs that do relatively well (compared to other designs) over a broad range of entrainment patterns. In particular, any good design must handle, as well as possible, entrainment patterns including several steep peaks of entrainment. These occur at times that can be predicted only approximately, perhaps to within a period of two to three weeks. Such peaks may be for different taxa, different life stages of a particular taxon, or both.

Note that no sampling design can be optimal under all conditions. To see this, observe that if we knew entrainment to be concentrated within a single week, any optimal design would concentrate all sampling effort within that week. Yet no design of this type merits serious consideration, because its optimality under the conditions of a one-week entrainment season is overshadowed by its extremely poor performance under virtually all other conditions, including those we are likely to encounter.

It would be mathematically convenient to restrict attention to the class of unbiased estimators. There are several reasons for this:

- (1) It limits the class of estimators to be dealt with, giving us fewer candidates to compare.
- (2) Since all estimators in this class are unbiased under all possible entrainment conditions, it is natural to judge estimators by comparing their variances under various entrainment conditions. This is because, for any unbiased estimator, we have

$$\begin{aligned} \text{MSE} &= \text{variance} + (\text{bias})^2 \\ &= \text{variance} \end{aligned} \tag{5.4}$$

since bias = 0. Therefore, in dealing with the class of unbiased estimators, comparisons of MSE reduce to comparisons of variance.

- (3) Unbiased estimators can often be derived without any distributional assumptions, as in Cochran (1977, Ch. 10).

Unfortunately, the restriction to unbiased estimators has the major drawback that it eliminates from consideration many potentially useful estimators. An estimator with a small amount of bias, perhaps a few percent or even less, can have a variance that is much lower than the best (minimum-variance) unbiased estimator. A slightly biased estimator of this kind has much to recommend it and should not be discarded without consideration. As will be seen in later chapters of this report, the estimators that are reasonable candidates exhibit little or no bias, so the MSE comparisons reduce to comparisons of variance.

Thus far, our discussion has assumed that the level of sampling effort, as measured by cost, is fixed. Variance, bias, and MSE of competing sampling designs can be compared under this fixed-cost assumption. There is another perspective that can be useful in thinking about design comparisons: the sampling effort needed to obtain a given degree of accuracy. Among designs that are unbiased or only slightly biased, the best designs are those that require the least sampling effort to achieve a specified standard error or variance for the estimates of interest. Sampling effort will not be known in advance if certain aspects of the sampling process, such as the number of ichthyoplankton collected in samples throughout the entrainment season, are random. In this case, the expected value of sampling effort can be used in place of sampling effort.

## 5.2 PERFORMANCE CRITERIA SELECTED

Two criteria have been chosen for the comparison of sampling design performance. The first is the bias of entrainment abundance and entrainment reduction estimates. This bias should be zero or nearly zero. The second is the variance of these estimates for a fixed level of sampling effort, or equivalently, the amount of sampling effort required for the estimates to attain a specified level of accuracy. Where appropriate, we will consider the coefficient of variation, which gives the level of accuracy relative to the mean, rather than the variance or standard deviation, which gives the absolute level of accuracy.

A good sampling design must perform well on both of these criteria over a broad range of entrainment conditions, including situations of rapidly fluctuating entrainment abundance.

The absence of substantial bias is highly desirable in any estimator of entrainment levels. A systematic tendency to overestimate entrainment abundance, for example, would create obvious problems. A systematic tendency in the other direction would be even worse, as it would raise questions of whether entrainment abundance was being underestimated intentionally for purely nonstatistical reasons. The overwhelmingly preferable behavior of an estimator is unbiasedness, guaranteeing that estimates are correct on average, so overestimates and underestimates will average out over time. However, if more precise estimates can be obtained by allowing a small amount of bias to be present, this tradeoff may be justified.

Minimizing the variances of entrainment estimates for a specified amount of sampling effort is the second aspect of sampling design performance. Alternatively, this can be thought of as minimizing the amount of sampling effort necessary to achieve a particular level of accuracy in the estimation, that is, to reduce the estimated variances of the entrainment estimates to specified levels. The variance or standard error of an estimate provides a measure of the precision of the estimate, with low variance reflecting a high degree of precision. An optimal sampling design gives the greatest precision possible if the amount of sampling effort is fixed in advance; it requires the minimum necessary sampling effort if the precision of the estimates is fixed in advance.

### 5.3 SOME ASPECTS OF THE ESTIMATION PROBLEM

One of the major goals of this study is to provide accurate point and interval estimates of the total number of entrained ichthyoplankton. Using the models developed by both empirical and theoretical means, this can be accomplished. Furthermore, by inspection of the form of the interval estimates, it is possible to get some idea of how to optimize the sampling and estimation process. As will be seen, it is impossible to complete a total optimization of the process because of the lack of certain necessary information; however, with certain assumptions (to be specified later), a design can be recommended. With further study, it may be possible to verify these assumptions.

The estimation problem treated here has two aspects. One is to estimate the entrainment actually achieved through the realized plant discharge regime. The other is to estimate the reduction in entrainment due to the difference between the potential and the realized discharge. Both aspects pertain to the actual temporal sequence of ichthyoplankton densities in the source water during the year in question, since the realized entrainment at any given time is a sample from that source and the potential entrainment is simply a larger sample from the same source. In other words, the realized entrainment is a sample from the potential entrainment, and hence is subject to subsampling error.

(Experiments have been undertaken in the past to test whether realized entrainment is indeed a representative sample of potential entrainment. This was tested by controlled changes in the discharge rate over short intervals to determine whether the density of entrained organisms changed with the discharge rate (EA, 1984a, Sec. 4.6; 1984c, Sec. 5.7).)

While actual density in the source water does vary through time according to some unknown stochastic process, our direct concerns are not with that process but with the actual density. Indirectly, we must be concerned with the temporal pattern over the year as it affects our sampling plan and sampling error, but chance variations in actual density are incorporated into both the actual and potential entrainment for that year and are thus included in the estimation target. If each entrained organism had a price tag, the total cost for the year would be determined by the realized entrainment, not by the process that led to the realized entrainment.

It has been suggested that time series techniques for investigating the underlying stochastic process can be used directly in estimating entrainment abundance at power plants. A recent paper by Madenjian and Jude (1983) takes this time series approach to the estimation of entrainment abundance. Their approach attempts to explain the underlying process variation, which, as stated in the preceding paragraph, is not of direct concern. Therefore, the time series approach is not appropriate for the estimation problems addressed in this study. A fuller explanation appears in Appendix VII.

#### 5.4 CONFIDENCE INTERVALS

For convenience, we will deal with each species-life stage combination separately. Thus, suppose we observe  $X_1, X_2, \dots, X_N$ , the observed counts of a particular species-life stage combination, over  $N$  p.s.u.'s. If we define

$\lambda_i$  = number of organisms per unit volume in p.s.u.  $i$ ,

$v_i$  = volume of water sampled in p.s.u.  $i$ ,

$V_i$  = volume of water in discharge channel in p.s.u.  $i$ ,

then the total number of organisms entrained during the  $N$  p.s.u.'s is

$$T = \sum_{i=1}^N V_i \lambda_i. \quad (5.5)$$

The quantity  $\lambda_i$  is the mean density in the discharge channel during p.s.u.  $i$ , hence  $V_i \lambda_i$  is the true total entrainment during p.s.u.  $i$ . Under the assumption of randomness in the discharge channel, the observed  $X_i$  is a realization of a random variable whose mean is  $v_i \lambda_i = \mu_i$ . Therefore,

the total number of organisms entrained can be expressed in terms of the  $\mu_i$ 's as

$$T = \sum_{i=1}^N (V_i/v_i) \mu_i \quad (5.6)$$

For now, assume that the sampling fraction  $v_i/V_i$  for p.s.u.  $i$  is the same for all  $i=1, \dots, N$ . This assumption will be dropped in Section 5.9. (It does, however, have some basis in fact. The quantity  $V_i$  is not measured directly, but is calculated from engineering specifications of the pumps involved. This flow rate is constant, or virtually constant, over long intervals of time, allowing us to take  $V_i \equiv V$  for all  $i$  in such intervals. Furthermore, when the sampling is done using constant velocity pumps, we also have  $v_i \equiv v$  for all  $i$ .) Under these conditions,

$$T = (V/v) \sum_{i=1}^N \mu_i, \quad (5.7)$$

and we now concentrate on a confidence interval for  $\sum_{i=1}^N \mu_i$ . The observed counts  $X_1, X_2, \dots, X_N$  can be assumed to have a variance that is a quadratic function of their means, i.e.,

$$\text{Var}(X_i) = \mu_i + c^2 \mu_i^2, \quad (5.8)$$

where the parameter  $c^2$  has been discussed in Sections 4.2 and 4.5. Simultaneous replicate sampling is necessary in order to estimate the parameter  $c^2$ ; however, as an alternative approach applicable when there has been no replicate sampling, the empirical conservative bound of Section 4.5.2,  $c^2 \leq 1$ , gives

$$\text{Var}(X_i) \leq \mu_i + \mu_i^2. \quad (5.9)$$

Using the fact that  $\sum X_i$  is a point estimate for  $\sum \mu_i$ , two different confidence intervals can be constructed. Any confidence interval for  $\sum \mu_i$  gives rise directly to a corresponding confidence interval for  $T$ : if

$(l_1, l_2)$  is a confidence interval for  $\sum \mu_i$ , then  $(vl_1/v, vl_2/v)$  is the corresponding confidence interval for  $T$ .

#### 5.4.1 CONFIDENCE INTERVAL 1

Since

$$\text{Var}\left(\sum_{i=1}^N X_i\right) = \sum_{i=1}^N \mu_i + c^2 \sum_{i=1}^N \mu_i^2, \quad (5.10)$$

estimation of this quantity would lead to a natural confidence interval for  $\sum \mu_i$ . Under the assumption that the  $X_i$ 's are independent with  $E(X_i) = \mu_i$  and  $\text{Var}(X_i) = \mu_i + c^2 \mu_i^2$ , it follows by substitution that

$$\begin{aligned} E(X_i^2) &= \text{Var}(X_i) + [E(X_i)]^2 \\ &= \mu_i + (c^2 + 1)\mu_i^2 \end{aligned} \quad (5.11)$$

and hence

$$\begin{aligned} E\left[\sum_{i=1}^N \frac{X_i(1+c^2X_i)}{1+c^2}\right] &= \frac{1}{1+c^2} \sum_i E(X_i) + \frac{c^2}{1+c^2} \sum_i E(X_i^2) \\ &= \frac{1}{1+c^2} \sum_i \mu_i + \frac{c^2}{1+c^2} \sum_i (\mu_i + (c^2+1)\mu_i^2) \\ &= \sum_{i=1}^N \mu_i + c^2 \sum_{i=1}^N \mu_i^2, \end{aligned} \quad (5.12)$$

giving us two methods of estimating the variance of  $\Sigma X_i$ . First, recalling from Section 4.5.2 the empirical bound  $c^2 \leq 1$ , we can estimate the variance conservatively by taking  $c^2 = 1$ , which leads to

$$\hat{S}^2 = \sum_{i=1}^N \frac{X_i(1 + X_i)}{2}. \quad (5.13)$$

Second, if the estimate  $\hat{c}^2$  of (VI.5) in Appendix VI is calculated from replicate sampling data, the variance can be estimated by

$$\hat{S}^2 = \sum_{i=1}^N \frac{X_i + \hat{c}^2 X_i^2}{1 + \hat{c}^2}. \quad (5.14)$$

In either case, a confidence interval for  $\Sigma \mu_i$  is then given by

$$\sum_{i=1}^N \mu_i \in \sum_{i=1}^N X_i \pm b\hat{S}, \quad (5.15)$$

where  $b$  is a constant chosen to give the desired confidence level, e.g., 90% or 95%.

#### 5.4.2 CONFIDENCE INTERVAL 2

An alternative confidence interval can be derived by noting that

$$\begin{aligned} \text{Var}\left(\sum_{i=1}^N X_i\right) &= \sum_{i=1}^N \mu_i + c^2 \sum_{i=1}^N \mu_i^2 \\ &= \sum_{i=1}^N \mu_i + (c^2/N) \left[ \sum_{i=1}^N \mu_i \right]^2 + c^2 \sum_{i=1}^N (\mu_i - \bar{\mu})^2. \end{aligned} \quad (5.16)$$

If  $\sum_{i=1}^N (\mu_i - \bar{\mu})^2$  is negligible as compared with

$\left[ \sum_{i=1}^N \mu_i \right]^2$ , which will be the case if the  $\mu_i$ 's are close together, then we

have

$$\begin{aligned} \text{Var} \left[ \sum_{i=1}^N X_i \right] &= \sum_{i=1}^N \mu_i + (c^2/N) \left[ \sum_{i=1}^N \mu_i \right]^2 \\ &= \tau + c^2 \tau^2 / N, \end{aligned} \quad (5.17)$$

where we define  $\tau = \sum_{i=1}^N \mu_i$ . Taking  $c^2 = 1$  as an empirical upper bound, a confidence interval for  $\tau$  is then the set of all values of  $\tau$  for which

$$\frac{\left[ \sum_{i=1}^N X_i - \tau \right]^2}{\tau + \tau^2 / N} \leq a^2 \quad (5.18)$$

or, equivalently,

$$\sum_{i=1}^N \mu_i \in \frac{2 \sum_{i=1}^N X_i + a^2 \pm a \sqrt{4 \sum_{i=1}^N X_i + a^2 + (4/N) \left[ \sum_{i=1}^N X_i \right]^2}}{2(1 - a^2/N)}, \quad (5.19)$$

where  $a^2$  is a constant chosen to give the desired confidence level. This confidence interval is usable only if  $a^2 < N$ , that is, the probability constant  $a^2$  must be less than the number of p.s.u.'s. If this does not hold, then the set of  $\tau$  satisfying the constraint is not an interval.

## 5.5 EVALUATION OF CONFIDENCE INTERVALS

Confidence intervals are evaluated according to two criteria, confidence

level and length. Informally speaking, one wants to get the greatest amount of confidence for the smallest length.

The two confidence intervals given here were compared by a simulation study. Five different values of the season length  $N$  were examined:  $N = 3, 6, 8, 16,$  and  $24$ . The number of p.s.u.'s in the season,  $N$ , may be thought of as the sample size in the confidence interval computation, so we should expect better performance as  $N$  increases. The true, unobserved means  $\mu_i$  of the observed counts  $X_i$  were drawn from three different populations: the uniform distribution on the interval  $(1,10)$ , the uniform distribution on  $(1,100)$ , and the uniform distribution on  $(1,200)$ . These distributions represent entrainment sampling from low, moderate, and high organism densities, respectively. Each observed count  $X_i$  was simulated as an observation from a negative binomial distribution with mean  $\mu_i$  and variance given by (5.8) with  $c^2 = 1$ , which is the empirical conservative bound for  $c^2$  described in Section 4.5.2. This choice of  $c^2$  was made to provide a situation with high variance of observed counts, in which the confidence intervals for  $\mu_i$  will be wide; smaller values of  $c^2$  would lead to shorter (better behaved) confidence intervals.

These five values of  $N$  and three populations for the mean  $\mu_i$  result in 15 ( $= 5 \times 3$ ) combinations. For each of these, 1000 repetitions were performed as follows: (1)  $N$  means  $\mu_i$ ,  $i=1, \dots, N$ , were drawn from the uniform distribution specified as the parent population of  $\mu_i$ ; (2) for each value of  $\mu_i$  drawn, a random count  $X_i$  was simulated, having a negative binomial distribution with mean  $\mu_i$  and corresponding variance (the simulation of negative binomial random variables is discussed in Appendix II); (3) the  $N$  means  $\mu_i$  and  $N$  observations  $X_i$  were used to calculate the true mean  $\Sigma\mu_i$ , the confidence intervals given by (5.15) and (5.13) for several values of the constant  $b$ , and the confidence intervals given by (5.19) for several values of the constant  $a^2$ ; (4) for each value of  $b$  in confidence interval 1, given by (5.15), and each value of  $a^2$  in confidence interval 2, given by (5.19), the length of the confidence interval was found and it was determined whether the true mean  $\Sigma\mu_i$  was covered by the confidence interval.

From the 1000 repetitions of these steps, the average length of the confidence interval and the probability of covering the true mean were calculated for each value of the constant,  $b$  in confidence interval 1 and  $a^2$  in confidence interval 2. These average lengths and coverage probabilities are reported for all five values of  $N$  and the first, second, and third populations for  $\mu_i$  in Tables 5.1, 5.2, and 5.3, respectively.

Table 5.1

Simulation Study of Confidence Intervals

$$\text{for } \sum_{i=1}^N \mu_i :$$

Observed Coverage Probabilities and  
Average Lengths When  $\mu_i$ 's Are Uniform (1,10)

<u>Sample Size N</u>	<u>Constant</u>	<u>Coverage Probability</u>		<u>Average Length</u>	
		<u>Interval 1</u>	<u>Interval 2</u>	<u>Interval 1</u>	<u>Interval 2</u>
3	1.50	.74	.90	25.63	113.96
	2.00	.80	--	34.17	--
	2.50	.83	--	42.71	--
	3.00	.85	--	51.25	--
6	1.50	.76	.87	39.29	63.88
	2.00	.84	.95	52.39	159.95
	2.50	.89	--	65.48	--
	3.00	.92	--	78.58	--
8	1.50	.77	.85	46.77	64.21
	2.00	.86	.95	62.35	123.21
	2.50	.90	.98	77.94	352.53
	3.00	.92	--	93.53	--
16	1.50	.80	.82	69.92	76.38
	2.00	.89	.92	93.22	116.74
	2.50	.93	.97	116.53	179.72
	3.00	.96	.99	139.83	300.63
24	1.50	.82	.81	86.66	87.99
	2.00	.90	.92	115.54	127.63
	2.50	.94	.97	144.43	179.84
	3.00	.97	.98	173.31	255.51

Coverage Probabilities and Average Lengths are  
calculated based on 1000 simulations

Table 5.2  
Simulation Study of Confidence Intervals  
 for  $\sum_{i=1}^N \mu_i$ :  
Observed Coverage Probabilities and  
Average Lengths When  $\mu_i$ 's Are Uniform (1, 100)

<u>Sample Size N</u>	<u>Constant</u>	<u>Coverage Probability</u>		<u>Average Length</u>	
		<u>Interval 1</u>	<u>Interval 2</u>	<u>Interval 1</u>	<u>Interval 2</u>
3	1.50	.70	.87	235.73	1043.02
	2.00	.75	---	314.31	---
	2.50	.80	---	392.89	---
	3.00	.84	---	471.47	---
6	1.50	.78	.83	381.73	607.00
	2.00	.84	.93	508.98	1517.54
	2.50	.87	---	636.22	---
	3.00	.90	---	763.46	---
8	1.50	.79	.82	455.24	612.00
	2.00	.87	.93	606.99	1173.01
	2.50	.91	.96	758.73	3351.51
	3.00	.92	---	910.48	---
16	1.50	.84	.82	666.59	714.59
	2.00	.91	.92	888.79	1091.74
	2.50	.94	.97	1110.99	1679.61
	3.00	.96	.99	1333.19	2807.37
24	1.50	.85	.81	826.21	825.14
	2.00	.91	.93	1101.61	1196.46
	2.50	.94	.97	1377.01	1685.17
	3.00	.97	.99	1652.41	2392.95

Coverage Probabilities and Average Lengths are  
 calculated based on 1000 simulations

Table 5.3Simulation Study of Confidence Intervals

for  $\sum_{i=1}^N \mu_i$ :

Observed Coverage Probabilities and  
Average Lengths When  $\mu_i$ 's Are Uniform (1,200)

<u>Sample Size N</u>	<u>Constant</u>	<u>Coverage Probability</u>		<u>Average Length</u>	
		<u>Interval 1</u>	<u>Interval 2</u>	<u>Interval 1</u>	<u>Interval 2</u>
3	1.50	.69	.89	466.13	2057.75
	2.00	.75	---	621.51	---
	2.50	.80	---	776.89	---
	3.00	.84	---	932.27	---
6	1.50	.76	.84	726.48	1151.56
	2.00	.82	.94	968.65	2903.91
	2.50	.87	---	1210.81	---
	3.00	.90	---	1452.97	---
8	1.50	.77	.84	867.57	1172.71
	2.00	.86	.93	1156.77	2247.69
	2.50	.90	.95	1445.96	6422.01
	3.00	.92	---	1735.15	---
16	1.50	.83	.83	1293.03	1390.97
	2.00	.90	.92	1724.04	2125.09
	2.50	.93	.97	2155.06	3269.38
	3.00	.96	.99	2586.07	5464.54
24	1.50	.84	.83	1617.82	1609.29
	2.00	.92	.93	2157.09	2333.47
	2.50	.95	.97	2696.37	3286.58
	3.00	.97	.99	3235.64	4666.96

Coverage Probabilities and Average Lengths are  
calculated based on 1000 simulations

Examination of these tables shows that the first confidence interval outperforms the second for smaller sample sizes ( $N = 3, 6, 8$ ): for the same observed coverage probability, the first interval has shorter average length; for the same observed average length, the first interval has higher empirical coverage probability. For example, in Table 5.2 for  $N = 6$ , when the constant  $b = 1.50$  is used in the second confidence interval, this interval covers the true mean  $\Sigma \mu_i$  with probability 83% and has average length 607; when the constant  $a^2$  in the first confidence interval takes the value 2.00, this interval covers the true mean with (coverage) probability 84% and has average length 509. Comparing these performances, we see that the first confidence interval has a higher coverage probability and a lower average length than the second.

In the same table for  $N = 8$ , confidence interval 2 with empirical coverage probability 82% has average length 612 (these correspond to the value 1.50 of the constant,  $b$ ), whereas the competing confidence interval 1 with coverage probability 87% has average length 607 (these are obtained for the value 2.00 of the constant,  $a^2$ ). The same phenomenon occurs throughout Tables 5.1 to 5.3. The difference in performance between the two types of confidence intervals appears to diminish as  $N$ , the number of p.s.u.'s, becomes larger.

Neither of the intervals performs particularly well in the case  $N = 3$  (3 p.s.u.'s) in the sense that the confidence coefficients are relatively low. That is, no matter which value of the constant (which is  $b$  for interval 1 or  $a^2$  for interval 2) is chosen, the probability that the interval contains the true value of  $\Sigma \mu_i$  cannot be made satisfactorily large (90% or 95%). For example, when we consider the constant  $b = 2.00$  for  $N = 3$ , the observed probability that  $\Sigma X_i \pm 2.00\hat{S}$  contains the true  $\Sigma \mu_i$  is .80 in Table 5.1, .75 in Table 5.2, and .75 in Table 5.3. Thus  $b = 2.00$  does not give a .90 or .95 confidence coefficient. Similarly, the other values of  $b$  for interval 1 and  $a^2$  for interval 2 fall short of the 90% and 95% confidence levels when  $N = 3$ .

For larger values of  $N$ , the performance is more satisfactory. In general, the recommended interval is interval 1:

$$\sum_{i=1}^N \mu_i \in \sum_{i=1}^N X_i \pm b \left[ \frac{1}{2} \sum_{i=1}^N X_i (1 + X_i) \right]^{1/2} \quad (5.20)$$

where, for a 90 percent confidence interval, the recommended values of  $b$ , based on Tables 5.1 to 5.3, are

N	b
6	3.0
8	2.5
≥ 16	2.0

For other values of N, linear interpolation will suffice for now; more precise calculations can be done when a value of N is decided upon.

If replicate sampling were employed, it would be possible to estimate the parameter  $c^2$ , and the result would be a smaller estimate of standard error.

More precisely, the estimated variance of  $\sum_{i=1}^N X_i$  from (5.14) can be written as the sum of two pieces:

$$\hat{\text{Var}}(\Sigma X_i) = \Sigma X_i + \frac{\hat{c}^2}{1 + \hat{c}^2} (\Sigma X_i^2 - \Sigma X_i). \quad (5.21)$$

The first piece,  $\Sigma X_i$ , is the variance under the Poisson assumption; the remaining piece can be thought of as measuring the error incurred because of the departure of the data from the Poisson model. This departure may be attributable to causes discussed in Sections 4.2 and 4.5. If  $c^2 = 0$ , the second term on the right-hand side is zero, and the estimated variance becomes the expression  $(\Sigma X_i)$  that is the optimal estimator under the Poisson model of variation.

Conservative estimates of error (this means that the confidence intervals may tend to be wider than needed) are obtained by taking  $c^2$  equal to the empirical conservative bound of  $c^2 = 1$ . However, this approach can be very costly in terms of the accuracy of estimation, because a great improvement is possible from estimating  $c^2$ , using (VI.5) of Appendix VI, and substituting (5.14) rather than (5.13) into the confidence interval formula (5.15).

An example will illustrate the techniques for obtaining the estimated variance and standard error of  $\Sigma X_i$ . To focus attention on the central issues, we will deal with an artificially simple situation. Consider an entrainment season divided into eight p.s.u.'s, each of which has a sample volume  $v$  drawn from a total volume in the discharge channel of  $V$ . The number of striped bass yolk-sac larvae identified in these samples is as follows:

i	$X_i$	(5.22)
1	13,000	
2	14,000	
3	18,000	
4	43,000	
5	69,000	
6	63,000	
7	7,000	
8	3,000	

To obtain a confidence interval for  $\Sigma\mu_i$ , we may proceed in any of three different ways:

1. We may assume that the  $X_i$ 's follow a Poisson model and estimate the variance of  $\Sigma X_i$ , which is our estimator of  $\Sigma\mu_i$ , by  $\Sigma X_i$ . The estimated standard error is the square root of this variance. An approximate 90% confidence interval for  $\Sigma\mu_i$ , based on the normal approximation to the Poisson distribution (Mood, Graybill, and Boes, 1974, p. 120), is

$$\Sigma X_i \pm 1.645(\Sigma X_i)^{1/2} = 230,000 \pm 789, \quad (5.23)$$

where the value 1.645 is the 95 percent point of the standard normal distribution. This method is, unfortunately, not appropriate in many entrainment situations. As detailed in Section 4.1, the Poisson model is not consistent with the pattern of entrainment data that has emerged from studies at all three power plants in which replicate observations were taken. The Poisson model is mentioned here primarily as a reminder that although it gives a simple method and a short confidence interval, its failure to fit the data adequately means that the confidence interval will be erroneous. That is, the interval that is purported to include the true underlying (unknown) value of  $\Sigma\mu_i$  with probability 90% may in fact include this value with a much smaller probability.

2. A conservative estimate of the standard error comes from equation (5.13), in which  $c^2$  has been taken to be 1. Applying this formula to the  $N = 8$  counts given above, or equivalently applying equation (5.21) with  $\hat{c}^2 = 1$ , gives

$$\hat{S} = [230,000 + \frac{1}{2} (11,326,000,000 - 230,000)]^{1/2} = 75,254. \quad (5.24)$$

The corresponding 90% confidence interval for  $\Sigma\mu_i$  is

$$\Sigma X_i \pm 2.50 (75,254) = 230,000 \pm 188,135, \quad (5.25)$$

where the constant 2.50 comes from the value of  $b$  corresponding to  $N = 8$  in the table below (5.20). This interval is correct, but it is so large that it may not be very useful: the length of this confidence interval is approximately 160% of the mean. This happens because the example was structured to have only eight p.s.u.'s. The number of p.s.u.'s plays a crucial role in determining the length of confidence intervals that can be obtained from the entrainment data. To illustrate this, take the eight p.s.u.'s in the example each to be one week long. Assume that these eight weeks span the entire entrainment season for a particular species and life stage of ichthyoplankton. (This is realistic for many species and life stages.) We could instead adopt a p.s.u. of 24 hours, which would give us 56 one-day p.s.u.'s instead of 8 seven-day p.s.u.'s.

Analyzing a given body of entrainment data as 56 one-day p.s.u.'s would give results that are very different from an analysis of the same data structured as 8 seven-day p.s.u.'s. This is because the variance

$\hat{S}^2$  is computed from equation (5.13) or (5.14). A 7-day p.s.u. produces one large term  $X_i(1 + X_i)/2$  in (5.13) or a similar large term in (5.14);

if the 7-day period is divided into 7 one-day p.s.u.'s, though, the result is seven small terms of the same type, whose sum is less than the single large term present when the seven days formed a single p.s.u. To illustrate this point, assume that the data of (5.22) come from an entrainment season that has been divided into eight 7-day p.s.u.'s. The data from these 7-day p.s.u.'s give the confidence interval (5.25) for  $\Sigma \mu_i$ . The

original data were undoubtedly structured with shorter p.s.u.'s, however, and p.s.u.'s of one day in length would give the following data for striped bass yolk-sac larvae in 56 p.s.u.'s (where it has been assumed for convenience that each of the seven days constituting a p.s.u. in the data of (5.22) has the same observed value, e.g., each of the first seven one-day p.s.u.'s has observed value  $13,000/7 = 1,857$ ):

$i$	$X_i$ for each of these	(5.26)
1 to 7	1,857	
8 to 14	2,000	
15 to 21	2,571	
22 to 28	6,143	
29 to 35	9,857	
36 to 42	9,000	
43 to 49	1,000	
50 to 56	429	

Applying formula (5.13) to these data, the first seven days now produce a combined term of  $7 \times 1,857(1 + 1,857)/2 = 12,076,071$  instead of  $13,000 \times 13,001/2 = 84,506,500$ . Similar reductions occur in the remaining terms, and (5.13) now yields

$$\begin{aligned}\hat{S} &= [1,857(1+1,857)/2 + \dots + 429(1+429)/2]^{1/2} = (809,103,001)^{1/2} \\ &= 28,445.\end{aligned}\quad (5.27)$$

The corresponding 90% confidence interval for  $\Sigma\mu_i$  is

$$\Sigma X_i \pm 2.00 (28,445) = 229,999 \pm 56,890, \quad (5.28)$$

where  $b = 2.00$  is chosen because  $N = 56$  is greater than 24.

This demonstrates forcefully that dividing the entrainment season into many p.s.u.'s, each of short duration, leads to more accurate estimates of entrainment than using few p.s.u.'s of long duration. This fact was discussed earlier in Section 4.3. The variance, standard deviation, and coefficient of variation of entrainment estimates are all reduced by adopting short p.s.u.'s instead of moderate to long ones.

3. An accurate and appropriate estimate of the standard error comes from equation (5.14), in which  $c^2$  is estimated instead of being set equal to 1. If a series of replicate paired samples were taken during this entrainment season, yielding an estimate  $\hat{c}^2 = .05$ , then applying (5.21), which is equivalent to (5.14), to the data of (5.22) gives

$$\hat{S} = [230,000 + \frac{.05}{1.05} (11,326,000,000 - 230,000)]^{1/2} = 23,228 \quad (5.29)$$

The corresponding 90% confidence interval for  $\Sigma\mu_i$ , again using  $b = 2.50$ , is

$$\Sigma X_i \pm 2.50(23,228) = 230,000 \pm 58,070. \quad (5.30)$$

A value of  $c^2$  like .05 is quite consistent with the values of  $K$  corresponding to several of the species-life stage combinations in Table 4.8.

As Cassie has noted, values of  $c^2$  between .1 and .2 are commonly encountered (see Section 4.2.1).

Changing the value of  $c^2$  in this example would produce the following results:

<u>c<sup>2</sup></u>	<u>S</u>	<u>% of Maximum S</u>
0	480	0.6
.01	10,600	14
.02	14,910	20
.03	18,169	24
.05	23,228	31
.10	32,091	43
.20	43,449	58
.50	61,445	82
.75	69,672	93
1.00	75,254	100

This table illustrates that there is substantial improvement possible from replicate sampling, since it allows the estimation of  $c^2$ . It also shows that the Poisson assumption results in an unrealistically small variance. In many cases, using the Poisson variance will give systematic underestimation, leading to erroneous confidence statements.

To estimate the total seasonal entrainment  $T$  from (5.30), multiply through by  $V/v$ . For instance, let  $v = 8,400$  and  $V = 60,480,000$ , as might be the case for a one-week p.s.u. at Indian Point. Then  $V/v = 7,200$ , and a 90% confidence interval for  $T$  is

$$1,656,000,000 \pm 418,104,000.$$

Estimates of total entrainment of striped bass yolk-sac larvae and juveniles for Bowline Point and Indian Point in 1983 are computed in Tables 5.4 to 5.9. P.s.u.'s are 3-hour periods. The confidence interval formulas in Section 5.4.1 are generalized in Section 5.9 to the case of unequal sampling ratios  $V_i/v_i$ ; these generalizations are used in the calculations leading to Tables 5.4, 5.5, 5.7 and 5.8. For convenience in calculation and for information about the entrainment season pattern, these tables are organized by one-week segments. The CV of the total number entrained during the entire season could be decreased by using replicate sampling to estimate  $c^2$  and then obtaining the estimated standard error from formula (5.14) instead of (5.13).

Notice that in each table the coefficient of variation of the estimated total number of striped bass entrained is much lower than the CV of the one-week estimated entrainment values. To understand the reason for this, consider the simple situation in which  $N$  weekly entrainment estimates  $\hat{T}_i$  are made, each having the same underlying mean  $T_1$  and variance  $\text{var}_1$ . The seasonal entrainment estimator

$$\hat{T} = \sum_{i=1}^N \hat{T}_i \tag{5.31}$$

has variance equal to  $N\text{var}_1$  and mean equal to  $NT_1$ , so

Table 5.4

Estimate of Total Entrainment of Striped Bass  
Yolk-Sac Larvae from Bowline Point 1983 Data

<u>Week</u> <u>Beginning</u>	<u>Total Estimated</u> <u>Entrainment</u>	<u>Standard</u> <u>Error *</u>	<u>Coefficient of</u> <u>Variation **</u>
22MAY83	6,649	4,077	61.3%
29MAY83	13,139	9,704	73.9%
10JUN83	16,619	8,717	52.5%
12JUN83	102,896	27,852	27.1%
19JUN83	411,330	80,110	19.5%
26JUN83	13,815	5,308	38.4%
Entire Season	564,447	86,071.7	15.2%

Approximate 90% confidence interval for total number entrained \*\*\* =

392,304 to 736,590

\* Standard error calculated from formula (5.9)

\*\* Coefficient of variation =  $100\% \times (\text{Standard Error}) / (\text{Total Estimated Entrainment})$

\*\*\* Confidence interval based on  $\pm 2$  standard errors

Table 5.5Estimate of Total Entrainment of Striped Bass  
Juveniles from Bowline Point 1983 Data

<u>Week Beginning</u>	<u>Total Estimated Entrainment</u>	<u>Standard Error *</u>	<u>Coefficient of Variation **</u>
19JUN83	26,496	9,857	37.2%
26JUN83	60,531	16,711	27.6%
03JUL83	54,673	16,336	29.9%
10JUL83	27,275	7,992	29.3%
17JUL83	13,805	4,831	35.0%
24JUL83	1,031	1,031	100.0%
31JUL83	0	-	-
07AUG83	7,107	3,381	47.6%
Entire Season	190,918	27,257.5	14.3%

Approximate 90% confidence interval for total number entrained \*\*\* =

136,403 to 245,433

\* Standard error calculated from formula (5.9)

\*\* Coefficient of variation =  $100\% \times (\text{Standard Error}) / (\text{Total Estimated Entrainment})$

\*\*\* Confidence interval based on  $\pm 2$  standard errors

Table 5.6

Estimate of Total Entrainment of Striped Bass  
Yolk-Sac Larvae and Juveniles, Bowline Point 1983:  
Coefficient of Variation for Selected Sampling Ratios

<u>Yolk-Sac Larvae</u>			
<u>Sampling Ratio</u>	<u>Estimated Season Entrainment</u>	<u>Standard Error *</u>	<u>Coefficient of Variation **</u>
Actual	564,447	86,072	15.2%
1,000	564,447	85,302	15.1%
2,000	564,447	86,791	15.4%
4,000	564,447	89,696	15.9%
8,000	564,447	95,240	16.9%
<u>Juveniles</u>			
Actual	190,918	27,257	14.3%
1,000	190,918	26,886	14.1%
2,000	190,918	28,576	15.0%
4,000	190,918	31,686	16.6%
8,000	190,918	37,133	19.4%

\* Standard error calculated from formula (5.9)

\*\* Coefficient of variation = 100% x (Standard Error)/(Total Estimated Entrainment)

Table 5.7

Estimate of Total Entrainment of Striped Bass  
Yolk-Sac Larvae from Indian Point 1983 Data

<u>Week Beginning</u>	<u>Total Estimated Entrainment</u>	<u>Standard Error *</u>	<u>Coefficient of Variation **</u>
03MAY83	11,318	7,524	66.5%
10MAY83	38,279	14,331	37.4%
17MAY83	62,249	20,240	32.5%
24MAY83	105,991	29,100	27.5%
31MAY83	267,897	60,003	22.4%
07JUN83	500,861	105,608	21.1%
14JUN83	241,726	115,204	47.7%
21JUN83	28,261	12,452	44.1%
28JUN83	9,216	6,518	70.7%
Entire Season	1,265,798	172,456.6	13.6%

Approximate 90% confidence interval for total number entrained \*\*\* =

920,885 to 1,610,711

\* Standard error calculated from formula (5.9)

\*\* Coefficient of variation =  $100\% \times (\text{Standard Error}) / (\text{Total Estimated Entrainment})$

\*\*\* Confidence interval based on  $\pm 2$  standard errors

Table 5.8

Estimate of Total Entrainment of Striped Bass  
Juveniles from Indian Point 1983 Data

<u>Week</u> <u>Beginning</u>	<u>Total Estimated</u> <u>Entrainment</u>	<u>Standard</u> <u>Error *</u>	<u>Coefficient of</u> <u>Variation **</u>
21JUN83	4,856	4,856	100.0%
28JUN83	68,498	30,718	44.8%
05JUL83	122,755	32,539	26.5%
12JUL83	78,147	23,821	30.5%
19JUL83	57,937	19,519	33.7%
26JUL83	26,303	10,563	40.2%
Entire Season	358,497	55,551.6	15.5%

Approximate 90% confidence interval for total number entrained \*\*\* =

247,394 to 469,600

\* Standard error calculated from formula (5.9)

\*\* Coefficient of variation =  $100\% \times (\text{Standard Error}) / (\text{Total Estimated Entrainment})$

\*\*\* Confidence interval based on  $\pm 2$  standard errors

Table 5.9

Estimate of Total Entrainment of Striped Bass  
Yolk-Sac and Juveniles, Indian Point 1983:  
Coefficient of Variation for Selected Sampling Ratios

<u>Yolk-Sac Larvae</u>			
<u>Sampling Ratio</u>	<u>Estimated Season Entrainment</u>	<u>Standard Error *</u>	<u>Coefficient of Variation **</u>
Actual	1,265,798	172,457	13.6%
1,000	1,265,798	162,996	12.9%
2,000	1,265,798	164,898	13.0%
4,000	1,265,798	168,638	13.3%
8,000	1,265,798	175,878	13.9%
 <u>Juveniles</u>  			
Actual	358,497	55,552	15.5%
1,000	358,497	49,668	13.9%
2,000	358,497	51,441	14.3%
4,000	358,497	54,815	15.3%
8,000	358,497	61,006	17.0%

$$CV(\hat{T}) = (N\text{var}_1)^{1/2}/NT_1 = N^{-1/2}\text{var}_1^{1/2}/T_1 = N^{-1/2}CV(\hat{T}_1). \quad (5.32)$$

In other words, the CV of the sum of N weekly estimates with the same mean and variance will be  $N^{-1/2}$  times the CV of any weekly estimate. When the weekly estimates are somewhat different in mean and variance, a more general formulation of this result continues to hold.

For example, consider a season with  $N = 9$  p.s.u.'s, in each of which the estimated entrainment has a mean of  $E(\hat{T}_i) = T_i = 100,000$  and a variance of  $\text{Var}(\hat{T}_i) = 2,500,000,000$ , hence a standard deviation of  $SD(\hat{T}_i) = 50,000$ . In each p.s.u., the coefficient of variation of the estimate is

$$CV(\hat{T}_i) = SD(\hat{T}_i)/E(\hat{T}_i) = 50,000/100,000 = .500 = 50\% \quad (5.33)$$

For the entire season,  $E(\hat{T}) = 9(100,000) = 900,000$  and

$$\text{Var}(\hat{T}) = \text{Var}(\hat{T}_1) + \dots + \text{Var}(\hat{T}_9) = 22,500,000,000,$$

$$SD(\hat{T}) = 150,000, \quad (5.34)$$

$$CV(\hat{T}) = 150,000/900,000 = .1667 = 16.67\% = 9^{-1/2}(50\%).$$

Tables 5.6 and 5.9 compare the accuracy of the sampling actually performed with the accuracy that would have resulted from fixed sampling ratios  $V_i/v_i = 1000, 2000, 4000, \text{ and } 8000$  for every p.s.u. It is assumed that the plant flow volume and the organism density observed in the sample are, for each 3-hour p.s.u., equal to the values that actually occurred. Such regular behavior of sample organism density, remaining fixed as sampling ratio  $V_i/v_i$  varies from 1000 to 8000, should not be expected, as Chapter 6 will detail. Nevertheless, assuming this regularity allows a simple comparison of different sampling ratios.

An important assumption in the method that produced Tables 5.4 to 5.9 is that each daily estimated entrainment figure is based on the number of organisms entrained in that day's sample. Each daily  $\hat{T}_i$  is an estimate of  $T_i$  based on the sample taken on day  $i$ , so the daily estimates are independent of each other. ("Independent" is used here in its technical sense, which means that knowing the error of estimation  $\hat{T}_i - T_i$  would not give you any information whatsoever about  $\hat{T}_j - T_j$  for any  $j \neq i$ .)

This independence plays a key role in the method used in combining daily estimates into weekly and seasonal total estimates. The data from Roseton do not have this independence, because there are days when no sampling occurred: for any such day, the estimated daily entrainment is derived by linear interpolation between the closest actual observations before and after this day. The Roseton data are therefore not suitable for the approach used to construct Tables 5.4 to 5.9. A method of dealing with gaps in the entrainment sampling will be discussed in Chapter 8.

#### 5.6 CONFIDENCE INTERVAL LENGTH AS A FUNCTION OF $v$

We now consider the relationship between the sample volume and the length of the corresponding confidence interval for the total entrainment  $T$ . We restrict attention to the case in which all samples have a common volume  $v$  and the volume of water in the discharge channel equals the same value  $V$  in every sampling period.

The total number of organisms entrained during the entire entrainment season is  $T = (V/v)\Sigma\mu_i$ . It was shown in Sections 5.4 and 5.5 that a (conservative) confidence interval for  $\Sigma\mu_i$  can be constructed from the  $\hat{S}$  of (5.13). This interval has length  $2b\hat{S}$ , so the corresponding confidence interval for  $T$ ,

$$(V/v)\Sigma X_i \pm (V/v)b\hat{S}, \quad (5.35)$$

has length

$$L = 2(V/v)b[\Sigma X_i(1 + X_i)/2]^{1/2}. \quad (5.36)$$

Using the fact that  $EX_i = v_i\lambda_i = v\lambda_i$ , we see that

$$\Sigma X_i = v\Sigma\lambda_i \text{ and } \Sigma X_i^2 = v^2\Sigma\lambda_i^2. \quad (5.37)$$

Therefore

$$\begin{aligned} L &= 2(V/v)b[v\Sigma\lambda_i/2 + v^2\Sigma\lambda_i^2/2]^{1/2} \\ &= 2Vb[\Sigma\lambda_i/2v + \Sigma\lambda_i^2/2]^{1/2} \end{aligned} \quad (5.38)$$

It is clear that when all else is held fixed, the confidence interval length  $L$  decreases as  $v$  increases. The rate of this change in  $L$  depends on the term

$$G(v) = \Sigma \lambda_i / v + \Sigma \lambda_i^2. \quad (5.39)$$

When  $v$  is much smaller than  $\Sigma \lambda_i$ , a small increase in  $v$  will cause this term  $G(v)$  to decrease substantially; however, when  $v$  is much larger than  $\Sigma \lambda_i$ , the same increase in  $v$  will cause only a negligible decrease in  $G(v)$ . No matter how large  $v$  becomes,  $G(v)$  remains greater than  $\Sigma \lambda_i^2$ . By using  $\Sigma \lambda_i^2 / v^2$  to approximate  $\Sigma \lambda_i^2$  and  $\Sigma \lambda_i / v$  to approximate  $\Sigma \lambda_i$ , we can investigate the relationship between sample volume and the length  $L$  of this confidence interval for  $T$ .

If replicate sampling data are available and the constant  $c^2$  can be estimated, the confidence interval for  $T$  is still

$$(V/v) \Sigma X_i \pm (V/v) b \hat{S}, \quad (5.40)$$

but  $\hat{S}$  is now given by (5.14). Routine algebra shows that

$$L = 2Vb[\Sigma \lambda_i / v(1 + \hat{c}^2) + \hat{c}^2 \Sigma \lambda_i^2 / (1 + \hat{c}^2)]^{1/2}. \quad (5.41)$$

Again  $L$  decreases as  $v$  increases, and the relationship between sample volume and  $L$  can be examined. If  $\hat{c}^2$  is sufficiently close to zero, the term  $\hat{c}^2 \Sigma \lambda_i^2 / (1 + \hat{c}^2)$  in  $L$  will be negligible, which would make the length of the confidence interval proportional to  $v^{-1/2}$ .

### 5.7 THE EFFECT OF SAMPLE VOLUME ON THE PRECISION OF ESTIMATION

Assume that there is continuous sampling in the discharge channel, so every p.s.u. in the entrainment season is sampled. The annual entrainment  $T$  is the sum over all  $N$  p.s.u.'s of the within-period entrainment,

$$T = \sum_{i=1}^N T_i, \quad (5.42)$$

and is estimated by

$$\hat{T} = \sum_{i=1}^N \hat{T}_i, \quad (5.43)$$

where the entrainment  $T_i$  in p.s.u.  $i$  is estimated by

$$\hat{T}_i = (V_i/v_i) \hat{\mu}_i \quad \text{for } i = 1, \dots, N. \quad (5.44)$$

If the variance function among replicate counted samples is  $\sigma^2 = \mu + c^2 \mu^2$ , then the coefficient of variation of the annual entrainment estimator  $\hat{T}$  is given approximately by

$$CV(\hat{T}) = \left[ \sum T_i^2 \left( \frac{1}{\mu_i} + c^2 \right) \right]^{1/2} / \sum T_i \quad (5.45)$$

The CV of the annual entrainment estimator  $\hat{T}$  is estimated by

$$\hat{CV}(\hat{T}) = \left[ \sum \hat{T}_i^2 \left( \frac{1}{\hat{\mu}_i} + \hat{c}^2 \right) \right]^{1/2} / \sum \hat{T}_i \quad (5.46)$$

The effect of the sample volume on  $\hat{CV}(\hat{T})$  depends on the magnitude of the constant  $c^2$ . If  $c^2$  is equal to or very close to zero, so the  $c^2$  term in (5.46) can be ignored, then  $\hat{CV}(\hat{T})$  is approximately proportional to  $v^{-1/2}$  through the term  $1/\hat{\mu}_i$ . In other words, doubling every sample volume  $v_j$  would multiply  $\hat{CV}(\hat{T})$  by  $2^{-1/2} = .71$ , quadrupling every  $v_j$  would multiply  $\hat{CV}(\hat{T})$  by  $4^{-1/2} = .5$ , and so on. This situation would occur if the observed entrainment data were found to satisfy the Poisson model, in which  $c^2 = 0$  holds. On the other hand, when  $c^2$  is not zero, it will, in most cases, be larger than  $V_i/v_i \hat{T}_i = 1/X_i$  (since  $X_i$  is usually a large number), so increasing the sample volume  $v$  will have a negligible effect on the value of  $\hat{CV}(\hat{T})$ . This situation would occur if the observed entrainment data were found to satisfy the negative binomial model but not the Poisson model.

Increasing the length of the time period over which the samples are composited serves to increase the sample volumes  $v_i$  and possibly the within-period entrainment levels  $T_i$ . (This assumes that the flow rates in the sampler and the discharge channel are not changed while the p.s.u.'s are lengthened.) Ideally, however,  $v_i$  will be increased only during periods when  $\lambda_i$  is relatively small, so as to retain relatively constant entrainment  $T_i$  across p.s.u.'s. This is because for a fixed number of p.s.u.'s  $N$ , the magnitude of  $(\sum T_i^2)^{1/2} / \sum T_i$  is minimized when all of the  $T_i$  are equal. This results in a small  $CV(\hat{T})$ . Thus, during periods of low entrainment the composite duration should be extended by combining adjacent p.s.u.'s, but only if these p.s.u.'s have a constant discharge volume  $V$ .

### 5.8 THE ESTIMATION OF ENTRAINMENT REDUCTION

For a given p.s.u.  $i$ , let  $V_i$  denote the realized discharge volume and let  $V_i + V_i'$  denote the potential discharge volume under full flow conditions.

Similarly, let  $T_i$  denote the realized entrainment and  $T_i + T_i'$  the potential entrainment when the discharge volume is  $V_i + V_i'$ . The entrainment reduction due to reduced flow is  $T_i'$  and its estimator  $\hat{T}_i'$  is given by

$$\hat{T}_i' = (V_i'/V_i)\hat{T}_i. \quad (5.47)$$

Temporarily, omit the subscript  $i$  on all terms (for notational convenience) until (5.52) below. Using the fact that

$$\hat{T}' - T' = (V'/V)(\hat{T} - T) + (V'/V)T - E(T') + E(T') - T' \quad (5.48)$$

it follows that

$$\begin{aligned} E[(\hat{T}' - T')^2 | T] &= (V'/V)^2 T^2 \left( \frac{1}{\mu} + c^2 \right) + E(T' - TV'/V)^2 \\ &= (V'/V)^2 T^2 \left( \frac{1}{\mu} + c^2 \right) + [TV'/V - E(T')]^2 \\ &\quad + E[T' - E(T')]^2. \end{aligned} \quad (5.49)$$

Assuming a Poisson division of the entrainment between the volumes  $V$  and  $V'$ , we see that

$$E[(T' - E(T'))^2] = E(T'). \quad (5.50)$$

Averaging over  $T$  gives  $E(T) = V\lambda$  and  $\text{Var}(T) = V\lambda$ ,  $\bar{T}' = V'\lambda$ , and

$$\begin{aligned} E(\hat{T}' - T')^2 &= \bar{T}' \left[ 1 + V'/V + (\bar{T}' + V'/V) \left( \frac{1}{\mu} + c^2 \right) \right] \\ &= \bar{T}'^2 \left[ 1/\bar{T}' + 1/\bar{T}' + (1 + 1/\bar{T}') \left( \frac{1}{\mu} + c^2 \right) \right]. \end{aligned} \quad (5.51)$$

Therefore

$$CV(\hat{T}'_i) = \left[ 1/\bar{T}'_i + 1/\bar{T}'_i + (1 + 1/\bar{T}'_i) \left( \frac{1}{\mu_i} + c^2 \right) \right]^{1/2} \quad (5.52)$$

and, letting  $CV(\Sigma \hat{T}'_i)$  denote the coefficient of variation of the estimator

$\sum_{i=1}^N \hat{T}'_i$  of the total entrainment reduction over all  $N$  periods,

$$CV(\Sigma \hat{T}'_i) = \frac{1}{\Sigma \bar{T}'_i} \left[ \Sigma \bar{T}'_i^2 CV^2(\hat{T}'_i) \right]^{1/2} \quad (5.53)$$

An estimator of the latter is given by

$$\hat{CV}(\Sigma \hat{T}'_i) = (1/\Sigma \hat{T}'_i) \left\{ \Sigma \hat{T}'_i^2 \left[ \frac{1/\hat{\mu}_i + \hat{c}^2}{1 + \hat{c}^2} + 1/\hat{T}'_i + 1/\hat{T}'_i \right] \right\}^{1/2} \quad (5.54)$$

Note that  $T_i$  and  $T'_i$  will ordinarily be large integers, so

$$\hat{CV}(\Sigma \hat{T}'_i) = (1/\Sigma \hat{T}'_i) \left\{ \Sigma \hat{T}'_i^2 \left[ \frac{V_i/V_i \hat{T}'_i + \hat{c}^2}{1 + \hat{c}^2} \right] \right\}^{1/2} \quad (5.55)$$

The task of decreasing  $CV(\hat{\Sigma T}_i)$  can be approached from several directions. The most clear and direct approach is to control the sample counts  $X_i = (v_i/V_i)T_i$  by increasing sample volume  $v_i$  during periods when ichthyo-plankton entrainment density is low, through more extensive compositing of samples. This could also have the side effect of equalizing the  $T_i$  values, and for a fixed number of composites--that is, for a fixed number of terms  $N$  in  $\sum_{i=1}^N T_i^2$ --the magnitude of  $(\Sigma T_i^2)^{1/2}/\Sigma T_i$  is minimized when all of the  $T_i$  are equal. The latter effect could be deliberately rather than incidentally achieved by using historic seasonal entrainment patterns in conjunction with the current outage regime to make the  $N$  values  $T_i$  for  $i=1, \dots, N$  approximately equal for the more important species and life stages through appropriate compositing.

Enlargement of the sampler's intake capacity to increase the sample volume per unit time would serve to counteract any cross-sectional clumping in the spatial distribution of entrained organisms in the discharge channel, and thereby to decrease the contribution of this factor to the value of  $c^2$ . The resulting increased cost of counting and sorting larger numbers of organisms captured in a continuous sampling program could then be counteracted by invoking subsampling at this stage of the monitoring process. If random mixing can be achieved in the plankton splitter then formula (5.53) for  $CV(\hat{\Sigma T}_i)$  applies and the clumping component of  $c^2$  will have been reduced. Quantification of this reduction can only be determined experimentally.

Formulas for estimation of total potential entrainment,  $T + T'$ , can be derived by the same methods used for the reduction in entrainment,  $T'$ . Total potential entrainment is estimated by

$$\widehat{T+T'} = \hat{T} + \hat{T}' = \sum_{i=1}^N (\hat{T}_i + \hat{T}'_i) = \sum_{i=1}^N ((V_i + V'_i)/v_i) X_i \quad (5.56)$$

For each p.s.u.  $i$ , using the fact that  $E(\hat{T}_i) = T_i$  and (5.47),

$$\begin{aligned} E[(\hat{T}_i - T_i)(\hat{T}'_i - T'_i)] &= E[(\hat{T}_i - T_i)[(V'_i/V_i)(\hat{T}_i - T_i) + (V'_i/V_i)T_i - T'_i]] \\ &= (V'_i/V_i)E[(\hat{T}_i - T_i)^2] + [(V'_i/V_i)T_i - T'_i]E[\hat{T}_i - T_i] \\ &= (V'_i/V_i)\text{Var}(\hat{T}_i) + 0. \end{aligned} \quad (5.57)$$

Consequently,

$$\begin{aligned}\text{Var}(\widehat{T+T'}) &= \sum_{i=1}^N \text{Var}(\widehat{T}_i + \widehat{T}'_i) = \Sigma E[(\widehat{T}_i + \widehat{T}'_i - T_i - T'_i)^2] \\ &= \Sigma E[(\widehat{T}_i - T_i)^2] + \Sigma E[(\widehat{T}'_i - T'_i)^2] + 2\Sigma E[(\widehat{T}_i - T_i)(\widehat{T}'_i - T'_i)] \\ &= \text{Var}(\widehat{T}) + \text{Var}(\widehat{T}') + 2\Sigma(V'_i/V_i)\text{Var}(\widehat{T}_i).\end{aligned}\quad (5.58)$$

Thus

$$\widehat{\text{Var}}(\widehat{T+T'}) = \widehat{\text{Var}}(\widehat{T}) + \widehat{\text{Var}}(\widehat{T}') + 2 \sum_{i=1}^N (V'_i/V_i) \widehat{\text{Var}}(\widehat{T}_i) \quad (5.59)$$

where

$$\widehat{\text{Var}}(\widehat{T}') = \widehat{\text{Var}}(\Sigma \widehat{T}'_i) = \sum_{i=1}^N \widehat{T}'_i{}^2 \left[ \frac{V_i/v_i \widehat{T}_i + \widehat{c}^2}{1 + \widehat{c}^2} + 1/\widehat{T}'_i + 1/\widehat{T}_i \right]. \quad (5.60)$$

Then

$$\widehat{CV}(\widehat{T+T'}) = [\widehat{\text{Var}}(\widehat{T+T'})]^{1/2} / (\widehat{T+T'}). \quad (5.61)$$

The formula for  $\widehat{\text{Var}}(\widehat{T}'_i)$ , from which  $\widehat{\text{Var}}(\Sigma \widehat{T}'_i)$  and  $\widehat{CV}(\Sigma \widehat{T}'_i)$  of (5.54) follow immediately, is derived as follows. Error variance in the predicted reduction  $T'_i$  due to an outage of volume  $V'_i$  is comprised of two uncorrelated components of error. The first component is due to estimation error in the achieved entrainment estimate  $\widehat{T}_i$  for the realized discharge of volume  $V_i$ ; this error contributes to the error in  $\widehat{T}'_i$  since  $\widehat{T}'_i = (V'_i/V_i)\widehat{T}_i$ , and the magnitude of this estimation error is  $(V'_i/V_i)(\widehat{T}_i - T_i)$ . Its squared value is estimated by  $(V'_i/v_i)^2 \widehat{\text{Var}}(X_i) = (V'_i/v_i)^2 (X_i + \widehat{c}^2 X_i^2) / (1 + \widehat{c}^2)$ . The second component of error is due to the deviation of the predictor  $V'_i T_i / V_i$  from an entrainment  $T'_i$  that would have occurred in an additional discharge of volume  $V'_i$ . Here  $T'_i$  is a chance variable, and assuming that the total potential entrainment  $T_i + T'_i$  during p.s.u.  $i$  would be randomly partitioned between  $V_i$  and  $V'_i$ , the squared error of the prediction,  $[(V'_i T_i / V_i) - T'_i]^2$ , is then estimated by  $\widehat{T}'_i [1 + (V'_i/V_i)]$ . Combining the two estimated components gives

$$\widehat{\text{Var}}(\widehat{T}'_i) = \widehat{T}'_i{}^2 \left( \frac{1/X_i + \widehat{c}^2}{1 + \widehat{c}^2} + 1/\widehat{T}'_i + 1/\widehat{T}_i \right) \quad (5.61a)$$

### 5.9 THE CASE OF UNEQUAL SAMPLING FRACTIONS $v_i/V_i$

The sampling fraction in the  $i^{\text{th}}$  p.s.u. is the ratio  $v_i/V_i$ . It has been assumed until now that this ratio, or equivalently its reciprocal  $R_i = V_i/v_i$ , is constant over all sampling periods, that is,  $R_1, R_2, R_3$ , and so on have a common value. Violation of this assumption can lead to many difficulties, some more subtle than others. To illustrate the problems associated with unequal sampling fractions, we shall examine its effect on the coefficient of variation (CV) of  $\hat{T}$ , the estimator of total entrainment abundance. Before doing so, though, we shall discuss the derivation of confidence intervals in the presence of unequal sampling fractions.

Assume that we observe ichthyoplankton counts  $X_1, X_2, \dots, X_N$  collected during  $N$  p.s.u.'s. When sampling is continuous,  $N$  is the total number of p.s.u.'s in the entrainment season. Define

$\lambda_i$  = mean density of organisms per unit volume in the discharge channel during p.s.u.  $i$ ,

$v_i$  = volume of water sampled in p.s.u.  $i$ ,

$V_i$  = volume of water in discharge channel in p.s.u.  $i$ .

Then, defining  $\mu_i = v_i \lambda_i$ , we have

$$E(X_i) = \mu_i, \quad \text{Var}(X_i) = \mu_i + c^2 \mu_i^2. \quad (5.62)$$

The total entrainment abundance is given by

$$T = \sum_{i=1}^N (V_i/v_i) \mu_i = \sum_{i=1}^N R_i \mu_i, \quad (6.63)$$

which is estimated by

$$\hat{T} = \sum_{i=1}^N (V_i/v_i)X_i = \sum_{i=1}^N R_i X_i. \quad (5.64)$$

This estimate has variance

$$\text{Var}(\hat{T}) = \sum_{i=1}^N (V_i/v_i)^2 (\mu_i + c^2 \mu_i^2) = \sum_{i=1}^N R_i^2 (\mu_i + c^2 \mu_i^2). \quad (5.65)$$

To develop an estimator of this variance, note that, as was pointed out in Section 5.4.1,

$$E \left[ \frac{X_i + c^2 X_i^2}{1 + c^2} \right] = \mu_i + c^2 \mu_i^2. \quad (5.66)$$

This leads to two estimators that are analogous to the  $\hat{S}^2$  of (5.13) and (5.14). First, if we use the empirical bound (see Section 4.5.2)  $c^2 \leq 1$  and set  $c^2$  equal to 1, we obtain the conservative estimator

$$\hat{\text{Var}}(\hat{T}) = \sum R_i^2 X_i (1 + X_i) / 2. \quad (5.67)$$

Second, if we are able to estimate  $c^2$  for the particular species-life stage combination being considered, we obtain the estimator

$$\hat{\text{Var}}(\hat{T}) = \sum R_i^2 (X_i + \hat{c}^2 X_i^2) / (1 + \hat{c}^2). \quad (5.68)$$

As has been discussed in Section 5.5, although replicate sampling must be performed in order to calculate  $c^2$ , the extra effort required is worthwhile: when  $c^2$  is very small, i.e., near zero rather than near one, the consequent reduction in  $\hat{\text{Var}}(\hat{T})$  can be dramatic.

Based on studies of the equal sampling fraction case, where all  $R_i$ 's are the same, and on asymptotic approximations of a theoretical nature, it appears acceptable to use the values of the constant  $b$ , which were tabled in Section 5.5, in the case of unequal sampling fractions. An important caution is called for, though: the further from equality the  $R_i$ 's are, the

less accurate this method will be. In other words, the nominal 90% confidence interval may actually have a confidence level of 70%, 97%, or some other value far from 90%. It would take extensive simulation work to specify precisely the patterns of inequality among the  $R_i$ 's that will cause this inaccuracy. This possibility should provide an inducement to keep the  $R_i$ 's as similar as possible.

Relatively small differences among the sampling ratios  $R_i$  will cause only minor inaccuracy. An example later in this section will show that if the largest  $R_i$  is twice as great as the smallest, the effect on the accuracy of estimation, as reflected in the value of the CV of  $\hat{T}$ , is not large, while a greater discrepancy between the largest and smallest  $R_i$  can lead to substantial inaccuracy.

Note that the preceding discussion of accuracy is not concerned with absolute sample volumes, but rather with the allocation of a given sample volume. For example, suppose one has decided on a sample volume of  $3600 \text{ m}^3$  in a 24-hour period. How are those 3600 best allocated? The answer is to allocate them so that the  $R_i$ 's are kept as constant as possible. Deviation from this will cause a loss in accuracy, which becomes greater as the  $R_i$ 's become more unequal.

We now consider the effect of unequal sampling fractions on the coefficient of variation (CV) of  $\hat{T}$ , the estimator of total entrainment abundance.

The coefficient of variation is the ratio of the standard deviation to the mean, which measures relative error. The square of the CV of  $\hat{T}$  is given by

$$[CV(\hat{T})]^2 = (CV)^2 = \frac{\sum_{i=1}^N R_i^2 (\mu_i + c^2 \mu_i^2)}{\left[ \sum_{i=1}^N R_i \mu_i \right]^2} . \quad (5.69)$$

When  $V_i$  is constant over all p.s.u.'s,  $R_i$  will be constant only if  $v_i$  is, that is, only if sampling is done with equal volume. It will now be shown that having constant  $R_i$  is an optimal case, which, in the case of constant  $V_i$ , means that sampling should be done with constant  $v_i$ . It should be realized, however, that if  $V_i$  varies,  $v_i$  should also vary in such a way that  $R_i$ , or  $V_i/v_i$ , is constant.

If  $R_i$  is constant, say  $R_i = R$ , then

$$(CV)_R^2 = \frac{\sum_{i=1}^N (\mu_i + c^2 \mu_i^2)}{\left[ \sum_{i=1}^N \mu_i \right]^2}, \quad (5.70)$$

while whether  $R_i$  is or is not constant, it is true that

$$(CV)_{R_i}^2 \geq \left[ \sum_{i=1}^N \frac{\mu_i^2}{\mu_i + c^2 \mu_i^2} \right]^{-1}. \quad (5.71)$$

We cannot answer whether  $(CV)_{R_i}^2 \geq (CV)_R^2$  in general, but some approximate calculations indicate that in most cases this is so. Begin by writing

$$\frac{1}{(CV)_R^2} = \frac{(\sum \mu_i)^2}{\sum (\mu_i + c^2 \mu_i^2)} = \frac{N \sum \mu_i^2}{\sum (\mu_i + c^2 \mu_i^2)} - \frac{N \sum (\mu_i - \bar{\mu})^2}{\sum (\mu_i + c^2 \mu_i^2)}. \quad (5.72)$$

The second term on the far right-hand side will, in many cases, be negligible in comparison to the first term, so we have

$$\frac{1}{(CV)_R^2} \approx \frac{N \sum \mu_i^2}{\sum (\mu_i + c^2 \mu_i^2)} \geq \sum \left( \frac{\mu_i^2}{\mu_i + c^2 \mu_i^2} \right), \quad (5.73)$$

where the second inequality always holds (its proof is an application of Chebyshev's inequality). Relating this last quantity to our lower bound on  $(CV)_{R_i}^2$ , we have that, to our level of approximation,

$$(CV)_{R_i}^2 \geq (CV)_R^2. \quad (5.74)$$

Therefore, unequal volume ratio sampling will lead to a higher degree of relative error than equal volume sampling when  $\Sigma \mu_i^2 \gg \Sigma (\mu_i - \bar{\mu})^2$ , a circumstance that occurs frequently.

To examine the effects of unequal  $R_i$ 's, it is helpful to rewrite formula (5.69), using the relationships  $R_i \mu_i = V_i \lambda_i = T_i$  and  $T = \Sigma T_i$ , where  $T_i$  is entrainment in p.s.u.  $i$  and summation is over all  $N$  p.s.u.'s. Substitution into (5.69) gives

$$\begin{aligned} [CV(\hat{T})]^2 &= (CV)^2 = \Sigma T_i^2 (c^2 + \mu_i^{-1}) / T^2 \\ &= c^2 \Sigma T_i^2 / T^2 + \Sigma T_i R_i / T^2. \end{aligned} \quad (5.75)$$

The first expression on the right-hand side depends only on the constant  $c^2$  and the daily entrainments  $T_i$ , and not on the sampling ratios  $R_i$ . Consider an entrainment season consisting of 320 3-hour p.s.u.'s (that is, 40 days), in which the actual entrainment is 10,000 organisms in each of 32 p.s.u.'s, 5,000 organisms in each of 48 p.s.u.'s, and zero in the remaining 240 p.s.u.'s. (These simple numbers have been chosen to simplify the computations, but are not entirely unrealistic.) Assume that  $c^2 = .2$  and that  $V_i$  is constant for all p.s.u.'s. We now compare three sets of  $R_i$ 's:

Number of p.s.u.'s	Entrainment/p.s.u.	First $R_i$ 's	Second $R_i$ 's	Third $R_i$ 's
32	10,000	2,500	4,000	10,000
48	5,000	2,500	2,000	1,666.7
240	0	-	-	-

The sampling ratios  $R_i$  in p.s.u.'s with  $T_i = 0$  do not affect the computation, so they are omitted from the table. The three sets of  $R_i$ 's here distribute the same total sample volume differently among the p.s.u.'s. (If  $V_i$  is 600,000 cubic meters per 3-hour p.s.u., the volume sampled using the first  $R_i$ 's totals

$$(32)(600,000/2,500) + (48)(600,000/2,500) = 19,200 \text{ cubic meters};$$

the second and third  $R_i$ 's yield the same total.) Computing the CV for each case:

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$$T = 32(10,000) + 48(5,000) = 560,000$$

$$\Sigma T_i^2 = 32(10,000^2) + 48(5,000^2) = 4,400,000,000$$

$$c^2 \Sigma T_i^2 / T^2 = .2(4,400,000,000) / 560,000^2 = .00281$$

$$\text{First } R_i \text{'s: } \Sigma T_i R_i / T^2 = [32(10,000)(2,500) + 48(5,000)(2,500)] / 560,000^2$$

$$= 1,400,000,000 / 560,000^2 = .00446$$

$$CV = (.00281 + .00446)^{1/2} = .0853$$

$$\text{Second } R_i \text{'s: } \Sigma T_i R_i / T^2 = [32(10,000)(4,000) + 48(5,000)(2,000)] / 560,000^2$$

$$= .00561$$

$$CV = (.00281 + .00561)^{1/2} = .0918$$

$$\text{Third } R_i \text{'s: } \Sigma T_i R_i / T^2 = [32(10,000)(10,000) + 48(5,000)(1,666.7)] / 560,000^2$$

$$= .01148$$

$$CV = (.00281 + .01148)^{1/2} = .1195$$

The difference between the first case, in which all  $R_i$ 's are equal, and the second case, in which the largest  $R_i$  is twice the smallest, is not great; the CV of  $\hat{T}$  has increased only a small amount because of the unequal  $R_i$ 's, so the variability of  $\hat{T}$  is almost the same as when all  $R_i$ 's are equal. Using the constant  $b$  with these unequal  $R_i$ 's is therefore quite accurate. In the third case, though, in which the largest  $R_i$  is six times the smallest, the CV has increased by 40% over its value when all  $R_i$ 's are equal, so using the constant  $b$  here does not give a correct picture of the variability of  $\hat{T}$ .

The sampling in this example was constructed to give an idea how much the CV can be affected by unequal  $R_i$ 's: sampling is performed at a lower rate (because  $R_i$  is higher) during high entrainment conditions than during low entrainment conditions, which gives less precise information about high entrainment periods, producing ultimately a less accurate estimate of  $T$ . When the p.s.u.'s with high  $R_i$  are spread more evenly among high and low entrainment conditions, the estimate  $\hat{T}$  will be less affected than in the extreme case analyzed here. Thus the analysis of situations in which the largest  $R_i$  is twice the smallest should be quite accurate.

In principle, one might improve on the accuracy of estimation resulting from equal  $R_i$ 's by doing the opposite of what was done in this example: sampling at a higher rate during high entrainment conditions than during low entrainment conditions. In practice, this should not be attempted for several reasons. First, a gain in accuracy will be made only if it is correctly anticipated which p.s.u.'s will have high entrainment and which will have low entrainment; if these forecasts are incorrect, a loss in accuracy will result. Second, any potential gain will be negligible unless the departures from equality of the  $R_i$ 's are extreme, leading to a great loss in accuracy if preseason judgments about the occurrence of high and low entrainment p.s.u.'s are incorrect. Third, while the process of predicting high and low entrainment periods might be feasible for a single taxon and life stage, it is intractably complex to attempt to choose unequal  $R_i$ 's that will simultaneously be effective for many taxa and life stages of ichthyoplankton. In short, any attempt to improve on equal  $R_i$ 's carries a high risk of producing inaccurate results for one or more taxa and life stages of interest. We conclude, as stated earlier in this section, that the  $R_i$ 's should be kept as similar as possible.

Throughout this discussion it has been assumed that  $V_i$ ,  $v_i$ , and  $v_i/V_i$  are known. If they are not known and must be estimated, there is a random amount of difference between our data value for  $V_i$  and the underlying, unknown true value of  $V_i$ , and similarly for  $v_i$  and  $v_i/V_i$ . The presence of these random errors would have serious mathematical consequences for the entire analysis. We therefore reiterate the critical assumption that  $V_i$  and  $v_i$  have been fixed, known constants wherever they have appeared.

## 6. A SIMULATION STUDY OF ENTRAINMENT

### 6.0 INTRODUCTION

The behavior of entrainment, the estimators of entrainment, and their variances will be examined in this chapter. Entrainment sampling is first examined over the course of a single day. This is done for a wide range of organism densities, sample volumes, and values of  $c^2$ , the parameter in the quadratic variance model  $\sigma^2 = \mu + c^2\mu^2$  discussed earlier in this report (e.g., Section 4.2). Both uniform variation throughout the sampling day and diel variation are considered. Estimators based on individual sampling days are discussed in detail. The effects of sample volume, organism density, temporal pattern (uniform or diel), and the quadratic variance parameter  $c^2$  on accuracy of entrainment estimation are treated. An important conclusion is that sample volumes of 150 cubic meters per 3-hour period, or 1200 cubic meters per 24-hour day, are sufficient for purposes of entrainment estimation; larger samples produce only slight gains in accuracy of estimation.

The estimation of entrainment over an entire sampling season is then treated. A season consists of a series of sampling days on which organisms are present with density that differs from one day to another. Both a short entrainment season of 18 days and a long season of 34 days are simulated. The estimation techniques developed in earlier chapters of this report are shown to produce accurate estimates of entrainment.

### 6.1 STRUCTURE OF THE SIMULATION STUDY OF DAILY ENTRAINMENT

The simulation of entrainment sampling presented here is structured in terms of a 24-hour day divided into eight 3-hour periods. The behavior of entrainment sampling over the course of a day is examined over a wide range of  $\lambda$ , the density of organisms in the discharge channel;  $c^2$ , the quadratic variance model parameter; and  $v$ , the volume of a sample drawn from the discharge flow over a three-hour period. All combinations of the following values have been examined:

$$c^2 = 0, .05, .10, .33, .50, \text{ and } 1.00$$

$$\lambda = 10, 50, 100, \text{ and } 250 \text{ organisms/1000 cubic meters} \quad (6.1)$$

$$v = 100, 150, 200, \text{ and } 300 \text{ cubic meters/3-hour period}$$

The values of  $c^2$  represent various degrees of departure from the Poisson model, all of which are within the negative binomial family of distributions. When  $c^2 = 0$ , there is no departure, because the negative binomial distribution coincides with the Poisson in this case. As  $c^2$  increases, the corresponding negative binomial distribution departs from the Poisson to a greater and greater degree. In particular, as was discussed in Section 5.5, the confidence intervals for entrainment estimates must be made wider as  $c^2$  becomes larger.

The values of  $\lambda$  represent a variety of possible organism densities within the discharge flow. A density of 10 organisms per 1000 cubic meters is rather sparse. A density of 250 organisms per 1000 cubic meters, on the other hand, is relatively high. The density  $\lambda$  is for the entire discharge flow, not merely for the sample that is drawn. The density during a 3-hour period equals the number of organisms present in the entire discharge flow during the period divided by the number of 1000-cubic-meter volumes of water in the discharge flow during the period. The entrainment sampling process aims to estimate the organism density in the entire discharge flow during a given period from a small sample of the discharge flow. (Estimating discharge flow density and total organism count during a given period are equivalent problems because the discharge flow volume is assumed known.) This simulation study addresses the question of how well the number of organisms entrained during the sampling day is estimated when samples are of the volumes  $v$ , ranging from 100 to 300 cubic meters per 3-hour period, listed above. Entrainment sampling procedures in which one sample per period is drawn are examined, as are procedures involving replicate sampling of two samples per period.

A channel flow volume of 100,000 cubic meters per 3-hour period has been adopted for numerical convenience. There is no loss of generality from this choice; conversion to different discharge flow volumes will be described in Section 6.3, after the details of the simulation have been treated.

At each of the 96 ( $6 \times 4 \times 4$ ) possible combinations of  $c^2$ ,  $\lambda$ , and  $v$ , 100 simulated days were run. To simulate one day with a specified combination of  $c^2$ ,  $\lambda$ , and  $v$ , the following steps were taken.

1. Random values of the density, based on the fixed value  $\lambda$ , were generated independently for the day's eight 3-hour periods. These random densities, which will be denoted by  $L$ , reflect the fact that the density of organisms is not fixed throughout the day; density varies from one 3-hour period to another during the day. Two patterns of density variation were considered, uniform variation and diel variation:

- a. Uniform variation. The density  $\lambda$  ( $=10, 50, 100, \text{ or } 250$ ) was perturbed by adding uniformly distributed random noise. This resulted in independent values of  $L$  having a  $\text{Uniform}(.5\lambda, 1.5\lambda)$  distribution. That is, starting with the given density  $\lambda$ , random values were generated from the interval between  $.5\lambda$  and  $1.5\lambda$ ; each random value was independent of all others and was drawn from a uniform distribution on this interval. This pattern of variation can be interpreted as consisting of 3-hour organism densities that vary from  $-50\%$  to  $+50\%$  of the density  $\lambda$ , throughout the entire day. Thus the average organism density that is realized in the simulated day's set of eight 3-hour periods is approximately  $\lambda$ .
- b. Diel variation. In this pattern of variation, three of the eight 3-hour periods have high densities relative to the other five periods. For comparability with the uniform variation case, we want the average organism density over the eight 3-hour periods to be approximately  $\lambda$ , where  $\lambda$  is 10, 50, 100, or 250 as before. To accomplish this, the random densities  $L$  during the three high-density periods are equal to  $2.25\lambda$  plus uniformly distributed random noise introducing variation from  $-50\%$  to  $+50\%$  of the density  $2.25\lambda$ . During the five low-density periods, the random densities  $L$  are equal to  $.25\lambda$  plus uniformly distributed random noise introducing variation from  $-100\%$  to  $+100\%$  of the density  $.25\lambda$ . In other words, the densities  $L$  during the five low-density periods are independent draws from a  $\text{Uniform}(0, .5\lambda)$  distribution. The average organism density obtained in the simulated day's set of eight 3-hour periods is approximately

$$[3(2.25\lambda) + 5(.25\lambda)]/8, \quad (6.2)$$

which reduces to  $\lambda$ . This makes the simulations of uniform variation and diel variation for a common  $\lambda$  comparable, because they have the same underlying daily mean density.

These two patterns of density variation were chosen to reflect important biological patterns, although the choices inevitably were somewhat arbitrary. The uniform pattern of variation represents the situation in which no strong diel effect is present, so the density fluctuates about a constant level throughout the day. Fluctuations varying from 50% below to 50% above the baseline density were judged to be a wide enough range to avoid the pattern of a density that remains essentially constant throughout the day, and yet narrow enough to be consistent with the interpretation that density varies about a fixed baseline level. The diel pattern of variation represents the situation in which three of the day's eight periods have densities that are quite high compared to the remaining five periods. This would occur if organism densities were systematically higher during nine hours (not necessarily nine consecutive hours, but any three 3-hour periods during

the day) than during the rest of the day. Random variation in the diel pattern was motivated by the same considerations as in the uniform pattern.

To simplify the exposition, the case of uniform variation will be described. The case of diel variation can be similarly described with only minor modifications.

2. Two random sample counts were generated from each of the simulated day's eight 3-hour periods. This simulated replicate sampling at two locations in the discharge channel. The first step in this process was to calculate  $\mu$ , the expected value of an observed sample count, for a specified period, for example, the first period. This is found as the product of the sample volume and the mean organism density  $L$ , randomly generated in step (1)(a), for this period:

$$\begin{aligned}\mu &= (\text{Sample volume}) * (\text{Mean density}) & (6.3) \\ &= v * L\end{aligned}$$

The sample volume and mean density must be expressed in compatible units: if  $L$  is in organisms per 100 cubic meters, then  $v$  must be expressed as a fraction (or multiple) of 1000 cubic meters, so a sample of 300 cubic meters in a 3-hour period must be written as  $v = .3$ . Thus  $\mu$  is the mean sample count under these sampling conditions.

Two independent random sample counts,  $X$  and  $Y$ , were then simulated using  $\mu$ . For  $c^2 = 0$ , these counts were simulated as Poisson( $\mu$ ) random variables with mean, or intensity parameter,  $\mu$ . For any  $c^2$  greater than 0, the two counts were simulated as negative binomial ( $K, p$ ) random variables with parameter values  $K$  and  $p$  determined by  $c^2$  and  $\mu$ :

$$K = 1/c^2 \quad (6.4)$$

$$p = 1/(c^2 \mu + 1).$$

The relationships in (6.4) and the method of simulation are both detailed in Appendix II. The parameter  $p$  can be viewed in any of three different ways. First, it can be thought of as a function, given by (6.4), of the constant  $c^2$  discussed by Cassie in Section 4.2 and the mean count  $\mu$  of a sample of specified volume taken in a specified p.s.u. This view of  $p$  is essentially as an empirical constant, which changes with the p.s.u. mean count  $\mu$ . Second,  $p$  can be thought of as equalling  $\beta/(\beta+v)$  of (4.15), where the negative binomial distribution was derived as a Gamma ( $K, \beta$ ) mixture of Poisson variables, so  $p$  depends on the parameters of the

## 6.5

Gamma mixing distribution and on the sample volume  $v$ . Third,  $p$  can be thought of as given by (6.4), the mean count  $\mu$ , and the classification model constant  $c^2$  given by (4.33) in Section 4.5. Of these three views, the first is the least restrictive, and therefore the most satisfactory. In all three interpretations,  $p$  changes from one p.s.u. to another.

(It was pointed out in Section 4.2 that the negative binomial family includes the Poisson family. Thus, although the parametrization used here for computational purposes makes it appear that the two families are entirely different, this is not the case.)

This process was carried out for each period in a simulated day. Two independent sample counts,  $X$  and  $Y$ , were generated for each of the eight 3-hour periods, based on that period's randomly generated mean density  $L$  and the sample volume  $v$ .

3. These simulated sample counts were used to compute one-sample and two-sample statistics for the sampling day. One-sample statistics were based on equations (5.13) and (5.15), using only the simulated  $X$  counts, and assuming that  $c^2 = 1$ . This approach ignores the  $Y$  counts, which are replicates of the  $X$  counts in the eight periods, proceeding as if only the  $X$  counts, one per period, were available. Two-sample statistics were calculated from equation (5.21), as it appears and with  $c$  replacing  $\hat{c}$ , using both the simulated  $X$  and  $Y$  counts. This was done in two ways: using the known value of  $c^2$ , as would be done if  $c^2$  were known from historical data; and using an estimated value  $\hat{c}^2$  based on replicate observations  $X$  and  $Y$ .

The one-sample statistics computed for each simulated day are estimates of:

Daily total (the true daily total entrainment)  $T$

Variance of  $\hat{T}$

Coefficient of Variation of  $\hat{T}$

All of these are computed, as discussed in Section 6.2, from data consisting of one sample per period. The estimated variance and coefficient of variation are based on the assumption that  $c^2 = 1$ .

The two-sample statistics computed for each simulated day are estimates of:

Daily Total  $T$

Variance of  $\hat{T}$  ( $c^2$  known)

Coefficient of Variation of  $\hat{T}$  ( $c^2$  known)

Variance of  $\hat{T}$  ( $c^2$  estimated)

Coefficient of Variation of  $\hat{T}$  ( $c^2$  estimated)

All of these are computed, as discussed in Section 6.2, from data consisting of two samples per period.

4. The true realized value  $T$  of the sampling day's total organism count was computed from the formula

$$T = \sum_1^8 VL_i = (V/v) \sum_1^8 \mu_i \quad (6.5)$$

where  $V$  is the volume of discharge flow during a 3-hour period, which is 100,000 cubic meters in this simulation. The true values of the variance and the coefficient of variation of the one-sample estimator of  $T$  were obtained from formulas to be treated in Section 6.2.

Steps (1) to (4) were executed to simulate a single day. For each combination of  $c^2$ ,  $\lambda$ ,  $v$ , and temporal pattern (uniform or diel variation), 100 of these simulated days were generated. The true values discussed in (4) and the one-sample and two-sample statistics described in (3) were averaged over the 100 simulated days; these averages appear in Tables 6.1 to 6.8. Standard deviations rather than variances are given in these tables, each being computed as the square root of the average variance over the 100 simulated days.

## 6.2 FORMULAS FOR THE SIMULATION STUDY OF DAILY ENTRAINMENT

A summary of the entrainment process described in Section 6.1 begins with the parameters defined there for an eight-period sampling day:

[text continues on page 6.55]

Table 6.1

Simulation of Sampling for One Day with Uniform  
Variation of Density About Level of 10 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	8010.23	8010.23	8010.23	8010.23
TRUE STD DEV	2830.24	2310.88	2001.28	1634.04
TRUE COEFF. OF VARIATION	35.46	28.96	25.08	20.47
DAILY TOTAL (one sample)	7980.00	8120.00	7975.00	8106.67
STANDARD DEV. (one sample)	3485.69	3126.94	2891.80	2649.74
COEFF. OF VAR. (one sample)	44.63	38.53	36.11	32.46
DAILY TOTAL (two sample)	7655.00	8040.00	7870.00	8111.67
STANDARD DEV. (csq known)	2766.77	2315.17	1983.68	1644.35
STANDARD DEV. (csq estimated)	3000.83	2472.07	2196.59	1795.52
COEFF. OF VAR. (csq known)	37.30	29.40	25.60	20.50
COEFF. OF VAR. (csq estimated)	40.27	31.18	27.97	22.15
ESTIMATE OF C-SQUARED	.41	.14	.18	.07

The value given is the average over 100 simulations.

Table 6.1 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	8010.23	8010.23	8010.23	8010.23
TRUE STD DEV	2905.78	2402.80	2106.75	1761.64
TRUE COEFF. OF VARIATION	36.40	30.09	26.38	22.05
DAILY TOTAL (one sample)	7990.00	7826.67	8045.00	8066.67
STANDARD DEV. (one sample)	3519.94	3071.73	2896.55	2697.32
COEFF. OF VAR. (one sample)	45.09	39.46	35.99	33.20
DAILY TOTAL (two sample)	8330.00	7860.00	7792.50	7990.00
STANDARD DEV. (csq known)	2967.14	2377.95	2073.49	1756.86
STANDARD DEV. (csq estimated)	3228.00	2514.84	2175.14	1807.08
COEFF. OF VAR. (csq known)	36.52	30.76	26.93	22.16
COEFF. OF VAR. (csq estimated)	39.34	32.23	28.07	22.61
ESTIMATE OF C-SQUARED	.41	.19	.15	.08

The value given is the average over 100 simulations.

Table 6.1 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	8010.23	8010.23	8010.23	8010.23
TRUE STD DEV	2979.40	2491.34	2207.20	1880.60
TRUE COEFF. OF VARIATION	37.31	31.19	27.63	23.53
DAILY TOTAL (one sample)	8220.00	8273.33	7705.00	7903.33
STANDARD DEV. (one sample)	3641.43	3247.56	2891.37	2699.18
COEFF. OF VAR. (one sample)	44.81	39.24	37.14	33.76
DAILY TOTAL (two sample)	7930.00	7940.00	7772.50	7823.33
STANDARD DEV. (csq known)	2960.13	2483.60	2171.61	1853.45
STANDARD DEV. (csq estimated)	3193.74	2624.67	2240.26	1962.42
COEFF. OF VAR. (csq known)	38.35	31.80	28.36	23.83
COEFF. OF VAR. (csq estimated)	40.73	33.06	28.84	24.67
ESTIMATE OF C-SQUARED	.49	.30	.16	.17

The value given is the average over 100 simulations.

Table 6.1 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	8010.23	8010.23	8010.23	8010.23
TRUE STD DEV	3301.34	2868.59	2625.59	2357.69
TRUE COEFF. OF VARIATION	41.30	35.86	32.81	29.44
DAILY TOTAL (one sample)	7440.00	8393.33	8145.00	8250.00
STANDARD DEV. (one sample)	3502.86	3447.38	3154.36	3006.10
COEFF. OF VAR. (one sample)	48.10	40.96	37.99	35.63
DAILY TOTAL (two sample)	7640.00	8093.33	7867.50	8055.00
STANDARD DEV. (csq known)	3180.52	2889.82	2599.89	2361.54
STANDARD DEV. (csq estimated)	3340.66	2935.61	2667.63	2471.50
COEFF. OF VAR. (csq known)	42.36	36.20	33.08	29.25
COEFF. OF VAR. (csq estimated)	43.69	35.99	33.07	30.09
ESTIMATE OF C-SQUARED	.79	.58	.61	.59

The value given is the average over 100 simulations.

Table 6.1 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	8010.23	8010.23	8010.23	8010.23
TRUE STD DEV	3513.29	3110.17	2887.58	2646.33
TRUE COEFF. OF VARIATION	43.92	38.86	36.06	33.02
DAILY TOTAL (one sample)	7690.00	8120.00	7625.00	8273.33
STANDARD DEV. (one sample)	3620.77	3446.09	3051.23	3082.39
COEFF. OF VAR. (one sample)	47.80	41.93	39.26	36.05
DAILY TOTAL (two sample)	8090.00	8063.33	7817.50	8098.33
STANDARD DEV. (csq known)	3520.80	3104.91	2813.58	2663.56
STANDARD DEV. (csq estimated)	3522.78	3192.00	2859.41	2638.29
COEFF. OF VAR. (csq known)	44.28	38.48	35.86	32.49
COEFF. OF VAR. (csq estimated)	43.42	38.77	35.22	30.89
ESTIMATE OF C-SQUARED	.85	1.10	.73	.59

The value given is the average over 100 simulations.

Table 6.1 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	8010.23	8010.23	8010.23	8010.23
TRUE STD DEV	4083.65	3742.47	3559.65	3366.90
TRUE COEFF. OF VARIATION	51.00	46.70	44.40	41.97
DAILY TOTAL (one sample)	7630.00	7766.67	8220.00	7900.00
STANDARD DEV. (one sample)	3836.67	3575.22	3616.63	3336.17
COEFF. OF VAR. (one sample)	51.21	44.74	41.92	39.86
DAILY TOTAL (two sample)	7910.00	7803.33	8122.50	8093.33
STANDARD DEV. (csq known)	4029.47	3628.79	3568.91	3428.80
STANDARD DEV. (csq estimated)	3940.81	3603.39	3635.76	3421.01
COEFF. OF VAR. (csq known)	50.37	45.17	42.90	40.56
COEFF. OF VAR. (csq estimated)	47.38	44.40	41.31	39.92
ESTIMATE OF C-SQUARED	1.27	1.89	2.67	1.74

The value given is the average over 100 simulations.

Table 6.2  
Simulation of Sampling for One Day with Uniform  
Variation of Density About Level of 50 Organisms/1000 m<sup>3</sup>

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39744.21	39744.21	39744.21	39744.21
TRUE STD DEV	6304.30	5147.44	4457.81	3639.79
TRUE COEFF. OF VARIATION	15.93	13.00	11.26	9.20
DAILY TOTAL (one sample)	40450.00	40093.33	39030.00	39463.33
STANDARD DEV. (one sample)	12319.90	11674.28	11095.04	10960.64
COEFF. OF VAR. (one sample)	30.18	28.84	28.19	27.62
DAILY TOTAL (two sample)	40000.00	40016.67	39227.50	39420.00
STANDARD DEV. (csq known)	6324.56	5165.05	4428.74	3624.91
STANDARD DEV. (csq estimated)	6989.28	5717.23	4955.68	3958.54
COEFF. OF VAR. (csq known)	15.93	12.99	11.36	9.25
COEFF. OF VAR. (csq estimated)	17.46	14.24	12.57	10.05
ESTIMATE OF C-SQUARED	.05	.03	.03	.01

The value given is the average over 100 simulations.

Table 6.2 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39744.21	39744.21	39744.21	39744.21
TRUE STD DEV	7105.65	6102.64	5533.37	4898.38
TRUE COEFF. OF VARIATION	17.93	15.39	13.94	12.33
DAILY TOTAL (one sample)	39790.00	40766.67	39515.00	39393.33
STANDARD DEV. (one sample)	12360.42	11938.73	11546.97	11067.82
COEFF. OF VAR. (one sample)	30.64	29.02	28.81	27.94
DAILY TOTAL (two sample)	40210.00	40333.33	39557.50	39850.00
STANDARD DEV. (csq known)	7154.53	6146.11	5520.23	4906.72
STANDARD DEV. (csq estimated)	7427.65	6258.86	5916.40	4824.82
COEFF. OF VAR. (csq known)	17.89	15.29	13.99	12.32
COEFF. OF VAR. (csq estimated)	18.30	15.38	14.70	11.89
ESTIMATE OF C-SQUARED	.07	.06	.08	.05

The value given is the average over 100 simulations.

Table 6.2 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39744.21	39744.21	39744.21	39744.21
TRUE STD DEV	7825.36	6927.35	6431.50	5894.08
TRUE COEFF. OF VARIATION	19.72	17.44	16.19	14.82
DAILY TOTAL (one sample)	39430.00	40133.33	40275.00	40386.67
STANDARD DEV. (one sample)	12472.37	12090.77	11862.12	11619.09
COEFF. OF VAR. (one sample)	31.18	29.81	29.00	28.46
DAILY TOTAL (two sample)	39500.00	39900.00	39665.00	40106.67
STANDARD DEV. (csq known)	7804.36	6950.27	6399.43	5938.06
STANDARD DEV. (csq estimated)	7742.09	6798.37	6394.14	5834.10
COEFF. OF VAR. (csq known)	19.81	17.43	16.12	14.77
COEFF. OF VAR. (csq estimated)	19.28	16.78	15.53	14.06
ESTIMATE OF C-SQUARED	.10	.10	.10	.10

The value given is the average over 100 simulations.

Table 6.2 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39744.21	39744.21	39744.21	39744.21
TRUE STD DEV	10553.85	9906.34	9566.17	9213.45
TRUE COEFF. OF VARIATION	26.53	24.89	24.02	23.12
DAILY TOTAL (one sample)	39950.00	39700.00	40615.00	39490.00
STANDARD DEV. (one sample)	13375.35	13030.56	12965.15	12373.58
COEFF. OF VAR. (one sample)	32.98	31.89	31.10	30.47
DAILY TOTAL (two sample)	40245.00	39763.33	40437.50	39531.67
STANDARD DEV. (csq known)	10613.41	9907.85	9688.50	9131.26
STANDARD DEV. (csq estimated)	10593.16	10188.12	9500.07	9028.38
COEFF. OF VAR. (csq known)	26.22	24.64	23.70	22.85
COEFF. OF VAR. (csq estimated)	25.29	24.29	22.66	21.94
ESTIMATE OF C-SQUARED	.39	.39	.34	.36

The value given is the average over 100 simulations.

Table 6.2 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39744.21	39744.21	39744.21	39744.21
TRUE STD DEV	12132.79	11573.96	11284.17	10986.74
TRUE COEFF. OF VARIATION	30.48	29.06	28.32	27.56
DAILY TOTAL (one sample)	39480.00	39446.67	40785.00	38196.67
STANDARD DEV. (one sample)	14011.07	13625.79	13979.36	12335.14
COEFF. OF VAR. (one sample)	34.36	33.16	32.68	31.09
DAILY TOTAL (two sample)	40565.00	40146.67	40952.50	37991.67
STANDARD DEV. (csq known)	12215.77	11642.07	11613.54	10461.04
STANDARD DEV. (csq estimated)	11938.80	11722.53	11894.17	10092.38
COEFF. OF VAR. (csq known)	29.88	28.44	27.78	26.94
COEFF. OF VAR. (csq estimated)	28.83	27.25	27.28	24.98
ESTIMATE OF C-SQUARED	.58	.58	.65	.49

The value given is the average over 100 simulations.

Table 6.2 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39744.21	39744.21	39744.21	39744.21
TRUE STD DEV	15958.22	15537.59	15322.95	15105.26
TRUE COEFF. OF VARIATION	40.05	38.98	38.43	37.88
DAILY TOTAL (one sample)	41780.00	37006.67	42145.00	40316.67
STANDARD DEV. (one sample)	16257.00	14609.59	16277.67	15402.78
COEFF. OF VAR. (one sample)	37.12	36.41	36.00	35.24
DAILY TOTAL (two sample)	41200.00	37653.33	40957.50	40245.00
STANDARD DEV. (csq known)	16419.60	14661.77	15646.53	15264.72
STANDARD DEV. (csq estimated)	16443.24	14705.55	15700.76	15341.83
COEFF. OF VAR. (csq known)	38.47	37.45	36.82	36.43
COEFF. OF VAR. (csq estimated)	37.35	36.96	35.80	34.92
ESTIMATE OF C-SQUARED	1.37	1.46	1.52	1.26

The value given is the average over 100 simulations.

Table 6.3  
Simulation of Sampling for One Day with Uniform  
 Variation of Density About Level of 100 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	79709.19	79709.19	79709.19	79709.19
TRUE STD DEV	8928.00	7289.68	6313.05	5154.58
TRUE COEFF. OF VARIATION	11.24	9.18	7.95	6.49
DAILY TOTAL (one sample)	79740.00	81120.00	79720.00	79520.00
STANDARD DEV. (one sample)	22524.65	22397.02	21763.44	21351.79
COEFF. OF VAR. (one sample)	28.05	27.45	27.18	26.69
DAILY TOTAL (two sample)	79550.00	80390.00	79940.00	79618.33
STANDARD DEV. (csq known)	8919.08	7320.75	6322.18	5151.64
STANDARD DEV. (csq estimated)	9740.12	8034.51	6819.82	5657.98
COEFF. OF VAR. (csq known)	11.28	9.14	7.94	6.50
COEFF. OF VAR. (csq estimated)	12.23	9.91	8.48	7.08
ESTIMATE OF C-SQUARED	.02	.01	.01	.01

The value given is the average over 100 simulations.

Table 6.3 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	79709.19	79709.19	79709.19	79709.19
TRUE STD DEV	11089.17	9818.35	9116.75	8356.45
TRUE COEFF. OF VARIATION	13.93	12.33	11.44	10.48
DAILY TOTAL (one sample)	79450.00	80286.67	78530.00	78620.00
STANDARD DEV. (one sample)	23089.17	22740.47	22004.09	21545.25
COEFF. OF VAR. (one sample)	28.85	28.07	27.73	27.17
DAILY TOTAL (two sample)	79325.00	80560.00	78980.00	79245.00
STANDARD DEV. (csq known)	11077.45	9912.35	9051.37	8300.28
STANDARD DEV. (csq estimated)	11032.91	10002.22	9494.47	8173.30
COEFF. OF VAR. (csq known)	13.99	12.31	11.45	10.46
COEFF. OF VAR. (csq estimated)	13.76	12.28	11.54	9.97
ESTIMATE OF C-SQUARED	.06	.06	.06	.05

The value given is the average over 100 simulations.

Table 6.3 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	79709.19	79709.19	79709.19	79709.19
TRUE STD DEV	12893.03	11817.81	11241.70	10634.42
TRUE COEFF. OF VARIATION	16.18	14.82	14.09	13.32
DAILY TOTAL (one sample)	78410.00	80080.00	81670.00	78806.67
STANDARD DEV. (one sample)	23181.03	23245.36	23208.19	22064.68
COEFF. OF VAR. (one sample)	29.14	28.57	28.05	27.62
DAILY TOTAL (two sample)	79220.00	79923.33	81330.00	78780.00
STANDARD DEV. (csq known)	12896.34	11819.56	11396.96	10502.29
STANDARD DEV. (csq estimated)	12436.64	12059.94	11965.58	10685.24
COEFF. OF VAR. (csq known)	16.27	14.75	13.97	13.28
COEFF. OF VAR. (csq estimated)	15.36	14.45	14.09	13.10
ESTIMATE OF C-SQUARED	.09	.11	.12	.11

The value given is the average over 100 simulations.

Table 6.3 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	79709.19	79709.19	79709.19	79709.19
TRUE STD DEV	19186.20	18480.81	18117.82	17747.41
TRUE COEFF. OF VARIATION	24.03	23.14	22.68	22.21
DAILY TOTAL (one sample)	76240.00	77546.67	77905.00	77246.67
STANDARD DEV. (one sample)	24137.73	24034.60	24119.65	23730.95
COEFF. OF VAR. (one sample)	30.74	30.09	29.99	30.13
DAILY TOTAL (two sample)	78175.00	79376.67	79312.50	79413.33
STANDARD DEV. (csq known)	18580.48	18393.37	18012.44	17731.91
STANDARD DEV. (csq estimated)	18244.59	17839.10	18664.77	17536.72
COEFF. OF VAR. (csq known)	23.48	22.92	22.39	22.00
COEFF. OF VAR. (csq estimated)	22.77	21.24	22.28	21.13
ESTIMATE OF C-SQUARED	.36	.33	.39	.35

The value given is the average over 100 simulations.

Table 6.3 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	79709.19	79709.19	79709.19	79709.19
TRUE STD DEV	22634.38	22039.63	21736.15	21428.38
TRUE COEFF. OF VARIATION	28.34	27.58	27.20	26.81
DAILY TOTAL (one sample)	77120.00	78673.33	78160.00	77700.00
STANDARD DEV. (one sample)	25606.64	26645.99	25495.98	25276.74
COEFF. OF VAR. (one sample)	32.34	32.43	31.48	30.97
DAILY TOTAL (two sample)	77610.00	78830.00	78077.50	79923.33
STANDARD DEV. (csq known)	21833.51	21958.62	21438.55	21455.11
STANDARD DEV. (csq estimated)	21866.18	22253.04	20627.68	21611.11
COEFF. OF VAR. (csq known)	27.74	27.18	26.88	26.16
COEFF. OF VAR. (csq estimated)	27.07	26.19	25.61	25.64
ESTIMATE OF C-SQUARED	.58	.61	.55	.56

The value given is the average over 100 simulations.

Table 6.3 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	79709.19	79709.19	79709.19	79709.19
TRUE STD DEV	30739.56	30304.31	30084.32	29862.71
TRUE COEFF. OF VARIATION	38.46	37.91	37.63	37.35
DAILY TOTAL (one sample)	75880.00	78700.00	85180.00	82463.33
STANDARD DEV. (one sample)	30189.90	30359.33	33012.99	31318.42
COEFF. OF VAR. (one sample)	36.17	35.36	35.46	35.04
DAILY TOTAL (two sample)	78220.00	80683.33	81990.00	81563.33
STANDARD DEV. (csq known)	30188.19	30760.52	31533.24	30357.47
STANDARD DEV. (csq estimated)	31260.84	32213.35	30833.10	30460.63
COEFF. OF VAR. (csq known)	36.88	36.60	36.82	35.73
COEFF. OF VAR. (csq estimated)	36.38	36.59	34.96	34.87
ESTIMATE OF C-SQUARED	1.45	1.41	1.25	1.33

The value given is the average over 100 simulations.

Table 6.4  
Simulation of Sampling for One Day with Uniform  
Variation of Density About Level of 250 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	201714.34	201714.34	201714.34	201714.34
TRUE STD DEV	14202.62	11596.39	10042.77	8199.89
TRUE COEFF. OF VARIATION	7.07	5.78	5.00	4.08
DAILY TOTAL (one sample)	201060.00	202180.00	201315.00	201910.00
STANDARD DEV. (one sample)	54180.44	53806.15	53175.35	53136.08
COEFF. OF VAR. (one sample)	26.80	26.46	26.29	26.16
DAILY TOTAL (two sample)	201535.00	201033.33	200425.00	201473.33
STANDARD DEV. (csq known)	14196.30	11576.80	10010.62	8194.98
STANDARD DEV. (csq estimated)	15573.86	13160.21	10928.75	8853.12
COEFF. OF VAR. (csq known)	7.08	5.79	5.02	4.09
COEFF. OF VAR. (csq estimated)	7.69	6.50	5.43	4.39
ESTIMATE OF C-SQUARED	.01	.01	0.00	0.00

The value given is the average over 100 simulations.

Table 6.4 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	201714.34	201714.34	201714.34	201714.34
TRUE STD DEV	21829.96	20231.39	19382.73	18495.16
TRUE COEFF. OF VARIATION	10.82	10.02	9.59	9.15
DAILY TOTAL (one sample)	201830.00	200626.67	200440.00	199596.67
STANDARD DEV. (one sample)	55293.22	54863.10	54445.27	53840.27
COEFF. OF VAR. (one sample)	27.14	27.03	26.93	26.74
DAILY TOTAL (two sample)	202800.00	201753.33	200035.00	199636.67
STANDARD DEV. (csq known)	21906.75	20276.63	19257.12	18323.55
STANDARD DEV. (csq estimated)	21982.49	20893.38	19024.13	18126.84
COEFF. OF VAR. (csq known)	10.78	10.02	9.60	9.14
COEFF. OF VAR. (csq estimated)	10.55	9.93	9.20	8.78
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.4 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	201714.34	201714.34	201714.34	201714.34
TRUE STD DEV	27411.32	26156.11	25505.35	24837.55
TRUE COEFF. OF VARIATION	13.57	12.94	12.61	12.27
DAILY TOTAL (one sample)	201670.00	206053.33	204905.00	200580.00
STANDARD DEV. (one sample)	56705.82	57200.23	57002.94	55451.55
COEFF. OF VAR. (one sample)	27.78	27.35	27.36	27.30
DAILY TOTAL (two sample)	202955.00	203683.33	204710.00	203793.33
STANDARD DEV. (csq known)	27649.18	26352.31	25920.95	25135.28
STANDARD DEV. (csq estimated)	27449.86	27008.35	26114.99	25020.46
COEFF. OF VAR. (csq known)	13.56	12.85	12.57	12.26
COEFF. OF VAR. (csq estimated)	12.94	12.81	12.14	11.99
ESTIMATE OF C-SQUARED	.10	.11	.11	.11

The value given is the average over 100 simulations.

Table 6.4 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	201714.34	201714.34	201714.34	201714.34
TRUE STD DEV	45098.96	44347.24	43966.56	43582.56
TRUE COEFF. OF VARIATION	22.28	21.90	21.71	21.52
DAILY TOTAL (one sample)	199070.00	210920.00	199875.00	203116.67
STANDARD DEV. (one sample)	60529.74	63189.17	61446.56	61520.86
COEFF. OF VAR. (one sample)	29.59	29.00	29.82	29.27
DAILY TOTAL (two sample)	201720.00	209593.33	200425.00	203663.33
STANDARD DEV. (csq known)	44805.71	45643.61	43811.97	44257.00
STANDARD DEV. (csq estimated)	46917.05	44801.44	42960.13	41653.66
COEFF. OF VAR. (csq known)	21.88	21.43	21.48	21.25
COEFF. OF VAR. (csq estimated)	21.92	20.33	20.85	19.44
ESTIMATE OF C-SQUARED	.40	.33	.36	.31

The value given is the average over 100 simulations.

Table 6.4 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	201714.34	201714.34	201714.34	201714.34
TRUE STD DEV	54314.31	53691.77	53377.78	53061.92
TRUE COEFF. OF VARIATION	26.83	26.51	26.35	26.20
DAILY TOTAL (one sample)	195740.00	203673.33	210075.00	200853.33
STANDARD DEV. (one sample)	63652.18	66512.52	67444.40	66310.29
COEFF. OF VAR. (one sample)	31.31	31.01	30.61	31.24
DAILY TOTAL (two sample)	201245.00	205940.00	206165.00	201586.67
STANDARD DEV. (csq known)	53970.54	55062.82	54218.29	53457.57
STANDARD DEV. (csq estimated)	52566.67	53476.85	56345.03	55124.74
COEFF. OF VAR. (csq known)	26.29	25.96	25.57	25.86
COEFF. OF VAR. (csq estimated)	24.95	24.43	25.29	25.85
ESTIMATE OF C-SQUARED	.52	.51	.58	.60

The value given is the average over 100 simulations.

Table 6.4 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	201714.34	201714.34	201714.34	201714.34
TRUE STD DEV	75487.57	75040.89	74816.55	74591.54
TRUE COEFF. OF VARIATION	37.27	37.05	36.93	36.82
DAILY TOTAL (one sample)	203900.00	204320.00	210225.00	192256.67
STANDARD DEV. (one sample)	77385.08	74912.72	78845.56	73258.33
COEFF. OF VAR. (one sample)	35.09	34.66	34.40	33.72
DAILY TOTAL (two sample)	203810.00	204473.33	204277.50	190016.67
STANDARD DEV. (csq known)	77094.85	74346.66	75832.91	70290.71
STANDARD DEV. (csq estimated)	77695.24	72854.31	74297.89	75305.56
COEFF. OF VAR. (csq known)	36.14	35.29	35.40	34.95
COEFF. OF VAR. (csq estimated)	34.38	34.31	34.99	35.14
ESTIMATE OF C-SQUARED	1.32	1.25	1.28	1.41

The value given is the average over 100 simulations.

Table 6.5  
Simulation of Sampling for One Day with Diel  
Variation of Density About Level of 10 Organisms/1000 m<sup>3</sup>  
SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	7905.42	7905.42	7905.42	7905.42
TRUE STD DEV	2811.66	2295.71	1988.14	1623.31
TRUE COEFF. OF VARIATION	35.83	29.26	25.34	20.69
DAILY TOTAL (one sample)	8030.00	8180.00	7900.00	8006.67
STANDARD DEV. (one sample)	3984.97	3757.66	3477.43	3328.66
COEFF. OF VAR. (one sample)	49.87	44.92	43.31	40.51
DAILY TOTAL (two sample)	7845.00	8150.00	7950.00	7991.67
STANDARD DEV. (csq known)	2800.89	2330.95	1993.74	1632.14
STANDARD DEV. (csq estimated)	3083.02	2554.73	2265.50	1863.84
COEFF. OF VAR. (csq known)	37.27	29.21	25.59	20.73
COEFF. OF VAR. (csq estimated)	40.62	31.70	28.52	23.28
ESTIMATE OF C-SQUARED	.26	.11	.09	.06

The value given is the average over 100 simulations.

Table 6.5 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	7905.42	7905.42	7905.42	7905.42
TRUE STD DEV	2952.32	2465.98	2182.54	1856.32
TRUE COEFF. OF VARIATION	37.56	31.35	27.73	23.55
DAILY TOTAL (one sample)	7430.00	8193.33	7650.00	7913.33
STANDARD DEV. (one sample)	3845.78	3784.18	3420.53	3314.61
COEFF. OF VAR. (one sample)	52.07	44.84	43.84	40.86
DAILY TOTAL (two sample)	7745.00	7960.00	7837.50	7886.67
STANDARD DEV. (csq known)	2922.14	2472.57	2169.78	1849.45
STANDARD DEV. (csq estimated)	3351.87	2703.91	2492.74	2037.43
COEFF. OF VAR. (csq known)	39.06	31.71	28.00	23.63
COEFF. OF VAR. (csq estimated)	43.35	33.51	31.18	25.12
ESTIMATE OF C-SQUARED	.46	.17	.18	.12

The value given is the average over 100 simulations.

Table 6.5 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	7905.42	7905.42	7905.42	7905.42
TRUE STD DEV	3086.58	2625.23	2360.99	2063.18
TRUE COEFF. OF VARIATION	39.21	33.31	29.92	26.10
DAILY TOTAL (one sample)	8230.00	7733.33	7800.00	7953.33
STANDARD DEV. (one sample)	4159.33	3839.56	3561.95	3389.53
COEFF. OF VAR. (one sample)	50.31	47.67	44.18	41.40
DAILY TOTAL (two sample)	8170.00	7673.33	7792.50	8061.67
STANDARD DEV. (csq known)	3147.86	2594.46	2344.33	2088.69
STANDARD DEV. (csq estimated)	3419.06	2843.32	2509.23	2181.87
COEFF. OF VAR. (csq known)	39.63	34.31	30.31	26.00
COEFF. OF VAR. (csq estimated)	41.99	36.74	31.61	26.84
ESTIMATE OF C-SQUARED	.51	.38	.22	.18

The value given is the average over 100 simulations.

Table 6.5 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	7905.42	7905.42	7905.42	7905.42
TRUE STD DEV	3648.37	3267.34	3059.07	2835.55
TRUE COEFF. OF VARIATION	46.13	41.22	38.53	35.63
DAILY TOTAL (one sample)	8150.00	7673.33	7405.00	7506.67
STANDARD DEV. (one sample)	4418.14	3916.91	3607.28	3538.99
COEFF. OF VAR. (one sample)	52.65	48.38	46.13	43.95
DAILY TOTAL (two sample)	8125.00	7716.67	7797.50	7771.67
STANDARD DEV. (csq known)	3731.04	3230.45	3055.05	2834.66
STANDARD DEV. (csq estimated)	3767.63	3249.96	3166.43	3083.92
COEFF. OF VAR. (csq known)	45.96	41.49	38.23	35.42
COEFF. OF VAR. (csq estimated)	44.14	40.02	37.91	36.22
ESTIMATE OF C-SQUARED	.84	.61	.68	.58

The value given is the average over 100 simulations.

Table 6.5 (continued)

SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	7905.42	7905.42	7905.42	7905.42
TRUE STD DEV	4001.66	3657.62	3472.84	3277.65
TRUE COEFF. OF VARIATION	50.48	46.03	43.64	41.10
DAILY TOTAL (one sample)	8040.00	8326.67	8595.00	8050.00
STANDARD DEV. (one sample)	4371.50	4396.97	4350.86	3875.42
COEFF. OF VAR. (one sample)	53.07	48.83	46.93	44.10
DAILY TOTAL (two sample)	8090.00	8003.33	8005.00	7951.67
STANDARD DEV. (csq known)	4043.51	3751.21	3439.91	3328.05
STANDARD DEV. (csq estimated)	4054.63	3813.72	3638.68	3001.76
COEFF. OF VAR. (csq known)	49.29	45.41	42.13	39.94
COEFF. OF VAR. (csq estimated)	48.07	44.05	42.82	36.15
ESTIMATE OF C-SQUARED	1.15	1.00	1.20	.73

The value given is the average over 100 simulations.

Table 6.5 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	7905.42	7905.42	7905.42	7905.42
TRUE STD DEV	4911.33	4635.30	4490.93	4341.76
TRUE COEFF. OF VARIATION	61.71	58.12	56.24	54.29
DAILY TOTAL (one sample)	7810.00	7806.67	8080.00	8880.00
STANDARD DEV. (one sample)	4929.50	4379.75	4513.04	5203.31
COEFF. OF VAR. (one sample)	58.85	51.94	50.73	47.78
DAILY TOTAL (two sample)	7740.00	7706.67	7760.00	8166.67
STANDARD DEV. (csq known)	4846.99	4400.51	4314.90	4575.74
STANDARD DEV. (csq estimated)	4354.31	4394.44	4122.20	4933.45
COEFF. OF VAR. (csq known)	59.02	53.47	52.45	50.69
COEFF. OF VAR. (csq estimated)	53.71	51.38	47.51	49.19
ESTIMATE OF C-SQUARED	1.39	2.63	3.36	2.90

The value given is the average over 100 simulations.

Table 6.6  
Simulation of Sampling for One Day with Diel  
Variation of Density About Level of 50 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39562.33	39562.33	39562.33	39562.33
TRUE STD DEV	6289.86	5135.65	4447.60	3631.45
TRUE COEFF. OF VARIATION	16.01	13.08	11.32	9.25
DAILY TOTAL (one sample)	38270.00	39233.33	39855.00	39640.00
STANDARD DEV. (one sample)	15115.55	15041.72	15150.66	14865.92
COEFF. OF VAR. (one sample)	39.05	37.68	37.27	36.98
DAILY TOTAL (two sample)	39440.00	39500.00	40012.50	39516.67
STANDARD DEV. (csq known)	6280.13	5131.60	4472.83	3629.36
STANDARD DEV. (csq estimated)	6855.65	5949.79	4879.68	4015.66
COEFF. OF VAR. (csq known)	16.09	13.12	11.31	9.28
COEFF. OF VAR. (csq estimated)	17.38	14.93	12.18	10.18
ESTIMATE OF C-SQUARED	.02	.02	.01	.01

The value given is the average over 100 simulations.

Table 6.6 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39562.33	39562.33	39562.33	39562.33
TRUE STD DEV	7753.99	6851.05	6351.63	5809.43
TRUE COEFF. OF VARIATION	19.63	17.31	16.03	14.63
DAILY TOTAL (one sample)	37950.00	39793.33	39615.00	39660.00
STANDARD DEV. (one sample)	15450.24	15600.43	15278.91	15143.72
COEFF. OF VAR. (one sample)	39.37	38.20	37.63	37.24
DAILY TOTAL (two sample)	38195.00	40043.33	40032.50	39708.33
STANDARD DEV. (csq known)	7577.85	6908.70	6389.39	5835.47
STANDARD DEV. (csq estimated)	7843.15	7029.30	6525.81	5729.12
COEFF. OF VAR. (csq known)	19.90	17.25	15.91	14.61
COEFF. OF VAR. (csq estimated)	20.08	16.96	15.62	13.39
ESTIMATE OF C-SQUARED	.07	.06	.06	.05

The value given is the average over 100 simulations.

Table 6.6 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39562.33	39562.33	39562.33	39562.33
TRUE STD DEV	8982.56	8215.78	7804.18	7369.64
TRUE COEFF. OF VARIATION	22.66	20.69	19.62	18.50
DAILY TOTAL (one sample)	39690.00	39460.00	39490.00	39813.33
STANDARD DEV. (one sample)	16148.99	16062.66	15768.72	15462.68
COEFF. OF VAR. (one sample)	39.13	38.80	38.64	37.56
DAILY TOTAL (two sample)	38690.00	39073.33	39735.00	39805.00
STANDARD DEV. (csq known)	8772.01	8146.59	7861.59	7425.07
STANDARD DEV. (csq estimated)	9005.55	8165.51	8236.05	7673.22
COEFF. OF VAR. (csq known)	22.58	20.57	19.56	18.35
COEFF. OF VAR. (csq estimated)	22.57	19.88	18.88	17.86
ESTIMATE OF C-SQUARED	.16	.12	.12	.12

The value given is the average over 100 simulations.

Table 6.6 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39562.33	39562.33	39562.33	39562.33
TRUE STD DEV	13290.65	12784.91	12524.38	12258.32
TRUE COEFF. OF VARIATION	33.35	32.03	31.36	30.66
DAILY TOTAL (one sample)	37710.00	37700.00	37100.00	41733.33
STANDARD DEV. (one sample)	16841.32	16143.39	16062.61	17885.81
COEFF. OF VAR. (one sample)	42.15	40.11	39.52	39.21
DAILY TOTAL (two sample)	38055.00	39360.00	37280.00	41216.67
STANDARD DEV. (csq known)	12810.03	12662.11	11689.38	12798.71
STANDARD DEV. (csq estimated)	12701.38	12600.00	11596.66	12226.16
COEFF. OF VAR. (csq known)	32.67	31.11	30.28	29.74
COEFF. OF VAR. (csq estimated)	31.33	28.91	28.49	25.72
ESTIMATE OF C-SQUARED	.44	.43	.43	.33

The value given is the average over 100 simulations.

Table 6.6 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39562.33	39562.33	39562.33	39562.33
TRUE STD DEV	15658.32	15231.40	15013.39	14792.16
TRUE COEFF. OF VARIATION	39.23	38.12	37.55	36.98
DAILY TOTAL (one sample)	36100.00	40506.67	40825.00	37390.00
STANDARD DEV. (one sample)	16428.33	18816.90	19006.38	16908.64
COEFF. OF VAR. (one sample)	41.64	41.52	40.75	40.21
DAILY TOTAL (two sample)	37435.00	39366.67	39302.50	39071.67
STANDARD DEV. (csq known)	14635.40	15231.87	15222.89	14815.46
STANDARD DEV. (csq estimated)	15835.25	15229.80	16166.44	14832.30
COEFF. OF VAR. (csq known)	37.35	36.56	35.86	35.53
COEFF. OF VAR. (csq estimated)	37.57	32.88	34.93	33.15
ESTIMATE OF C-SQUARED	.80	.58	1.09	.63

The value given is the average over 100 simulations.

Table 6.6 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	39562.33	39562.33	39562.33	39562.33
TRUE STD DEV	21232.14	20919.28	20761.08	20601.67
TRUE COEFF. OF VARIATION	53.11	52.29	51.88	51.46
DAILY TOTAL (one sample)	41360.00	35640.00	37990.00	36420.00
STANDARD DEV. (one sample)	22547.51	19210.65	20103.92	18136.73
COEFF. OF VAR. (one sample)	46.02	43.44	43.00	43.08
DAILY TOTAL (two sample)	40240.00	38396.67	38757.50	37743.33
STANDARD DEV. (csq known)	21601.31	19640.38	20813.71	19767.19
STANDARD DEV. (csq estimated)	23337.52	21854.42	21581.10	20738.08
COEFF. OF VAR. (csq known)	49.27	46.98	46.79	45.91
COEFF. OF VAR. (csq estimated)	49.19	48.31	43.88	43.34
ESTIMATE OF C-SQUARED	5.54	2.92	1.86	1.68

The value given is the average over 100 simulations.

Table 6.7  
Simulation of Sampling for One Day with Diel  
Variation of Density About Level of 100 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	78634.06	78634.06	78634.06	78634.06
TRUE STD DEV	8867.58	7240.35	6270.33	5119.70
TRUE COEFF. OF VARIATION	11.34	9.26	8.02	6.55
DAILY TOTAL (one sample)	78320.00	78926.67	78895.00	78500.00
STANDARD DEV. (one sample)	29791.78	29603.75	29142.84	28815.39
COEFF. OF VAR. (one sample)	37.38	36.77	36.48	36.22
DAILY TOTAL (two sample)	77425.00	78310.00	78735.00	79225.00
STANDARD DEV. (csq known)	8799.15	7225.42	6274.35	5138.90
STANDARD DEV. (csq estimated)	10425.69	8140.84	6882.59	5908.42
COEFF. OF VAR. (csq known)	11.45	9.30	8.03	6.53
COEFF. OF VAR. (csq estimated)	13.24	10.38	8.72	7.38
ESTIMATE OF C-SQUARED	.02	.01	.01	.01

The value given is the average over 100 simulations.

Table 6.7 (continued)

SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	78634.06	78634.06	78634.06	78634.06
TRUE STD DEV	12623.03	11538.18	10955.54	10340.12
TRUE COEFF. OF VARIATION	16.03	14.62	13.87	13.07
DAILY TOTAL (one sample)	78490.00	78246.67	77380.00	79433.33
STANDARD DEV. (one sample)	30343.04	29923.61	29104.77	30299.34
COEFF. OF VAR. (one sample)	37.60	37.45	36.70	37.06
DAILY TOTAL (two sample)	79275.00	78526.67	78185.00	79340.00
STANDARD DEV. (csq known)	12680.80	11550.62	10864.95	10479.61
STANDARD DEV. (csq estimated)	13084.53	12072.19	11538.20	10518.08
COEFF. OF VAR. (csq known)	15.94	14.62	13.80	13.06
COEFF. OF VAR. (csq estimated)	15.81	14.32	13.65	12.30
ESTIMATE OF C-SQUARED	.06	.06	.06	.06

The value given is the average over 100 simulations.

Table 6.7 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	78634.06	78634.06	78634.06	78634.06
TRUE STD DEV	15493.47	14623.14	14167.94	13697.63
TRUE COEFF. OF VARIATION	19.62	18.49	17.90	17.29
DAILY TOTAL (one sample)	78210.00	77720.00	77145.00	78856.67
STANDARD DEV. (one sample)	30873.61	30550.43	30134.24	30629.58
COEFF. OF VAR. (one sample)	38.29	38.12	37.59	37.44
DAILY TOTAL (two sample)	78525.00	78660.00	77715.00	79501.67
STANDARD DEV. (csq known)	15477.32	14659.18	14078.84	13849.14
STANDARD DEV. (csq estimated)	16310.58	14857.55	14222.87	13852.94
COEFF. OF VAR. (csq known)	19.55	18.39	17.75	17.15
COEFF. OF VAR. (csq estimated)	19.60	17.92	17.21	16.00
ESTIMATE OF C-SQUARED	.14	.12	.11	.10

The value given is the average over 100 simulations.

Table 6.7 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	78634.06	78634.06	78634.06	78634.06
TRUE STD DEV	24832.92	24299.44	24028.26	23753.98
TRUE COEFF. OF VARIATION	31.33	30.63	30.28	29.92
DAILY TOTAL (one sample)	78820.00	76206.67	81320.00	75736.67
STANDARD DEV. (one sample)	33701.63	32764.62	34547.50	31187.28
COEFF. OF VAR. (one sample)	40.01	39.30	39.18	38.28
DAILY TOTAL (two sample)	79810.00	79520.00	79085.00	77495.00
STANDARD DEV. (csq known)	24830.25	24733.47	24189.48	23419.02
STANDARD DEV. (csq estimated)	26076.62	27165.26	23848.11	25388.81
COEFF. OF VAR. (csq known)	30.09	29.93	29.27	28.63
COEFF. OF VAR. (csq estimated)	29.28	28.14	26.33	27.65
ESTIMATE OF C-SQUARED	.44	.51	.36	.44

The value given is the average over 100 simulations.

Table 6.7 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	78634.06	78634.06	78634.06	78634.06
TRUE STD DEV	29760.75	29317.07	29092.70	28866.58
TRUE COEFF. OF VARIATION	37.52	36.94	36.65	36.35
DAILY TOTAL (one sample)	74240.00	78960.00	76245.00	78360.00
STANDARD DEV. (one sample)	32922.18	35167.60	34626.15	34388.43
COEFF. OF VAR. (one sample)	39.74	39.85	39.98	39.91
DAILY TOTAL (two sample)	77070.00	78400.00	72532.50	77905.00
STANDARD DEV. (csq known)	28932.82	29127.03	26556.29	29042.61
STANDARD DEV. (csq estimated)	27273.06	28516.58	28219.30	24643.27
COEFF. OF VAR. (csq known)	35.29	35.01	34.42	34.37
COEFF. OF VAR. (csq estimated)	31.33	32.94	32.77	29.05
ESTIMATE OF C-SQUARED	.51	.68	.70	.50

The value given is the average over 100 simulations.

Table 6.7 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	78634.06	78634.06	78634.06	78634.06
TRUE STD DEV	41143.29	40823.51	40662.68	40501.21
TRUE COEFF. OF VARIATION	51.83	51.41	51.20	50.99
DAILY TOTAL (one sample)	71010.00	79933.33	75930.00	81146.67
STANDARD DEV. (one sample)	36668.38	40420.68	39048.91	41516.41
COEFF. OF VAR. (one sample)	42.87	43.11	43.21	43.19
DAILY TOTAL (two sample)	70160.00	80856.67	74727.50	80931.67
STANDARD DEV. (csq known)	35488.73	42449.28	38651.39	41050.04
STANDARD DEV. (csq estimated)	35877.99	36801.18	37426.75	40008.84
COEFF. OF VAR. (csq known)	45.91	46.36	46.48	46.57
COEFF. OF VAR. (csq estimated)	43.36	40.95	43.69	43.54
ESTIMATE OF C-SQUARED	1.73	1.58	1.61	1.53

The value given is the average over 100 simulations.

Table 6.8  
Simulation of Sampling for One Day with Diel  
Variation of Density About Level of 250 Organisms/1000 m<sup>3</sup>

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	197901.02	197901.02	197901.02	197901.02
TRUE STD DEV	14067.73	11486.25	9947.39	8122.01
TRUE COEFF. OF VARIATION	7.17	5.85	5.07	4.14
DAILY TOTAL (one sample)	199370.00	197673.33	196330.00	198413.33
STANDARD DEV. (one sample)	74067.94	72579.92	72088.14	72568.70
COEFF. OF VAR. (one sample)	36.40	36.16	36.17	36.04
DAILY TOTAL (two sample)	199010.00	198050.00	196752.50	198248.33
STANDARD DEV. (csq known)	14107.09	11490.58	9918.48	8129.13
STANDARD DEV. (csq estimated)	15876.08	12570.07	11135.92	9278.50
COEFF. OF VAR. (csq known)	7.16	5.86	5.08	4.14
COEFF. OF VAR. (csq estimated)	7.94	6.30	5.60	4.65
ESTIMATE OF C-SQUARED	.01	0.00	0.00	0.00

The value given is the average over 100 simulations.

Table 6.8 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	197901.02	197901.02	197901.02	197901.02
TRUE STD DEV	26738.78	25475.39	24819.59	24145.98
TRUE COEFF. OF VARIATION	13.41	12.75	12.41	12.06
DAILY TOTAL (one sample)	198290.00	196013.33	197585.00	197940.00
STANDARD DEV. (one sample)	74681.39	73976.00	74101.48	74168.61
COEFF. OF VAR. (one sample)	36.72	36.81	36.48	36.40
DAILY TOTAL (two sample)	198590.00	197836.67	196885.00	197965.00
STANDARD DEV. (csq known)	26759.34	25461.84	24691.99	24119.79
STANDARD DEV. (csq estimated)	26872.48	26108.28	24899.05	24801.20
COEFF. OF VAR. (csq known)	13.35	12.72	12.35	11.96
COEFF. OF VAR. (csq estimated)	12.75	12.24	11.54	11.34
ESTIMATE OF C-SQUARED	.05	.06	.06	.05

The value given is the average over 100 simulations.

Table 6.8 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	197901.02	197901.02	197901.02	197901.02
TRUE STD DEV	35100.20	34147.57	33661.15	33167.60
TRUE COEFF. OF VARIATION	17.55	17.05	16.80	16.54
DAILY TOTAL (one sample)	201120.00	197520.00	199060.00	196833.33
STANDARD DEV. (one sample)	77949.73	75639.50	76383.78	75424.28
COEFF. OF VAR. (one sample)	37.15	37.07	36.78	36.99
DAILY TOTAL (two sample)	199580.00	197660.00	196995.00	196591.67
STANDARD DEV. (csq known)	35279.40	33867.13	33460.60	32894.39
STANDARD DEV. (csq estimated)	38917.99	32860.78	30520.12	33284.66
COEFF. OF VAR. (csq known)	17.37	16.82	16.56	16.42
COEFF. OF VAR. (csq estimated)	17.23	14.76	14.30	15.56
ESTIMATE OF C-SQUARED	.12	.10	.09	.10

The value given is the average over 100 simulations.

Table 6.8 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	197901.02	197901.02	197901.02	197901.02
TRUE STD DEV	60373.36	59824.54	59548.23	59270.64
TRUE COEFF. OF VARIATION	30.10	29.82	29.67	29.52
DAILY TOTAL (one sample)	206010.00	207346.67	198025.00	207193.33
STANDARD DEV. (one sample)	88565.74	87946.42	82324.18	87123.85
COEFF. OF VAR. (one sample)	39.01	39.03	38.43	38.95
DAILY TOTAL (two sample)	200535.00	206943.33	198322.50	201836.67
STANDARD DEV. (csq known)	61349.67	63769.10	58996.41	60888.17
STANDARD DEV. (csq estimated)	66573.83	62657.00	61095.86	59292.57
COEFF. OF VAR. (csq known)	29.10	29.08	28.58	28.65
COEFF. OF VAR. (csq estimated)	28.56	25.20	26.54	25.92
ESTIMATE OF C-SQUARED	.44	.33	.41	.36

The value given is the average over 100 simulations.

Table 6.8 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	197901.02	197901.02	197901.02	197901.02
TRUE STD DEV	73270.15	72818.60	72591.77	72364.23
TRUE COEFF. OF VARIATION	36.52	36.28	36.16	36.04
DAILY TOTAL (one sample)	204930.00	199560.00	203765.00	197510.00
STANDARD DEV. (one sample)	91791.23	89631.42	93073.29	91016.73
COEFF. OF VAR. (one sample)	40.32	39.83	39.70	39.17
DAILY TOTAL (two sample)	203700.00	198363.33	203932.50	208858.33
STANDARD DEV. (csq known)	76008.41	72889.67	76356.38	75987.06
STANDARD DEV. (csq estimated)	74049.65	72163.53	72822.77	81650.13
COEFF. OF VAR. (csq known)	34.90	34.50	34.09	34.07
COEFF. OF VAR. (csq estimated)	31.22	32.85	30.94	33.75
ESTIMATE OF C-SQUARED	.59	.64	.54	.76

The value given is the average over 100 simulations.

Table 6.8 (continued)

## SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to diurnal perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE DAILY TOTAL	197901.02	197901.02	197901.02	197901.02
TRUE STD DEV	102660.26	102338.47	102177.19	102015.66
TRUE COEFF. OF VARIATION	51.14	50.97	50.88	50.80
DAILY TOTAL (one sample)	199140.00	194400.00	200850.00	191456.67
STANDARD DEV. (one sample)	99741.97	105160.68	101733.43	96379.72
COEFF. OF VAR. (one sample)	42.20	42.28	41.84	41.69
DAILY TOTAL (two sample)	205700.00	204720.00	200625.00	200303.33
STANDARD DEV. (csq known)	103691.82	106867.63	103907.53	101982.63
STANDARD DEV. (csq estimated)	102056.85	116753.07	97171.64	106045.32
COEFF. OF VAR. (csq known)	45.65	46.30	46.04	45.74
COEFF. OF VAR. (csq estimated)	43.07	43.00	41.83	45.66
ESTIMATE OF C-SQUARED	2.05	1.70	1.32	1.96

The value given is the average over 100 simulations.

$c^2$   
 $\lambda$ , used to generate  $L_i$ ,  $i=1, \dots, 8$ , in uniform or diel pattern  
 $v$

The index  $i$  represents the period within the day, ranging from 1 to 8. From  $v$  and  $L_i$ ,  $i=1, \dots, 8$ , we can obtain the expected value, or mean, of the number of organisms in a sample of volume  $v$  drawn during period  $i$ ,

$$\mu_i = vL_i. \quad (6.6)$$

Then, using  $c^2$  and  $\mu_i$ , we generate two independent, identically distributed random observations  $X_i$  and  $Y_i$  from the distribution

$$\text{Negative binomial}(1/c^2, 1/(c^2\mu_i+1)),$$

whose probability density function is (II.3) in Appendix II, using (II.4) and (II.6). Applying general formulas for the mean and variance of a Negative binomial random variable (see Mood, Graybill, and Boes, 1974, p. 538) to this distribution shows routinely that for  $i=1, \dots, 8$ ,

$$E(X_i) = \mu_i, \text{Var}(X_i) = \mu_i + c^2\mu_i^2, \quad (6.7)$$

and likewise for  $Y_i$ .

The parameters of the situation are the true underlying values of the various quantities of interest. The following important parameters cannot be calculated from the sample data, observations  $X_i$  and  $Y_i$ ; they can be calculated in this simulation from the values of  $c^2$  and  $\mu_i$ , as well as  $v$  and  $V$ , but would be unknown in a real sampling situation. Throughout this section, all summations  $\Sigma$  will indicate sums as  $i$  ranges from 1 to 8. In addition, it will be useful to define the sampling ratio  $R = V/v$  and the variable for  $i=1, \dots, 8$

$$Z_i = (X_i + Y_i)/2, \quad (6.8)$$

which has  $E(Z_i) = \mu_i$  and  $\text{var}(Z_i) = (\mu_i + c^2 \mu_i^2)/2$ .

As was noted in Section 6.1, the true realized value  $T$  of the sampling day's total organism count is given by

$$T = R \sum \mu_i. \quad (6.9)$$

The one-sample and two-sample estimators of  $T$  are

$$\hat{T}_1 = R \sum X_i \quad (6.10)$$

$$\hat{T}_2 = R \sum Z_i.$$

Both are unbiased estimators of  $T$ , since (for example)

$$E(\hat{T}_1) = R \sum E(X_i) = R \sum \mu_i = T \quad (6.11)$$

Not surprisingly, the variance of the one-sample estimator is twice that of the two-sample estimator:

$$\text{var}(\hat{T}_1) = R^2 \sum \text{var}(X_i) = R^2 \sum (\mu_i + c^2 \mu_i^2) \quad (6.12)$$

$$\text{var}(\hat{T}_2) = R^2 \sum \text{var}(Z_i) = \frac{1}{2} R^2 \sum (\mu_i + c^2 \mu_i^2).$$

Standard deviations (SD) and coefficients of variation (CV) are easily obtained as

$$SD(\hat{T}_1) = [\text{var}(\hat{T}_1)]^{1/2} \quad (6.13)$$

$$CV(\hat{T}_1) = SD(\hat{T}_1)/E(\hat{T}_1) = SD(\hat{T}_1)/T$$

and similarly for  $\hat{T}_2$ .

We now examine the variability of the one-sample estimator of  $T$ . Under the assumption that  $c^2 = 1$ , the estimated variance, SD, and CV of  $\hat{T}_1$  are

$$\begin{aligned} \text{est var}(\hat{T}_1) &= R^2 \Sigma \text{est var}(X_i) = R^2 \Sigma X_i(1+X_i)/2 \\ \text{est SD}(\hat{T}_1) &= [\text{est var}(\hat{T}_1)]^{1/2} \\ \text{est CV}(\hat{T}_1) &= \text{est SD}(\hat{T}_1)/\hat{T}_1. \end{aligned} \quad (6.14)$$

The variability of the two-sample estimator of  $T$  must be considered in two ways. Let  $\hat{T}_2^c$  denote the estimator  $\hat{T}_2$  when  $c^2$  is known,  $\hat{T}_2^{\hat{c}^2}$  the estimator when  $c^2$  must be estimated. When the value of  $c^2$  is known.

$$\text{est var}(\hat{T}_2^c) = R^2 \Sigma (Z_i + c^2 Z_i^2) / (2 + c^2). \quad (6.15)$$

This choice of estimator for the variance of  $\hat{T}_2^c$  is based on this estimator's unbiasedness for  $\text{var}(\hat{T}_2)$ , that is,

$$E[\text{est var}(\hat{T}_2^c)] = \text{var}(\hat{T}_2). \quad (6.16)$$

When the value of  $c^2$  is unknown and must be estimated, substituting the estimate  $\hat{c}^2$  for  $c^2$  gives

$$\text{est var}(\hat{T}_2^{\hat{c}^2}) = R^2 \Sigma (Z_i + \hat{c}^2 Z_i^2) / (2 + \hat{c}^2). \quad (6.17)$$

For both of these situations, estimated SD's and CV's are calculated in the usual way.

It is informative to see the effect of erroneously assuming in these computations that each observation  $X_i$  or  $Y_i$  has a Poisson distribution:

$$\begin{aligned} \text{est var}_p(\hat{T}_1) &= R^2 \Sigma X_i = R\hat{T}_1 \\ \text{est var}_p(\hat{T}_2) &= R^2 \Sigma Z_i / 2 = R\hat{T}_2 / 2. \end{aligned} \quad (6.18)$$

These formulas will be instrumental in assessing the accuracy of various entrainment estimation procedures over an entire sampling season. This will be treated in Section 6.4.

### 6.3 RESULTS OF THE SIMULATION STUDY OF DAILY ENTRAINMENT

The accuracy of entrainment estimation is affected by the sample volume  $v$ , the organism density  $\lambda$ , the quadratic variance parameter  $c^2$ , and the temporal pattern (uniform or diel variation). The relative performance of one-sample and two-sample procedures is also of interest. The behavior of estimation procedures can be judged by examining the standard deviations and coefficients of variation associated with them. The results reported in Tables 6.1 to 6.8 will now be discussed in detail.

The sample volume  $v$  assumed values of 100, 150, 200, and 300 cubic meters per 3-hour period of this study. Over the range from 150 to 300, changes in sample volume can be seen to have relatively little impact on the accuracy of entrainment estimation. Increasing the sample volume results in lower standard deviations and coefficients of variation, as we should expect, but the changes are small as  $v$  increases from 150 to 300. These changes can be observed by reading from left to right across any row of Tables 6.1 to 6.8. The slight gains in accuracy are not sufficient to justify using sample volumes larger than 150; it would be much more productive to increase the amount of replicate sampling done instead of increasing the volume  $v$  of each individual sample. This point will be returned to in Section 6.4, where sampling season calculations are discussed. (Other values of  $v$  were considered in preliminary work but were found to lie outside the range of usefulness: they were either too small to provide reasonably accurate estimation or too large to be practical for the same reasons as  $v = 300$ .)

The density  $\lambda$  of organisms in the discharge channel has a greater effect than the sample volume does on entrainment estimation. This confirms the intuitive belief that the estimation of sparsely distributed organisms and the estimation of more densely distributed ones can be achieved with differing levels of relative and absolute accuracy. The values  $\lambda = 10, 50, 100, \text{ and } 250$  organisms per 1000 cubic meters were used in the simulation study. As  $\lambda$  increases while the combination of  $c^2$ ,  $v$ , and temporal pattern is fixed, standard deviations increase but coefficients of variation decrease. For example, with  $c^2 = .10$ ,  $v = 150$ , and uniform variation, two-sample estimates with  $c^2$  estimated (as it would be if its value were not known in advance) can be extracted from Tables 6.1 to 6.4, giving

Density $\lambda$	10	50	100	250
Estd daily total $\hat{T}$	7,940.00	39,900.00	79,923.33	203,683.33
Estd std dev of $\hat{T}$	2,624.67	6,798.37	12,059.94	27,008.35
Estd CV of $\hat{T}$	33.06%	16.78%	14.45%	12.81%

This display illustrates that as density increases, the variability of entrainment estimation increases when viewed in absolute terms, as a standard deviation, but decreases when viewed in relative terms, as a coefficient of variation. This distinction makes it clear that the decision whether to measure the efficiency of entrainment estimation procedures in terms of relative or absolute error is an important choice with far-reaching implications.

Two temporal patterns were examined. Under uniform variation, each of a simulated day's eight 3-hour sampling periods had its density  $L$  drawn independently from a uniform distribution centered at  $\lambda$ . Under diel variation, three of a day's sampling periods had their densities  $L$  drawn independently from a uniform distribution centered at  $2.25\lambda$ , while the remaining five periods had densities  $L$  drawn independently from a uniform distribution centered at  $.25\lambda$ . Once these  $L$ 's had been randomly generated for a given day, sample counts were simulated using these realized densities  $L$ , as described in Section 6.1.

There is little difference between these two temporal patterns in the simulation results. For given values of the average daily density  $\lambda$ ,  $c^2$ , and  $v$ , the diel pattern produces greater variability in the entrainment estimates than the uniform pattern does. This is not surprising, because the diel pattern puts most of the organisms that appear during a day into three relatively dense periods rather than spreading the organisms fairly evenly over eight periods. Therefore the bulk of the work in estimating entrainment must be done by three samples of volume  $v$  rather than by eight samples. Aside from this natural difference between the uniform and diel patterns, the simulation results for these two temporal patterns are quite similar.

It is straightforward to adapt the results of this simulation study to situations in which the volume  $V$  of the discharge channel flow differs from the value of 100,000 cubic meters per 3-hour period used here. Conversion of the results in Tables 6.1 to 6.8 to apply to a discharge channel flow of  $V$  cubic meters per 3-hour period requires that:

- (i) daily totals, both true and estimated, must be adjusted by multiplying each table entry of this type by  $V/100,000$ ;
- (ii) standard deviations, both true and estimated, must be adjusted by multiplying each table entry of this type by  $V/100,000$ ;
- (iii) coefficients of variation, both true and estimated, are not changed and thus need no adjustment.

The simulation of daily entrainment sampling is not intended to illustrate every sampling situation that actually occurs, but rather a representative range of situations. There are two reasons for this: first, implementing an exhaustive approach would require prohibitive amounts of time and effort; second, the consequent proliferation of detail would complicate enormously the tasks of assimilating, presenting, and comprehending the study's results, without improving our understanding of the entrainment process in any way. Any actual one-day sampling situation is a combination of a density pattern over the day's eight 3-hour periods, an average organism density  $\lambda$ , a sample volume  $v$ , a discharge channel flow volume  $V$ , and a quadratic variance parameter  $c^2$ . A broad range of combinations of these variables was analyzed in the simulation study. Any actual sampling situation can be handled by (i) approximation if it is very similar to one of the cases analyzed, (ii) interpolation if one or more variables assume values between those analyzed, (iii) extrapolation if one or more variables assume values outside, but not too far from, the range analyzed, or (iv) a combination of these three methods.

This is a more effective and illuminating approach than attempting to simulate every potential sampling situation that might occur.

The daily simulation study can be used to examine the behavior of entrainment and its estimation over an entire sampling season. This is the primary issue in the evaluation of entrainment sampling designs. It is the topic of the rest of this chapter.

#### 6.4 SIMULATION RESULTS CONCERNING ESTIMATION OF $c^2$

The estimates of  $c^2$  in Tables 6.1 to 6.8 are somewhat unsatisfactory. Comparing the true value of  $c^2$ , given in the table headings, to the estimates on the bottom line of each page of these tables, we see that the average estimate of  $c^2$  over 100 simulations can be far from the true value of  $c^2$ . This happens when organism density is low,  $c^2$  is high, or both. This deficiency must be remedied before we can seriously consider using  $\hat{c}^2$  in our estimation procedures for entrainment abundance.

In Tables 6.1 to 6.8,  $c^2$  is estimated in each simulation based on only eight paired 3-hour observations. This small number of pairs cannot be expected to lead to very accurate estimates of  $c^2$ . Not only can the average  $\hat{c}^2$  over 100 simulations under specified conditions differ substantially from the true value of  $c^2$ , but the individual  $\hat{c}^2$ 's vary greatly from one simulation to another.

Using a larger number of paired observations should improve this situation. This is logical on both mathematical and practical grounds: we would be uncomfortable using only eight paired observations to estimate  $c^2$  and using the resulting  $\hat{c}^2$ , which is likely to be inaccurate, in seasonal calculations. The key issue to be resolved is the number of paired observations needed to produce more accurate estimates of  $c^2$ .

Tables 6.9 to 6.22 address this question. Tables 6.9 to 6.12 report the results of simulating one week, consisting of 56 3-hour periods, rather than one day, which contains only 8 3-hour periods. The mean density of organisms is  $\lambda = 10, 50, 100,$  and  $250$  organisms per 1000 cubic meters in Tables 6.9, 6.10, 6.11, and 6.12, respectively. Perturbation of the density about these mean levels is uniform, as in Tables 6.1 to 6.4. Results are quite similar for diel variation of the density, which is not tabled in this report. Tables 6.13, 6.14, 6.15, and 6.16 are identical to Tables 6.9 to 6.12, except that they simulate five days, consisting of 40 3-hour periods, rather than 7 days. As with Tables 6.9 to 6.12, the density is uniformly perturbed, and similar results for diel variation are not reported. Tables 6.17 to 6.20 are also identical to Tables 6.9 to 6.12, except that they simulate three days, consisting of 24 3-hour periods, rather than 7 days. Similar results for diel variation are not reported. Tables 6.21 and 6.22 report on simulations of 18-day and 34-day entrainment seasons, each consisting of a series of days of different mean organism intensity. Again, only uniform variation of the density level is discussed in detail; results for the case of diel variation are quite similar to these.

In these tables, the average estimate of  $c^2$  over 100 simulations is always very close to the true value of  $c^2$ . Estimation improves as the mean density of organisms increases, becoming very good indeed for  $\lambda = 100$  and  $250$  organisms per 1000 cubic meters. This tells us that when we have a reasonably large number of paired observations, e.g., 24 to 56 of them, the

[Text continues on page 6.146]

Table 6.9  
Simulation of Sampling for One Week with Uniform  
Variation of Density About Level of 10 Organisms/1000 m<sup>3</sup>

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	56132.56	56132.56	56132.56	56132.56
TRUE STD DEV	7492.17	6117.33	5297.76	4325.60
TRUE COEFF. OF VARIATION	13.35	10.90	9.44	7.71
WEEKLY TOTAL (one sample)	55900.00	57293.33	57055.00	55423.33
STANDARD DEV. (one sample)	9269.30	8341.06	7769.97	6949.50
COEFF. OF VAR. (one sample)	16.61	14.56	13.59	12.53
WEEKLY TOTAL (two sample)	55915.00	56080.00	56645.00	55666.67
STANDARD DEV. (csq known)	7477.63	6114.46	5321.89	4307.62
STANDARD DEV. (csq estimated)	7694.48	6316.47	5594.86	4464.55
COEFF. OF VAR. (csq known)	13.41	10.93	9.42	7.75
COEFF. OF VAR. (csq estimated)	13.78	11.30	9.87	8.03
ESTIMATE OF C-SQUARED	.06	.05	.05	.03

The value given is the average over 100 simulations.

Table 6.9 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	56132.56	56132.56	56132.56	56132.56
TRUE STD DEV	7692.68	6361.33	5577.73	4664.29
TRUE COEFF. OF VARIATION	13.71	11.34	9.94	8.31
WEEKLY TOTAL (one sample)	53720.00	55106.67	55225.00	56133.33
STANDARD DEV. (one sample)	9131.81	8212.73	7649.84	7122.89
COEFF. OF VAR. (one sample)	17.04	14.90	13.83	12.67
WEEKLY TOTAL (two sample)	55300.00	55843.33	56025.00	56228.33
STANDARD DEV. (csq known)	7633.99	6345.36	5573.18	4669.48
STANDARD DEV. (csq estimated)	7863.84	6491.53	5677.15	4679.45
COEFF. OF VAR. (csq known)	13.87	11.40	9.97	8.32
COEFF. OF VAR. (csq estimated)	14.26	11.64	10.11	8.29
ESTIMATE OF C-SQUARED	.13	.09	.07	.05

The value given is the average over 100 simulations.

Table 6.9 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	56132.56	56132.56	56132.56	56132.56
TRUE STD DEV	7888.10	6596.31	5844.30	4980.00
TRUE COEFF. OF VARIATION	14.06	11.76	10.42	8.87
WEEKLY TOTAL (one sample)	57170.00	56660.00	55670.00	54536.67
STANDARD DEV. (one sample)	9534.15	8493.66	7833.10	7047.54
COEFF. OF VAR. (one sample)	16.72	15.00	14.05	12.89
WEEKLY TOTAL (two sample)	55995.00	56420.00	55717.50	55583.33
STANDARD DEV. (csq known)	7879.25	6613.85	5826.11	4950.73
STANDARD DEV. (csq estimated)	7933.16	6850.47	5915.97	4905.33
COEFF. OF VAR. (csq known)	14.13	11.75	10.48	8.92
COEFF. OF VAR. (csq estimated)	14.22	12.13	10.61	8.80
ESTIMATE OF C-SQUARED	.13	.17	.13	.09

The value given is the average over 100 simulations.

Table 6.9 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	56132.56	56132.56	56132.56	56132.56
TRUE STD DEV	8742.48	7597.37	6954.47	6245.74
TRUE COEFF. OF VARIATION	15.58	13.54	12.39	11.13
WEEKLY TOTAL (one sample)	54800.00	54880.00	55920.00	57023.33
STANDARD DEV. (one sample)	9558.77	8707.09	8270.13	7745.68
COEFF. OF VAR. (one sample)	17.43	15.86	14.74	13.55
WEEKLY TOTAL (two sample)	55855.00	56193.33	56550.00	55860.00
STANDARD DEV. (csq known)	8693.21	7620.26	7002.56	6211.84
STANDARD DEV. (csq estimated)	8704.88	7688.38	6970.47	6089.52
COEFF. OF VAR. (csq known)	15.60	13.58	12.38	11.11
COEFF. OF VAR. (csq estimated)	15.59	13.60	12.26	10.87
ESTIMATE OF C-SQUARED	.40	.37	.33	.32

The value given is the average over 100 simulations.

Table 6.9 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	56132.56	56132.56	56132.56	56132.56
TRUE STD DEV	9304.86	8238.30	7649.45	7011.33
TRUE COEFF. OF VARIATION	16.58	14.68	13.63	12.49
WEEKLY TOTAL (one sample)	56910.00	55673.33	53980.00	54573.33
STANDARD DEV. (one sample)	10173.99	9126.03	8350.90	7840.56
COEFF. OF VAR. (one sample)	17.83	16.29	15.38	14.31
WEEKLY TOTAL (two sample)	57320.00	55630.00	55342.50	55481.67
STANDARD DEV. (csq known)	9451.67	8234.70	7559.35	6953.30
STANDARD DEV. (csq estimated)	9633.28	8263.04	7553.06	6980.41
COEFF. OF VAR. (csq known)	16.51	14.79	13.64	12.52
COEFF. OF VAR. (csq estimated)	16.73	14.79	13.55	12.52
ESTIMATE OF C-SQUARED	.64	.58	.52	.53

The value given is the average over 100 simulations.

Table 6.9 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	56132.56	56132.56	56132.56	56132.56
TRUE STD DEV	10817.96	9915.51	9431.97	8922.25
TRUE COEFF. OF VARIATION	19.27	17.66	16.80	15.89
WEEKLY TOTAL (one sample)	54440.00	55686.67	57385.00	56533.33
STANDARD DEV. (one sample)	10582.06	9881.75	9467.44	9007.03
COEFF. OF VAR. (one sample)	19.38	17.62	16.40	15.79
WEEKLY TOTAL (two sample)	56215.00	56526.67	55522.50	56863.33
STANDARD DEV. (csq known)	10853.95	9964.53	9286.03	9078.85
STANDARD DEV. (csq estimated)	10830.28	9875.22	9184.70	9229.00
COEFF. OF VAR. (csq known)	19.29	17.58	16.67	15.88
COEFF. OF VAR. (csq estimated)	19.11	17.32	16.45	16.08
ESTIMATE OF C-SQUARED	1.15	1.03	1.05	1.15

The value given is the average over 100 simulations.

Table 6.10  
Simulation of Sampling for One Week with Uniform  
Variation of Density About Level of 50 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	279716.68	279716.68	279716.68	279716.68
TRUE STD DEV	16724.73	13655.69	11826.17	9656.03
TRUE COEFF. OF VARIATION	5.98	4.88	4.23	3.45
WEEKLY TOTAL (one sample)	279550.00	279206.67	277540.00	278406.67
STANDARD DEV. (one sample)	32201.40	30638.61	29728.35	29041.31
COEFF. OF VAR. (one sample)	11.50	10.96	10.70	10.42
WEEKLY TOTAL (two sample)	278810.00	278303.33	278800.00	279345.00
STANDARD DEV. (csq known)	16697.60	13621.14	11806.78	9649.61
STANDARD DEV. (csq estimated)	17227.01	14113.59	12228.86	10019.84
COEFF. OF VAR. (csq known)	6.00	4.90	4.24	3.46
COEFF. OF VAR. (csq estimated)	6.18	5.07	4.38	3.58
ESTIMATE OF C-SQUARED	.01	.01	.01	0.00

The value given is the average over 100 simulations.

Table 6.10 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	279716.68	279716.68	279716.68	279716.68
TRUE STD DEV	18851.82	16191.12	14681.04	12996.67
TRUE COEFF. OF VARIATION	6.74	5.79	5.25	4.65
WEEKLY TOTAL (one sample)	277640.00	277960.00	282005.00	280680.00
STANDARD DEV. (one sample)	32563.17	31092.62	30750.98	29883.94
COEFF. OF VAR. (one sample)	11.71	11.17	10.89	10.63
WEEKLY TOTAL (two sample)	279810.00	280396.67	279155.00	280436.67
STANDARD DEV. (csq known)	18858.57	16218.14	14666.57	13014.75
STANDARD DEV. (csq estimated)	18917.19	16066.60	14417.70	13223.97
COEFF. OF VAR. (csq known)	6.75	5.79	5.26	4.64
COEFF. OF VAR. (csq estimated)	6.74	5.71	5.14	4.69
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.10 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	279716.68	279716.68	279716.68	279716.68
TRUE STD DEV	20762.12	18380.06	17064.80	15639.31
TRUE COEFF. OF VARIATION	7.42	6.57	6.10	5.59
WEEKLY TOTAL (one sample)	278980.00	276546.67	274215.00	277360.00
STANDARD DEV. (one sample)	33402.25	31643.50	30490.20	30103.32
COEFF. OF VAR. (one sample)	11.95	11.41	11.09	10.83
WEEKLY TOTAL (two sample)	277765.00	279230.00	277080.00	279543.33
STANDARD DEV. (csq known)	20676.21	18361.40	16933.33	15612.36
STANDARD DEV. (csq estimated)	21055.52	18201.04	16861.94	15669.04
COEFF. OF VAR. (csq known)	7.45	6.58	6.11	5.58
COEFF. OF VAR. (csq estimated)	7.55	6.48	6.06	5.57
ESTIMATE OF C-SQUARED	.11	.10	.10	.10

The value given is the average over 100 simulations.

Table 6.10 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	279716.68	279716.68	279716.68	279716.68
TRUE STD DEV	28003.72	26286.30	25384.05	24448.53
TRUE COEFF. OF VARIATION	10.01	9.40	9.07	8.74
WEEKLY TOTAL (one sample)	276130.00	281446.67	282130.00	279393.33
STANDARD DEV. (one sample)	35304.11	34559.51	34155.78	33204.95
COEFF. OF VAR. (one sample)	12.73	12.23	12.03	11.83
WEEKLY TOTAL (two sample)	279905.00	277986.67	282552.50	278235.00
STANDARD DEV. (csq known)	27993.30	26071.86	25670.49	24360.03
STANDARD DEV. (csq estimated)	27647.33	25680.60	25853.70	24825.22
COEFF. OF VAR. (csq known)	9.99	9.36	9.07	8.74
COEFF. OF VAR. (csq estimated)	9.79	9.17	9.05	8.85
ESTIMATE OF C-SQUARED	.32	.32	.34	.36

The value given is the average over 100 simulations.

Table 6.10 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	279716.68	279716.68	279716.68	279716.68
TRUE STD DEV	32194.12	30711.93	29943.33	29154.48
TRUE COEFF. OF VARIATION	11.51	10.98	10.70	10.42
WEEKLY TOTAL (one sample)	272190.00	271060.00	277635.00	275390.00
STANDARD DEV. (one sample)	36860.95	35147.50	35495.28	34371.21
COEFF. OF VAR. (one sample)	13.46	12.87	12.71	12.40
WEEKLY TOTAL (two sample)	277135.00	276073.33	281080.00	279421.67
STANDARD DEV. (csq known)	32030.81	30337.60	30097.53	29155.59
STANDARD DEV. (csq estimated)	31850.67	30016.29	30542.14	28940.45
COEFF. OF VAR. (csq known)	11.53	10.96	10.68	10.41
COEFF. OF VAR. (csq estimated)	11.39	10.81	10.75	10.27
ESTIMATE OF C-SQUARED	.51	.50	.53	.50

The value given is the average over 100 simulations.

Table 6.10 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	279716.68	279716.68	279716.68	279716.68
TRUE STD DEV	42346.27	41230.66	40661.38	40084.02
TRUE COEFF. OF VARIATION	15.14	14.74	14.53	14.33
WEEKLY TOTAL (one sample)	283050.00	282066.67	279990.00	272660.00
STANDARD DEV. (one sample)	42584.50	41239.84	40726.47	39847.77
COEFF. OF VAR. (one sample)	14.92	14.41	14.39	14.31
WEEKLY TOTAL (two sample)	276495.00	282426.67	279132.50	277048.33
STANDARD DEV. (csq known)	41630.06	41593.91	40610.87	40157.69
STANDARD DEV. (csq estimated)	41206.01	41226.53	41346.54	40390.43
COEFF. OF VAR. (csq known)	14.98	14.63	14.46	14.32
COEFF. OF VAR. (csq estimated)	14.78	14.44	14.63	14.27
ESTIMATE OF C-SQUARED	1.02	1.02	1.14	1.08

The value given is the average over 100 simulations.

Table 6.11  
Simulation of Sampling for One Week with Uniform  
Variation of Density About Level of 100 Organisms/1000 m<sup>3</sup>

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	559434.61	559434.61	559434.61	559434.61
TRUE STD DEV	23652.37	19312.08	16724.75	13655.70
TRUE COEFF. OF VARIATION	4.23	3.45	2.99	2.44
WEEKLY TOTAL (one sample)	558880.00	558633.33	558720.00	561216.67
STANDARD DEV. (one sample)	59884.47	58264.07	57409.99	56904.05
COEFF. OF VAR. (one sample)	10.70	10.42	10.27	10.13
WEEKLY TOTAL (two sample)	558680.00	559696.67	558102.50	560765.00
STANDARD DEV. (csq known)	23636.41	19316.60	16704.83	13671.93
STANDARD DEV. (csq estimated)	24528.55	20106.66	17449.75	14284.43
COEFF. OF VAR. (csq known)	4.23	3.45	2.99	2.44
COEFF. OF VAR. (csq estimated)	4.39	3.59	3.12	2.54
ESTIMATE OF C-SQUARED	.01	.01	0.00	0.00

The value given is the average over 100 simulations.

Table 6.11 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	559434.61	559434.61	559434.61	559434.61
TRUE STD DEV	29359.08	25989.95	24129.62	22113.33
TRUE COEFF. OF VARIATION	5.25	4.65	4.31	3.95
WEEKLY TOTAL (one sample)	560490.00	556440.00	557255.00	563413.33
STANDARD DEV. (one sample)	61369.05	59379.27	58650.98	58382.84
COEFF. OF VAR. (one sample)	10.93	10.66	10.51	10.35
WEEKLY TOTAL (two sample)	562300.00	557700.00	556107.50	559011.67
STANDARD DEV. (csq known)	29478.30	25935.47	24019.64	22113.86
STANDARD DEV. (csq estimated)	29671.20	25743.43	24157.94	21780.38
COEFF. OF VAR. (csq known)	5.24	4.65	4.32	3.96
COEFF. OF VAR. (csq estimated)	5.25	4.60	4.32	3.88
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.11 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	559434.61	559434.61	559434.61	559434.61
TRUE STD DEV	34124.43	31272.97	29744.91	28133.97
TRUE COEFF. OF VARIATION	6.10	5.59	5.32	5.03
WEEKLY TOTAL (one sample)	560860.00	558493.33	554360.00	551643.33
STANDARD DEV. (one sample)	62461.75	60809.68	59604.19	58374.87
COEFF. OF VAR. (one sample)	11.11	10.87	10.72	10.57
WEEKLY TOTAL (two sample)	561320.00	557283.33	559305.00	555203.33
STANDARD DEV. (csq known)	34194.06	31190.11	29769.51	27926.91
STANDARD DEV. (csq estimated)	34406.25	31893.47	29651.22	27797.92
COEFF. OF VAR. (csq known)	6.09	5.60	5.32	5.03
COEFF. OF VAR. (csq estimated)	6.10	5.69	5.27	4.97
ESTIMATE OF C-SQUARED	.10	.11	.10	.10

The value given is the average over 100 simulations.

Table 6.11 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	559434.61	559434.61	559434.61	559434.61
TRUE STD DEV	50756.50	48885.01	47921.86	46938.96
TRUE COEFF. OF VARIATION	9.07	8.74	8.56	8.39
WEEKLY TOTAL (one sample)	554730.00	556646.67	566120.00	563926.67
STANDARD DEV. (one sample)	67384.49	66063.91	66062.21	65412.20
COEFF. OF VAR. (one sample)	12.09	11.81	11.60	11.54
WEEKLY TOTAL (two sample)	559595.00	563360.00	559595.50	563626.67
STANDARD DEV. (csq known)	50837.81	49255.70	47520.22	47302.47
STANDARD DEV. (csq estimated)	50938.15	48674.02	46967.45	48152.07
COEFF. OF VAR. (csq known)	9.07	8.73	8.52	8.38
COEFF. OF VAR. (csq estimated)	9.03	8.59	8.36	8.47
ESTIMATE OF C-SQUARED	.34	.33	.33	.35

The value given is the average over 100 simulations.

Table 6.11 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	559434.61	559434.61	559434.61	559434.61
TRUE STD DEV	59871.91	58293.80	57488.51	56671.77
TRUE COEFF. OF VARIATION	10.70	10.42	10.27	10.13
WEEKLY TOTAL (one sample)	556820.00	548866.67	542975.00	552023.33
STANDARD DEV. (one sample)	71530.20	68888.99	67360.54	67873.61
COEFF. OF VAR. (one sample)	12.74	12.48	12.32	12.22
WEEKLY TOTAL (two sample)	553640.00	558400.00	550707.50	560683.33
STANDARD DEV. (csq known)	59355.22	58219.43	56628.14	56747.65
STANDARD DEV. (csq estimated)	60166.52	58607.32	56190.05	57164.58
COEFF. OF VAR. (csq known)	10.68	10.40	10.26	10.08
COEFF. OF VAR. (csq estimated)	10.78	10.39	10.14	10.08
ESTIMATE OF C-SQUARED	.53	.52	.50	.52

The value given is the average over 100 simulations.

Table 6.11 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	559434.61	559434.61	559434.61	559434.61
TRUE STD DEV	81301.03	80145.99	79562.18	78974.05
TRUE COEFF. OF VARIATION	14.53	14.32	14.22	14.11
WEEKLY TOTAL (one sample)	557310.00	561313.33	564605.00	556296.67
STANDARD DEV. (one sample)	80732.71	79212.63	79419.88	78420.95
COEFF. OF VAR. (one sample)	14.30	13.93	13.96	13.96
WEEKLY TOTAL (two sample)	563030.00	553433.33	567392.50	555438.33
STANDARD DEV. (csq known)	81961.45	78821.66	80738.62	78167.78
STANDARD DEV. (csq estimated)	80692.13	78306.72	80137.64	79473.77
COEFF. OF VAR. (csq known)	14.49	14.15	14.15	14.00
COEFF. OF VAR. (csq estimated)	14.20	14.01	13.95	14.15
ESTIMATE OF C-SQUARED	.99	1.04	1.03	1.13

The value given is the average over 100 simulations.

Table 6.12  
Simulation of Sampling for One Week with Uniform  
Variation of Density About Level of 250 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	1405554.17	1405554.17	1405554.17	1405554.17
TRUE STD DEV	37490.72	30611.05	26509.94	21645.28
TRUE COEFF. OF VARIATION	2.67	2.18	1.89	1.54
WEEKLY TOTAL (one sample)	1408470.00	1402053.33	1401735.00	1402340.00
STANDARD DEV. (one sample)	143535.19	141176.09	140394.88	139458.59
COEFF. OF VAR. (one sample)	10.19	10.06	10.01	9.94
WEEKLY TOTAL (two sample)	1405470.00	1404433.33	1402070.00	1403565.00
STANDARD DEV. (csq known)	37489.60	30598.84	26477.07	21629.96
STANDARD DEV. (csq estimated)	39355.05	31744.05	27411.95	22413.77
COEFF. OF VAR. (csq known)	2.67	2.18	1.89	1.54
COEFF. OF VAR. (csq estimated)	2.80	2.26	1.95	1.60
ESTIMATE OF C-SQUARED	0.00	0.00	0.00	0.00

The value given is the average over 100 simulations.

Table 6.12 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared=.05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	1405554.17	1405554.17	1405554.17	1405554.17
TRUE STD DEV	57569.05	53344.89	51102.04	48756.12
TRUE COEFF. OF VARIATION	4.10	3.79	3.64	3.47
WEEKLY TOTAL (one sample)	1407530.00	1408980.00	1395145.00	1404303.33
STANDARD DEV. (one sample)	146672.77	144916.45	142770.89	142992.51
COEFF. OF VAR. (one sample)	10.41	10.28	10.22	10.17
WEEKLY TOTAL (two sample)	1412855.00	1404743.33	1398350.00	1402126.67
STANDARD DEV. (csq known)	57809.05	53264.93	50857.15	48615.63
STANDARD DEV. (csq estimated)	58234.23	51626.46	50359.63	49341.63
COEFF. OF VAR. (csq known)	4.09	3.79	3.64	3.47
COEFF. OF VAR. (csq estimated)	4.09	3.66	3.58	3.50
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.12 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	1405554.17	1405554.17	1405554.17	1405554.17
TRUE STD DEV	72269.20	68951.57	67231.39	65466.03
TRUE COEFF. OF VARIATION	5.14	4.90	4.78	4.66
WEEKLY TOTAL (one sample)	1412490.00	1409786.67	1403330.00	1412233.33
STANDARD DEV. (one sample)	150525.78	148492.89	147267.21	147454.96
COEFF. OF VAR. (one sample)	10.64	10.51	10.47	10.42
WEEKLY TOTAL (two sample)	1410560.00	1405913.33	1409927.50	1410490.00
STANDARD DEV. (csq known)	72501.79	69041.40	67516.61	65784.03
STANDARD DEV. (csq estimated)	73367.64	68759.19	66931.73	65684.96
COEFF. OF VAR. (csq known)	5.14	4.91	4.78	4.66
COEFF. OF VAR. (csq estimated)	5.17	4.85	4.71	4.63
ESTIMATE OF C-SQUARED	.10	.10	.10	.10

The value given is the average over 100 simulations.

Table 6.12 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	1405554.17	1405554.17	1405554.17	1405554.17
TRUE STD DEV	118868.43	116881.08	115874.62	114859.34
TRUE COEFF. OF VARIATION	8.45	8.31	8.24	8.17
WEEKLY TOTAL (one sample)	1418790.00	1404753.33	1417380.00	1385730.00
STANDARD DEV. (one sample)	165870.01	163093.67	162878.51	158535.19
COEFF. OF VAR. (one sample)	11.63	11.55	11.44	11.40
WEEKLY TOTAL (two sample)	1407465.00	1414450.00	1403277.50	1404611.67
STANDARD DEV. (csq known)	118941.64	117464.90	115287.60	115182.13
STANDARD DEV. (csq estimated)	118489.94	118640.94	114843.90	115621.25
COEFF. OF VAR. (csq known)	8.43	8.28	8.20	8.19
COEFF. OF VAR. (csq estimated)	8.36	8.31	8.10	8.16
ESTIMATE OF C-SQUARED	.33	.35	.34	.34

The value given is the average over 100 simulations.

Table 6.12 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	1405554.17	1405554.17	1405554.17	1405554.17
TRUE STD DEV	143150.16	141504.25	140674.06	139838.96
TRUE COEFF. OF VARIATION	10.18	10.06	10.00	9.94
WEEKLY TOTAL (one sample)	1426410.00	1410980.00	1412735.00	1427950.00
STANDARD DEV. (one sample)	175567.37	171628.79	172980.22	176234.23
COEFF. OF VAR. (one sample)	12.23	12.08	12.17	12.23
WEEKLY TOTAL (two sample)	1426480.00	1407846.67	1407460.00	1417908.33
STANDARD DEV. (csq known)	145927.87	141649.88	141044.85	141878.29
STANDARD DEV. (csq estimated)	143985.59	141590.35	139916.56	143206.88
COEFF. OF VAR. (csq known)	10.19	10.03	9.99	9.96
COEFF. OF VAR. (csq estimated)	9.99	9.94	9.87	10.01
ESTIMATE OF C-SQUARED	.50	.50	.50	.52

The value given is the average over 100 simulations.

Table 6.12 (continued)

## SIMULATION OF SAMPLING FOR ONE WEEK- FIFTY SIX 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE WEEKLY TOTAL	1405554.17	1405554.17	1405554.17	1405554.17
TRUE STD DEV	198943.17	197762.15	197168.98	196574.03
TRUE COEFF. OF VARIATION	14.15	14.06	14.02	13.98
WEEKLY TOTAL (one sample)	1418480.00	1417106.67	1382510.00	1422156.67
STANDARD DEV. (one sample)	204112.15	198366.22	193167.41	198437.87
COEFF. OF VAR. (one sample)	14.10	13.87	13.74	13.70
WEEKLY TOTAL (two sample)	1420615.00	1402173.33	1408890.00	1428013.33
STANDARD DEV. (csq known)	200883.77	195432.32	199019.10	200637.73
STANDARD DEV. (csq estimated)	201967.56	197644.80	199128.19	197043.70
COEFF. OF VAR. (csq known)	14.00	13.87	14.00	13.95
COEFF. OF VAR. (csq estimated)	13.95	13.97	13.90	13.55
ESTIMATE OF C-SQUARED	1.06	1.09	1.04	.98

The value given is the average over 100 simulations.

Table 6.13

Simulation of Sampling for Five Days with Uniform  
Variation of Density About Level of 10 Organisms/1000 m<sup>3</sup>

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	40039.39	40039.39	40039.39	40039.39
TRUE STD DEV	6327.67	5166.52	4474.34	3653.28
TRUE COEFF. OF VARIATION	15.82	12.92	11.18	9.13
SEASON TOTAL (one sample)	38770.00	39866.67	40435.00	40276.67
STANDARD DEV. (one sample)	7681.15	6918.25	6500.77	5944.00
COEFF. OF VAR. (one sample)	19.86	17.36	16.07	14.74
SEASON TOTAL (two sample)	39365.00	40140.00	40020.00	40151.67
STANDARD DEV. (csq known)	6274.15	5173.01	4473.25	3658.40
STANDARD DEV. (csq estimated)	6518.05	5366.98	4670.92	3830.80
COEFF. OF VAR. (csq known)	15.99	12.93	11.21	9.13
COEFF. OF VAR. (csq estimated)	16.58	13.40	11.69	9.54
ESTIMATE OF C-SQUARED	.08	.05	.05	.03

The value given is the average over 100 simulations.

Table 6.13 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	40039.39	40039.39	40039.39	40039.39
TRUE STD DEV	6496.92	5372.47	4710.65	3939.16
TRUE COEFF. OF VARIATION	16.24	13.43	11.77	9.84
SEASON TOTAL (one sample)	39860.00	39953.33	40670.00	39893.33
STANDARD DEV. (one sample)	7921.49	7001.90	6621.56	6011.01
COEFF. OF VAR. (one sample)	19.89	17.54	16.25	15.03
SEASON TOTAL (two sample)	40715.00	40010.00	40360.00	40188.33
STANDARD DEV. (csq known)	6552.78	5369.79	4730.16	3948.13
STANDARD DEV. (csq estimated)	6795.22	5584.70	4887.74	4080.65
COEFF. OF VAR. (csq known)	16.17	13.48	11.76	9.84
COEFF. OF VAR. (csq estimated)	16.72	13.97	12.11	10.11
ESTIMATE OF C-SQUARED	.15	.12	.09	.08

The value given is the average over 100 simulations.

Table 6.13 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	40039.39	40039.39	40039.39	40039.39
TRUE STD DEV	6661.86	5570.81	4935.66	4205.65
TRUE COEFF. OF VARIATION	16.65	13.92	12.33	10.51
SEASON TOTAL (one sample)	39360.00	40413.33	39755.00	40620.00
STANDARD DEV. (one sample)	7886.70	7165.66	6605.11	6178.37
COEFF. OF VAR. (one sample)	20.04	17.73	16.59	15.16
SEASON TOTAL (two sample)	39825.00	40540.00	39617.50	40766.67
STANDARD DEV. (csq known)	6635.96	5607.69	4906.63	4249.72
STANDARD DEV. (csq estimated)	6768.68	5782.73	4969.28	4357.75
COEFF. OF VAR. (csq known)	16.73	13.88	12.42	10.44
COEFF. OF VAR. (csq estimated)	17.00	14.26	12.52	10.61
ESTIMATE OF C-SQUARED	.17	.17	.12	.12

The value given is the average over 100 simulations.

Table 6.13 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	40039.39	40039.39	40039.39	40039.39
TRUE STD DEV	7383.04	6415.83	5872.79	5274.13
TRUE COEFF. OF VARIATION	18.45	16.03	14.67	13.17
SEASON TOTAL (one sample)	38900.00	40573.33	40195.00	40333.33
STANDARD DEV. (one sample)	8085.17	7492.22	7030.65	6565.65
COEFF. OF VAR. (one sample)	20.77	18.43	17.44	16.21
SEASON TOTAL (two sample)	39970.00	40096.67	39910.00	40743.33
STANDARD DEV. (csq known)	7352.06	6410.47	5864.08	5332.01
STANDARD DEV. (csq estimated)	7353.23	6407.81	5919.25	5381.24
COEFF. OF VAR. (csq known)	18.43	16.02	14.70	13.08
COEFF. OF VAR. (csq estimated)	18.33	15.90	14.71	13.08
ESTIMATE OF C-SQUARED	.38	.36	.38	.36

The value given is the average over 100 simulations.

Table 6.13 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	40039.39	40039.39	40039.39	40039.39
TRUE STD DEV	7857.77	6956.87	6459.48	5920.44
TRUE COEFF. OF VARIATION	19.63	17.38	16.13	14.78
SEASON TOTAL (one sample)	40010.00	40573.33	40490.00	40166.67
STANDARD DEV. (one sample)	8410.11	7763.73	7267.05	6767.57
COEFF. OF VAR. (one sample)	21.00	19.09	17.85	16.73
SEASON TOTAL (two sample)	40340.00	40183.33	40527.50	40523.33
STANDARD DEV. (csq known)	7880.23	6964.58	6525.20	5944.58
STANDARD DEV. (csq estimated)	7811.53	6983.63	6590.62	5992.12
COEFF. OF VAR. (csq known)	19.54	17.34	16.08	14.63
COEFF. OF VAR. (csq estimated)	19.22	17.24	16.10	14.60
ESTIMATE OF C-SQUARED	.54	.56	.56	.55

The value given is the average over 100 simulations.

Table 6.13 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	40039.39	40039.39	40039.39	40039.39
TRUE STD DEV	9135.08	8372.77	7964.29	7533.71
TRUE COEFF. OF VARIATION	22.82	20.91	19.89	18.81
SEASON TOTAL (one sample)	39540.00	40646.67	40110.00	39516.67
STANDARD DEV. (one sample)	8943.15	8422.98	8035.23	7375.03
COEFF. OF VAR. (one sample)	22.64	20.50	19.73	18.47
SEASON TOTAL (two sample)	40370.00	40160.00	40962.50	39860.00
STANDARD DEV. (csq known)	9196.19	8418.23	8124.58	7441.87
STANDARD DEV. (csq estimated)	9140.57	8544.00	8228.53	7441.62
COEFF. OF VAR. (csq known)	22.75	20.86	19.73	18.55
COEFF. OF VAR. (csq estimated)	22.54	20.89	19.77	18.43
ESTIMATE OF C-SQUARED	1.21	1.21	1.19	1.11

The value given is the average over 100 simulations.

6.92

Table 6.14

Simulation of Sampling for Five Days with Uniform  
Variation of Density About Level of 50 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of  $\epsilon$ -squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	200835.19	200835.19	200835.19	200835.19
TRUE STD DEV	14171.63	11571.09	10020.86	8182.00
TRUE COEFF. OF VARIATION	7.06	5.77	4.99	4.08
SEASON TOTAL (one sample)	201960.00	200826.67	201290.00	200846.67
STANDARD DEV. (one sample)	27468.53	26079.54	25444.89	24657.97
COEFF. OF VAR. (one sample)	13.57	12.97	12.62	12.26
SEASON TOTAL (two sample)	201415.00	200893.33	200575.00	200431.67
STANDARD DEV. (csq known)	14192.08	11572.76	10014.36	8173.77
STANDARD DEV. (csq estimated)	14896.14	12046.58	10239.51	8410.87
COEFF. OF VAR. (csq known)	7.06	5.77	5.00	4.08
COEFF. OF VAR. (csq estimated)	7.39	5.99	5.11	4.20
ESTIMATE OF C-SQUARED	.02	.01	0.00	0.00

The value given is the average over 100 simulations.

Table 6.14 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	200835.19	200835.19	200835.19	200835.19
TRUE STD DEV	15981.45	13728.13	12449.46	11023.45
TRUE COEFF. OF VARIATION	7.96	6.84	6.20	5.49
SEASON TOTAL (one sample)	200960.00	200720.00	200655.00	200566.67
STANDARD DEV. (one sample)	27703.61	26473.13	25845.65	25182.60
COEFF. OF VAR. (one sample)	13.75	13.16	12.85	12.54
SEASON TOTAL (two sample)	200160.00	200293.33	200440.00	199946.67
STANDARD DEV. (csq known)	15955.22	13709.36	12443.49	10987.15
STANDARD DEV. (csq estimated)	15446.36	13214.64	11985.62	10531.22
COEFF. OF VAR. (csq known)	7.98	6.85	6.21	5.50
COEFF. OF VAR. (csq estimated)	7.72	6.57	5.97	5.23
ESTIMATE OF C-SQUARED	.04	.04	.04	.04

The value given is the average over 100 simulations.

Table 6.14 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	200835.19	200835.19	200835.19	200835.19
TRUE STD DEV	17606.19	15589.52	14476.20	13269.81
TRUE COEFF. OF VARIATION	8.77	7.76	7.21	6.61
SEASON TOTAL (one sample)	201540.00	201333.33	200840.00	201946.67
STANDARD DEV. (one sample)	28211.52	27066.09	26385.65	25930.38
COEFF. OF VAR. (one sample)	13.96	13.40	13.11	12.80
SEASON TOTAL (two sample)	200160.00	200590.00	200307.50	200683.33
STANDARD DEV. (csq known)	17585.29	15585.94	14471.51	13286.31
STANDARD DEV. (csq estimated)	16863.87	14960.61	13927.18	12818.87
COEFF. OF VAR. (csq known)	8.79	7.77	7.22	6.62
COEFF. OF VAR. (csq estimated)	8.41	7.41	6.91	6.34
ESTIMATE OF C-SQUARED	.03	.08	.09	.09

The value given is the average over 100 simulations.

Table 6.14 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	200835.19	200835.19	200835.19	200835.19
TRUE STD DEV	23762.18	22309.10	21545.85	20754.54
TRUE COEFF. OF VARIATION	11.83	11.11	10.72	10.33
SEASON TOTAL (one sample)	199620.00	200200.00	200205.00	200660.00
STANDARD DEV. (one sample)	30056.95	28960.51	28499.74	28018.90
COEFF. OF VAR. (one sample)	14.97	14.36	14.15	13.84
SEASON TOTAL (two sample)	199020.00	198896.67	199505.00	199721.67
STANDARD DEV. (csq known)	23683.62	22122.55	21503.17	20774.67
STANDARD DEV. (csq estimated)	22764.45	21339.84	20687.07	19843.12
COEFF. OF VAR. (csq known)	11.87	11.09	10.75	10.35
COEFF. OF VAR. (csq estimated)	11.38	10.62	10.28	9.84
ESTIMATE OF C-SQUARED	.30	.31	.30	.30

The value given is the average over 100 simulations.

Table 6.14 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	200835.19	200835.19	200835.19	200835.19
TRUE STD DEV	27323.06	26069.23	25419.13	24751.96
TRUE COEFF. OF VARIATION	13.60	12.98	12.65	12.32
SEASON TOTAL (one sample)	198490.00	199226.67	199805.00	199390.00
STANDARD DEV. (one sample)	31208.49	30456.82	29744.16	29281.56
COEFF. OF VAR. (one sample)	15.59	15.10	14.73	14.51
SEASON TOTAL (two sample)	197855.00	198226.67	199372.50	199200.00
STANDARD DEV. (csq known)	27090.35	26015.26	25422.06	24753.42
STANDARD DEV. (csq estimated)	26026.81	24925.04	23997.32	23715.35
COEFF. OF VAR. (csq known)	13.62	13.03	12.67	12.32
COEFF. OF VAR. (csq estimated)	13.03	12.42	11.92	11.79
ESTIMATE OF C-SQUARED	.46	.46	.45	.47

The value given is the average over 100 simulations.

Table 6.14 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	200835.19	200835.19	200835.19	200835.19
TRUE STD DEV	35948.07	35004.56	34523.13	34034.89
TRUE COEFF. OF VARIATION	17.89	17.42	17.18	16.94
SEASON TOTAL (one sample)	197930.00	198920.00	197995.00	195540.00
STANDARD DEV. (one sample)	35003.00	34204.09	33785.17	32558.85
COEFF. OF VAR. (one sample)	17.29	16.85	16.74	16.32
SEASON TOTAL (two sample)	198070.00	198570.00	197912.50	196275.00
STANDARD DEV. (csq known)	36138.30	35496.47	34635.09	33702.18
STANDARD DEV. (csq estimated)	34486.08	33253.14	32741.81	32262.01
COEFF. OF VAR. (csq known)	17.99	17.55	17.25	16.91
COEFF. OF VAR. (csq estimated)	17.07	16.41	16.35	16.11
ESTIMATE OF C-SQUARED	.96	.91	.96	.96

The value given is the average over 100 simulations.

6.98

Table 6.15  
Simulation of Sampling for Five Days with Uniform  
Variation of Density About Level of 100 Organisms/1000 m<sup>3</sup>  
 SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	399046.10	399046.10	399046.10	399046.10
TRUE STD DEV	19976.14	16310.45	14125.26	11533.23
TRUE COEFF. OF VARIATION	5.01	4.09	3.54	2.89
SEASON TOTAL (one sample)	400980.00	400360.00	400125.00	399943.33
STANDARD DEV. (one sample)	50707.59	49425.77	48678.18	47931.83
COEFF. OF VAR. (one sample)	12.63	12.33	12.15	11.97
SEASON TOTAL (two sample)	398265.00	400033.33	399120.00	399801.67
STANDARD DEV. (csq known)	19956.58	16330.61	14126.57	11544.14
STANDARD DEV. (csq estimated)	20812.14	17097.63	14935.36	12048.07
COEFF. OF VAR. (csq known)	5.02	4.09	3.54	2.89
COEFF. OF VAR. (csq estimated)	5.22	4.27	3.74	3.01
ESTIMATE OF C-SQUARED	.01	.01	.01	0.00

The value given is the average over 100 simulations.

Table 6.15 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	399046.10	399046.10	399046.10	399046.10
TRUE STD DEV	24800.23	21955.32	20384.51	18682.10
TRUE COEFF. OF VARIATION	6.22	5.50	5.11	4.68
SEASON TOTAL (one sample)	398080.00	399860.00	397640.00	397100.00
STANDARD DEV. (one sample)	51571.31	50557.16	49495.03	48805.71
COEFF. OF VAR. (one sample)	12.92	12.62	12.42	12.27
SEASON TOTAL (two sample)	398745.00	399946.67	397607.50	398998.33
STANDARD DEV. (csq known)	24787.13	22017.51	20345.10	18667.23
STANDARD DEV. (csq estimated)	24722.16	21862.19	20171.42	18834.73
COEFF. OF VAR. (csq known)	6.22	5.51	5.12	4.68
COEFF. OF VAR. (csq estimated)	6.16	5.42	5.03	4.69
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

6.100

Table 6.15 continued

SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	399046.10	399046.10	399046.10	399046.10
TRUE STD DEV	28828.05	26420.47	25130.33	23770.28
TRUE COEFF. OF VARIATION	7.22	6.62	6.30	5.95
SEASON TOTAL (one sample)	396190.00	399186.67	395440.00	396826.67
STANDARD DEV. (one sample)	52367.17	51703.94	50329.79	49862.48
COEFF. OF VAR. (one sample)	13.17	12.91	12.69	12.53
SEASON TOTAL (two sample)	398565.00	401143.33	398892.50	398463.33
STANDARD DEV. (csq known)	28827.93	26565.62	25155.47	23725.50
STANDARD DEV. (csq estimated)	28968.34	27245.18	25129.84	23893.84
COEFF. OF VAR. (csq known)	7.23	6.62	6.30	5.95
COEFF. OF VAR. (csq estimated)	7.19	6.74	6.25	5.94
ESTIMATE OF C-SQUARED	.10	.11	.10	.10

The value given is the average over 100 simulations.

Table 6.15 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	399046.10	399046.10	399046.10	399046.10
TRUE STD DEV	42884.35	41304.38	40491.28	39661.52
TRUE COEFF. OF VARIATION	10.74	10.35	10.14	9.93
SEASON TOTAL (one sample)	397340.00	397113.33	394735.00	393103.33
STANDARD DEV. (one sample)	57526.43	55857.08	55042.48	54258.63
COEFF. OF VAR. (one sample)	14.35	13.98	13.81	13.70
SEASON TOTAL (two sample)	401935.00	397653.33	398757.50	397326.67
STANDARD DEV. (csq known)	43245.77	41158.12	40349.64	39580.40
STANDARD DEV. (csq estimated)	43689.76	40915.25	41083.04	40126.77
COEFF. OF VAR. (csq known)	10.72	10.32	10.08	9.94
COEFF. OF VAR. (csq estimated)	10.75	10.15	10.22	9.94
ESTIMATE OF C-SQUARED	.35	.33	.36	.35

The value given is the average over 100 simulations.

Table 6.15 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	399046.10	399046.10	399046.10	399046.10
TRUE STD DEV	50587.54	49255.29	48575.47	47865.99
TRUE COEFF. OF VARIATION	12.67	12.34	12.16	11.99
SEASON TOTAL (one sample)	394230.00	392706.67	401140.00	392313.33
STANDARD DEV. (one sample)	59382.57	58349.86	59564.86	57202.18
COEFF. OF VAR. (one sample)	14.91	14.70	14.70	14.42
SEASON TOTAL (two sample)	398820.00	396826.57	403925.00	398343.33
STANDARD DEV. (csq known)	50148.46	49035.03	49346.95	47970.75
STANDARD DEV. (csq estimated)	51253.68	49317.66	50056.27	47909.08
COEFF. OF VAR. (csq known)	12.51	12.29	12.17	11.98
COEFF. OF VAR. (csq estimated)	12.67	12.19	12.27	11.81
ESTIMATE OF C-SQUARED	.54	.52	.54	.50

The value given is the average over 100 simulations.

Table 6.15 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	399046.10	399046.10	399046.10	399046.10
TRUE STD DEV	68696.09	67721.02	67228.19	66731.71
TRUE COEFF. OF VARIATION	17.20	16.96	16.83	16.71
SEASON TOTAL (one sample)	394660.00	396013.33	391585.00	396790.00
STANDARD DEV. (one sample)	68408.19	67443.54	66574.36	66964.38
COEFF. OF VAR. (one sample)	16.97	16.64	16.64	16.44
SEASON TOTAL (two sample)	396980.00	402566.67	397277.50	401463.33
STANDARD DEV. (csq known)	68393.45	68136.28	67531.19	67316.70
STANDARD DEV. (csq estimated)	68023.30	68671.29	67986.66	67925.40
COEFF. OF VAR. (csq known)	17.05	16.74	16.84	16.57
COEFF. OF VAR. (csq estimated)	16.78	16.73	16.75	16.64
ESTIMATE OF C-SQUARED	1.07	1.10	1.09	1.12

The value given is the average over 100 simulations.

Table 6.16  
Simulation of Sampling for Five Days with Uniform  
Variation of Density About Level of 250 Organisms/1000 m<sup>3</sup>

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	998845.50	998845.50	998845.50	998845.50
TRUE STD DEV	31604.52	25804.98	22347.77	18246.88
TRUE COEFF. OF VARIATION	3.17	2.59	2.24	1.83
SEASON TOTAL (one sample)	998880.00	994953.33	996255.00	998766.67
STANDARD DEV. (one sample)	120553.93	118682.81	118158.88	117700.83
COEFF. OF VAR. (one sample)	12.06	11.92	11.85	11.78
SEASON TOTAL (two sample)	997860.00	996536.67	997540.00	998346.67
STANDARD DEV. (csq known)	31588.92	25775.14	22333.16	18242.32
STANDARD DEV. (csq estimated)	33214.30	27115.35	23450.75	19171.51
COEFF. OF VAR. (csq known)	3.17	2.59	2.24	1.83
COEFF. OF VAR. (csq estimated)	3.32	2.72	2.35	1.92
ESTIMATE OF C-SQUARED	0.00	0.00	0.00	0.00

The value given is the average over 100 simulations.

Table 6.16 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	998845.50	998845.50	998845.50	998845.50
TRUE STD DEV	48481.52	44916.69	43023.65	41043.40
TRUE COEFF. OF VARIATION	4.85	4.50	4.31	4.11
SEASON TOTAL (one sample)	998850.00	1000066.67	1003355.00	999093.33
STANDARD DEV. (one sample)	122862.12	121942.10	121849.59	120436.63
COEFF. OF VAR. (one sample)	12.28	12.17	12.13	12.03
SEASON TOTAL (two sample)	996390.00	997840.00	1000592.50	995990.00
STANDARD DEV. (csq known)	48394.17	44910.20	43176.69	40922.11
STANDARD DEV. (csq estimated)	46630.14	43371.68	41346.81	39628.88
COEFF. OF VAR. (csq known)	4.86	4.50	4.31	4.11
COEFF. OF VAR. (csq estimated)	4.65	4.32	4.11	3.95
ESTIMATE OF C-SQUARED	.04	.05	.04	.05

The value given is the average over 100 simulations.

Table 6.16 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	998845.50	998845.50	998845.50	998845.50
TRUE STD DEV	60844.63	58044.13	56591.93	55101.47
TRUE COEFF. OF VARIATION	6.09	5.81	5.66	5.51
SEASON TOTAL (one sample)	999380.00	1000153.33	998000.00	997276.67
STANDARD DEV. (one sample)	125904.05	124455.08	123164.78	122580.92
COEFF. OF VAR. (one sample)	12.58	12.42	12.31	12.26
SEASON TOTAL (two sample)	994840.00	996506.67	995315.00	994361.67
STANDARD DEV. (csq known)	60683.21	57954.61	56364.64	54795.81
STANDARD DEV. (csq estimated)	58486.32	55597.92	53810.78	52915.81
COEFF. OF VAR. (csq known)	6.10	5.81	5.66	5.50
COEFF. OF VAR. (csq estimated)	5.84	5.54	5.37	5.28
ESTIMATE OF C-SQUARED	.09	.09	.09	.09

The value given is the average over 100 simulations.

Table 6.16 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	998845.50	998845.50	998845.50	998845.50
TRUE STD DEV	100047.50	98369.47	97519.63	96662.32
TRUE COEFF. OF VARIATION	10.01	9.84	9.76	9.67
SEASON TOTAL (one sample)	995950.00	998273.33	993960.00	992436.67
STANDARD DEV. (one sample)	136048.74	136012.81	134142.44	132913.73
COEFF. OF VAR. (one sample)	13.56	13.54	13.43	13.34
SEASON TOTAL (two sample)	991685.00	993736.67	994160.00	990730.00
STANDARD DEV. (csq known)	99629.35	98462.13	97638.23	95923.07
STANDARD DEV. (csq estimated)	95549.54	94064.79	92982.73	91751.25
COEFF. OF VAR. (csq known)	10.01	9.87	9.79	9.65
COEFF. OF VAR. (csq estimated)	9.54	9.39	9.29	9.18
ESTIMATE OF C-SQUARED	.31	.31	.30	.31

The value given is the average over 100 simulations.

Table 6.16 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	998845.50	998845.50	998845.50	998845.50
TRUE STD DEV	120478.07	119088.27	118387.26	117682.06
TRUE COEFF. OF VARIATION	12.06	11.92	11.85	11.78
SEASON TOTAL (one sample)	998810.00	993080.00	994600.00	994450.00
STANDARD DEV. (one sample)	145997.95	143774.07	142595.91	141622.41
COEFF. OF VAR. (one sample)	14.40	14.26	14.19	14.09
SEASON TOTAL (two sample)	994380.00	989196.67	988282.50	992251.67
STANDARD DEV. (csq known)	121314.01	119128.08	118054.22	117679.52
STANDARD DEV. (csq estimated)	115329.74	114273.27	111880.38	112567.26
COEFF. OF VAR. (csq known)	12.11	11.95	11.85	11.78
COEFF. OF VAR. (csq estimated)	11.43	11.41	11.22	11.21
ESTIMATE OF C-SQUARED	.45	.47	.45	.46

The value given is the average over 100 simulations.

Table 6.16 continued

## SIMULATION OF SAMPLING FOR FIVE DAYS - FORTY 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	998845.50	998845.50	998845.50	998845.50
TRUE STD DEV	167424.86	166427.57	165926.68	165424.27
TRUE COEFF. OF VARIATION	16.75	16.65	16.60	16.55
SEASON TOTAL (one sample)	989790.00	994713.33	988310.00	980526.67
STANDARD DEV. (one sample)	163394.55	164043.96	163636.59	159597.23
COEFF. OF VAR. (one sample)	16.18	16.10	16.12	15.94
SEASON TOTAL (two sample)	992135.00	989896.67	982787.50	979835.00
STANDARD DEV. (csq known)	168822.33	168865.63	165399.05	164588.06
STANDARD DEV. (csq estimated)	160229.16	158598.00	158803.14	154121.89
COEFF. OF VAR. (csq known)	16.82	16.73	16.60	16.56
COEFF. OF VAR. (csq estimated)	15.92	15.79	15.80	15.43
ESTIMATE OF C-SQUARED	.95	.92	.95	.90

The value given is the average over 100 simulations.

Table 6.17  
Simulation of Sampling for Three Days with Uniform  
 Variation of Density About Level of 10 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	24250.09	24250.09	24250.09	24250.09
TRUE STD DEV	4924.44	4020.79	3482.10	2843.13
TRUE COEFF. OF VARIATION	20.33	16.60	14.38	11.74
3-DAY TOTAL (one sample)	24450.00	24526.67	24160.00	24073.33
STANDARD DEV. (one sample)	6183.04	5456.09	5015.23	4592.02
COEFF. OF VAR. (one sample)	25.33	22.25	20.75	19.05
3-DAY TOTAL (two sample)	24175.00	23663.33	24200.00	23998.33
STANDARD DEV. (csq known)	4916.81	3971.85	3478.51	2828.33
STANDARD DEV. (csq estimated)	5280.62	4166.80	3635.93	2982.63
COEFF. OF VAR. (csq known)	20.57	16.91	14.45	11.84
COEFF. OF VAR. (csq estimated)	21.97	17.70	15.08	12.45
ESTIMATE OF C-SQUARED	.18	.08	.05	.04

The value given is the average over 100 simulations.

Table 6.17 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	24250.09	24250.09	24250.09	24250.09
TRUE STD DEV	5057.14	4182.26	3667.37	3067.23
TRUE COEFF. OF VARIATION	20.88	17.26	15.14	12.66
3-DAY TOTAL (one sample)	23540.00	24026.67	25245.00	23603.33
STANDARD DEV. (one sample)	6043.18	5421.77	5305.89	4562.89
COEFF. OF VAR. (one sample)	25.86	22.58	20.91	19.27
3-DAY TOTAL (two sample)	23495.00	24126.67	24847.50	23956.67
STANDARD DEV. (csq known)	4974.09	4169.25	3719.78	3044.33
STANDARD DEV. (csq estimated)	5177.35	4316.89	3827.37	3126.23
COEFF. OF VAR. (csq known)	21.37	17.37	15.04	12.74
COEFF. OF VAR. (csq estimated)	22.19	17.93	15.39	13.00
ESTIMATE OF C-SQUARED	.22	.12	.09	.07

The value given is the average over 100 simulations.

Table 6.17 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	24250.09	24250.09	24250.09	24250.09
TRUE STD DEV	5186.45	4337.72	3843.72	3276.05
TRUE COEFF. OF VARIATION	21.41	17.90	15.86	13.52
3-DAY TOTAL (one sample)	24350.00	24520.00	24395.00	24446.67
STANDARD DEV. (one sample)	6246.60	5628.10	5171.32	4828.62
COEFF. OF VAR. (one sample)	25.73	22.96	21.15	19.66
3-DAY TOTAL (two sample)	24185.00	24726.67	24155.00	24505.00
STANDARD DEV. (csq known)	5178.00	4385.93	3834.41	3302.72
STANDARD DEV. (csq estimated)	5399.54	4544.10	3906.72	3306.48
COEFF. OF VAR. (csq known)	21.60	17.81	15.96	13.50
COEFF. OF VAR. (csq estimated)	22.37	18.37	16.13	13.45
ESTIMATE OF C-SQUARED	.24	.18	.14	.11

The value given is the average over 100 simulations.

Table 6.17 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	24250.09	24250.09	24250.09	24250.09
TRUE STD DEV	5751.56	4999.71	4577.71	4112.63
TRUE COEFF. OF VARIATION	23.73	20.63	18.88	16.96
3-DAY TOTAL (one sample)	24650.00	24220.00	23590.00	24340.00
STANDARD DEV. (one sample)	6581.79	5801.92	5316.01	5102.72
COEFF. OF VAR. (one sample)	26.61	23.91	22.49	20.70
3-DAY TOTAL (two sample)	25060.00	24293.33	23902.50	24256.67
STANDARD DEV. (csq known)	5860.39	4996.25	4518.54	4103.55
STANDARD DEV. (csq estimated)	5914.39	5024.38	4533.62	4164.13
COEFF. OF VAR. (csq known)	23.53	20.61	18.91	16.90
COEFF. OF VAR. (csq estimated)	23.51	20.58	18.79	16.88
ESTIMATE OF C-SQUARED	.53	.41	.39	.38

The value given is the average over 100 simulations.

Table 6.17 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	24250.09	24250.09	24250.09	24250.09
TRUE STD DEV	6123.38	5423.32	5036.93	4618.33
TRUE COEFF. OF VARIATION	25.26	22.37	20.77	19.04
3-DAY TOTAL (one sample)	22860.00	23400.00	23845.00	24723.33
STANDARD DEV. (one sample)	6349.02	5881.42	5561.92	5380.93
COEFF. OF VAR. (one sample)	27.86	24.84	23.16	21.52
3-DAY TOTAL (two sample)	23520.00	24383.33	24090.00	24370.00
STANDARD DEV. (csq known)	5971.60	5485.54	5008.99	4634.99
STANDARD DEV. (csq estimated)	5984.98	5483.11	4881.60	4716.17
COEFF. OF VAR. (csq known)	25.56	22.46	20.76	18.94
COEFF. OF VAR. (csq estimated)	25.22	22.25	20.07	18.92
ESTIMATE OF C-SQUARED	.62	.59	.50	.57

The value given is the average over 100 simulations.

Table 6.17 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 10.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	24250.09	24250.09	24250.09	24250.09
TRUE STD DEV	7123.30	6531.31	6214.21	5880.02
TRUE COEFF. OF VARIATION	29.38	26.93	25.62	24.23
3-DAY TOTAL (one sample)	24270.00	24293.33	24750.00	23923.33
STANDARD DEV. (one sample)	7107.04	6741.58	6296.63	5930.43
COEFF. OF VAR. (one sample)	29.13	26.72	25.05	24.07
3-DAY TOTAL (two sample)	24485.00	24296.67	24552.50	24005.00
STANDARD DEV. (csq known)	7213.76	6619.56	6223.31	5880.81
STANDARD DEV. (csq estimated)	6938.66	6511.02	6190.01	5880.05
COEFF. OF VAR. (csq known)	29.29	26.90	25.15	24.06
COEFF. OF VAR. (csq estimated)	28.00	25.97	24.56	23.72
ESTIMATE OF C-SQUARED	1.20	1.22	1.22	1.13

The value given is the average over 100 simulations.

Table 6.18  
Simulation of Sampling for Three Days with Uniform  
 Variation of Density About Level of 50 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	119787.21	119787.21	119787.21	119787.21
TRUE STD DEV	10944.73	8936.34	7739.10	6318.95
TRUE COEFF. OF VARIATION	9.15	7.47	6.47	5.28
3-DAY TOTAL (one sample)	119450.00	120186.67	119970.00	119326.67
STANDARD DEV. (one sample)	20995.00	20101.41	19564.76	19036.92
COEFF. OF VAR. (one sample)	17.51	16.69	16.27	15.92
3-DAY TOTAL (two sample)	119205.00	120150.00	119915.00	119408.33
STANDARD DEV. (csq known)	10918.10	8949.86	7743.22	6308.94
STANDARD DEV. (csq estimated)	11619.17	9344.16	8210.36	6691.99
COEFF. OF VAR. (csq known)	9.19	7.47	6.47	5.29
COEFF. OF VAR. (csq estimated)	9.77	7.78	6.84	5.59
ESTIMATE OF C-SQUARED	.03	.01	.01	.01

The value given is the average over 100 simulations.

Table 6.18 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	119787.21	119787.21	119787.21	119787.21
TRUE STD DEV	12336.30	10595.06	9606.81	8504.48
TRUE COEFF. OF VARIATION	10.31	8.85	8.02	7.10
3-DAY TOTAL (one sample)	119890.00	117880.00	118180.00	118030.00
STANDARD DEV. (one sample)	21469.51	20168.29	19738.48	19255.85
COEFF. OF VAR. (one sample)	17.86	17.04	16.63	16.23
3-DAY TOTAL (two sample)	119905.00	118680.00	118345.00	119701.67
STANDARD DEV. (csq known)	12334.79	10525.98	9530.59	8508.33
STANDARD DEV. (csq estimated)	12569.21	10688.52	9218.87	8450.74
COEFF. OF VAR. (csq known)	10.31	8.88	8.06	7.11
COEFF. OF VAR. (csq estimated)	10.43	8.94	7.71	6.98
ESTIMATE OF C-SQUARED	.06	.06	.04	.05

The value given is the average over 100 simulations.

Table 6.18 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	119787.21	119787.21	119787.21	119787.21
TRUE STD DEV	13586.08	12027.15	11166.37	10233.44
TRUE COEFF. OF VARIATION	11.35	10.04	9.32	8.54
3-DAY TOTAL (one sample)	118170.00	119606.67	119480.00	119036.67
STANDARD DEV. (one sample)	21443.65	20704.70	20345.88	19803.28
COEFF. OF VAR. (one sample)	18.08	17.23	16.95	16.57
3-DAY TOTAL (two sample)	119500.00	119076.67	119340.00	120091.67
STANDARD DEV. (csq known)	13534.70	11938.84	11147.16	10257.38
STANDARD DEV. (csq estimated)	13236.69	12187.15	10993.86	10065.48
COEFF. OF VAR. (csq known)	11.34	10.03	9.33	8.53
COEFF. OF VAR. (csq estimated)	11.02	10.14	9.10	8.29
ESTIMATE OF C-SQUARED	.09	.11	.10	.10

The value given is the average over 100 simulations.

Table 6.18 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	119787.21	119787.21	119787.21	119787.21
TRUE STD DEV	18323.92	17199.91	16609.41	15997.12
TRUE COEFF. OF VARIATION	15.29	14.35	13.86	13.34
3-DAY TOTAL (one sample)	122900.00	118073.33	118565.00	117036.67
STANDARD DEV. (one sample)	23921.12	22126.71	21854.86	21196.86
COEFF. OF VAR. (one sample)	19.17	18.51	18.26	17.86
3-DAY TOTAL (two sample)	121670.00	118830.00	120130.00	118345.00
STANDARD DEV. (csq known)	18445.58	17032.21	16611.88	15748.53
STANDARD DEV. (csq estimated)	18353.47	16622.27	16612.65	15993.21
COEFF. OF VAR. (csq known)	15.11	14.26	13.76	13.26
COEFF. OF VAR. (csq estimated)	14.73	13.73	13.55	13.28
ESTIMATE OF C-SQUARED	.34	.32	.34	.36

The value given is the average over 100 simulations.

Table 6.18 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	119787.21	119787.21	119787.21	119787.21
TRUE STD DEV	21065.58	20095.51	19592.47	19076.18
TRUE COEFF. OF VARIATION	17.58	16.76	16.34	15.91
3-DAY TOTAL (one sample)	115040.00	119726.67	117795.00	120586.67
STANDARD DEV. (one sample)	23700.21	24003.43	23095.94	23002.99
COEFF. OF VAR. (one sample)	20.13	19.73	19.37	18.71
3-DAY TOTAL (two sample)	117680.00	120940.00	118535.00	119206.67
STANDARD DEV. (csq known)	20789.18	20366.60	19421.03	18965.08
STANDARD DEV. (csq estimated)	20716.42	19843.39	19412.05	18968.31
COEFF. OF VAR. (csq known)	17.54	16.69	16.28	15.79
COEFF. OF VAR. (csq estimated)	17.04	16.11	16.06	15.44
ESTIMATE OF C-SQUARED	.51	.48	.52	.51

The value given is the average over 100 simulations.

Table 6.18 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms= 50.0/ 1000 cubic meters      Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	119787.21	119787.21	119787.21	119787.21
TRUE STD DEV	27707.94	26977.79	26605.20	26227.31
TRUE COEFF. OF VARIATION	23.11	22.50	22.19	21.87
3-DAY TOTAL (one sample)	119120.00	121433.33	119550.00	117140.00
STANDARD DEV. (one sample)	27885.66	27356.98	26241.00	26205.66
COEFF. OF VAR. (one sample)	22.76	21.60	21.34	21.58
3-DAY TOTAL (two sample)	120865.00	121396.67	120287.50	119046.67
STANDARD DEV. (csq known)	28161.59	26974.71	26604.74	26281.36
STANDARD DEV. (csq estimated)	27329.01	27149.18	26601.01	26446.02
COEFF. OF VAR. (csq known)	22.90	21.88	21.74	21.66
COEFF. OF VAR. (csq estimated)	22.19	21.54	21.27	21.35
ESTIMATE OF C-SQUARED	1.06	1.14	1.08	1.15

The value given is the average over 100 simulations.

Table 6.19  
Simulation of Sampling for Three Days with Uniform  
 Variation of Density About Level of 100 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	237817.32	237817.32	237817.32	237817.32
TRUE STD DEV	15421.33	12591.46	10904.52	8903.51
TRUE COEFF. OF VARIATION	6.49	5.30	4.59	3.75
3-DAY TOTAL (one sample)	238700.00	238306.67	237505.00	239013.33
STANDARD DEV. (one sample)	39144.73	37966.59	37301.71	36940.33
COEFF. OF VAR. (one sample)	16.36	15.90	15.67	15.42
3-DAY TOTAL (two sample)	238870.00	237283.33	238335.00	238801.67
STANDARD DEV. (csq known)	15455.42	12577.32	10916.39	8921.91
STANDARD DEV. (csq estimated)	16229.91	13276.13	11317.57	9484.70
COEFF. OF VAR. (csq known)	6.48	5.31	4.59	3.74
COEFF. OF VAR. (csq estimated)	6.79	5.59	4.75	3.97
ESTIMATE OF C-SQUARED	.01	.01	0.00	0.00

The value given is the average over 100 simulations.

Table 6.19 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	237817.32	237817.32	237817.32	237817.32
TRUE STD DEV	19114.48	16914.22	15698.88	14381.19
TRUE COEFF. OF VARIATION	8.04	7.11	6.60	6.05
3-DAY TOTAL (one sample)	238920.00	236306.67	236350.00	235423.33
STANDARD DEV. (one sample)	39910.40	38372.56	37914.01	37279.98
COEFF. OF VAR. (one sample)	16.65	16.18	15.99	15.78
3-DAY TOTAL (two sample)	239170.00	237363.33	237247.50	236383.33
STANDARD DEV. (csq known)	19188.67	16891.86	15658.96	14323.12
STANDARD DEV. (csq estimated)	19098.95	16935.63	15660.98	14160.04
COEFF. OF VAR. (csq known)	8.03	7.12	6.60	6.06
COEFF. OF VAR. (csq estimated)	7.95	7.02	6.54	5.93
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.19 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	237817.32	237817.32	237817.32	237817.32
TRUE STD DEV	22201.57	20338.07	19339.10	18285.64
TRUE COEFF. OF VARIATION	9.34	8.55	8.13	7.68
3-DAY TOTAL (one sample)	234690.00	235640.00	239190.00	237256.67
STANDARD DEV. (one sample)	40066.20	39099.47	39416.30	38221.75
COEFF. OF VAR. (one sample)	16.98	16.53	16.39	16.05
3-DAY TOTAL (two sample)	238830.00	236750.00	237347.50	238390.00
STANDARD DEV. (csq known)	22293.61	20237.30	19319.37	18334.57
STANDARD DEV. (csq estimated)	22000.68	19939.30	19597.03	17584.15
COEFF. OF VAR. (csq known)	9.33	8.54	8.13	7.68
COEFF. OF VAR. (csq estimated)	9.11	8.33	8.16	7.30
ESTIMATE OF C-SQUARED	.10	.10	.11	.09

The value given is the average over 100 simulations.

Table 6.19 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	237817.32	237817.32	237817.32	237817.32
TRUE STD DEV	32986.60	31762.30	31132.10	30488.87
TRUE COEFF. OF VARIATION	13.86	13.34	13.08	12.81
3-DAY TOTAL (one sample)	240170.00	238833.33	233550.00	236406.67
STANDARD DEV. (one sample)	44616.59	42860.29	41740.09	42046.63
COEFF. OF VAR. (one sample)	18.33	17.78	17.65	17.61
3-DAY TOTAL (two sample)	239305.00	239840.00	236192.50	237298.33
STANDARD DEV. (csq known)	33138.74	31828.10	30840.46	30363.18
STANDARD DEV. (csq estimated)	32647.43	31129.05	30764.94	30931.12
COEFF. OF VAR. (csq known)	13.78	13.22	12.97	12.73
COEFF. OF VAR. (csq estimated)	13.46	12.78	12.73	12.74
ESTIMATE OF C-SQUARED	.33	.32	.34	.36

The value given is the average over 100 simulations.

Table 6.19 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	237817.32	237817.32	237817.32	237817.32
TRUE STD DEV	38900.88	37868.27	37341.26	36806.70
TRUE COEFF. OF VARIATION	16.34	15.91	15.69	15.46
3-DAY TOTAL (one sample)	236470.00	229253.33	236205.00	235706.67
STANDARD DEV. (one sample)	45910.89	43980.65	44735.50	44102.42
COEFF. OF VAR. (one sample)	19.18	18.83	18.65	18.45
3-DAY TOTAL (two sample)	236900.00	236076.67	239562.50	236533.33
STANDARD DEV. (csq known)	38675.06	37850.24	37590.29	36487.64
STANDARD DEV. (csq estimated)	38683.85	37549.64	37161.32	36885.23
COEFF. OF VAR. (csq known)	16.24	15.91	15.57	15.29
COEFF. OF VAR. (csq estimated)	15.98	15.50	15.22	15.22
ESTIMATE OF C-SQUARED	.52	.51	.50	.54

The value given is the average over 100 simulations.

Table 6.19 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=100.0/ 1000 cubic meters Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	237817.32	237817.32	237817.32	237817.32
TRUE STD DEV	52808.51	52052.54	51670.40	51285.42
TRUE COEFF. OF VARIATION	22.18	21.86	21.70	21.54
3-DAY TOTAL (one sample)	234510.00	239513.33	241425.00	240930.00
STANDARD DEV. (one sample)	51853.93	52402.16	52746.16	51828.46
COEFF. OF VAR. (one sample)	21.53	21.29	20.96	20.88
3-DAY TOTAL (two sample)	233075.00	241120.00	241807.50	238586.67
STANDARD DEV. (csq known)	52047.69	52738.01	52132.18	51339.86
STANDARD DEV. (csq estimated)	50657.63	49353.33	52609.23	50460.35
COEFF. OF VAR. (csq known)	21.94	21.54	21.27	21.16
COEFF. OF VAR. (csq estimated)	21.18	20.06	21.07	20.51
ESTIMATE OF C-SQUARED	1.07	.93	1.21	1.04

The value given is the average over 100 simulations.

Table 6.20  
Simulation of Sampling for Three Days with Uniform  
Variation of Density About Level of 250 Organisms/1000 m<sup>3</sup>

SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	600996.93	600996.93	600996.93	600996.93
TRUE STD DEV	24515.24	20016.61	17334.89	14153.88
TRUE COEFF. OF VARIATION	4.08	3.33	2.89	2.36
3-DAY TOTAL (one sample)	603570.00	599620.00	602670.00	601476.67
STANDARD DEV. (one sample)	93792.43	92197.32	92144.82	91363.26
COEFF. OF VAR. (one sample)	15.52	15.35	15.27	15.17
3-DAY TOTAL (two sample)	601285.00	600583.33	601782.50	602045.00
STANDARD DEV. (csq known)	24521.11	20009.72	17346.22	14166.22
STANDARD DEV. (csq estimated)	26004.90	21119.50	18212.46	15050.05
COEFF. OF VAR. (csq known)	4.08	3.34	2.89	2.36
COEFF. OF VAR. (csq estimated)	4.32	3.51	3.02	2.50
ESTIMATE OF C-SQUARED	0.00	0.00	0.00	0.00

The value given is the average over 100 simulations.

Table 6.20 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	600996.93	600996.93	600996.93	600996.93
TRUE STD DEV	37625.45	34861.76	33394.26	31859.22
TRUE COEFF. OF VARIATION	6.26	5.80	5.55	5.30
3-DAY TOTAL (one sample)	597310.00	605913.33	603725.00	601290.00
STANDARD DEV. (one sample)	95105.84	95375.52	94561.00	93433.02
COEFF. OF VAR. (one sample)	15.87	15.70	15.62	15.51
3-DAY TOTAL (two sample)	598950.00	602456.67	606362.50	600691.67
STANDARD DEV. (csq known)	37558.13	34922.16	33669.61	31838.99
STANDARD DEV. (csq estimated)	36725.06	34881.48	33295.78	30840.48
COEFF. OF VAR. (csq known)	6.27	5.79	5.55	5.29
COEFF. OF VAR. (csq estimated)	6.05	5.74	5.40	5.07
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.20 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	600996.93	600996.93	600996.93	600996.93
TRUE STD DEV	47226.61	45055.75	43930.11	42774.85
TRUE COEFF. OF VARIATION	7.85	7.49	7.30	7.11
3-DAY TOTAL (one sample)	606290.00	600273.33	603620.00	607360.00
STANDARD DEV. (one sample)	98905.21	96562.45	97057.14	96247.87
COEFF. OF VAR. (one sample)	16.22	16.02	16.01	15.79
3-DAY TOTAL (two sample)	601525.00	598083.33	598280.00	602650.00
STANDARD DEV. (csq known)	47273.61	44748.25	43815.10	42804.38
STANDARD DEV. (csq estimated)	47575.36	44845.83	44770.05	42105.17
COEFF. OF VAR. (csq known)	7.85	7.47	7.31	7.09
COEFF. OF VAR. (csq estimated)	7.79	7.39	7.35	6.89
ESTIMATE OF C-SQUARED	.10	.10	.11	.10

The value given is the average over 100 simulations.

Table 6.20 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	600996.93	600996.93	600996.93	600996.93
TRUE STD DEV	77666.78	76366.20	75707.53	75043.08
TRUE COEFF. OF VARIATION	12.91	12.70	12.59	12.47
3-DAY TOTAL (one sample)	602650.00	592126.67	610690.00	611403.33
STANDARD DEV. (one sample)	107687.51	105019.57	107670.98	106413.98
COEFF. OF VAR. (one sample)	17.68	17.49	17.45	17.22
3-DAY TOTAL (two sample)	594755.00	598690.00	605035.00	608488.33
STANDARD DEV. (csq known)	76831.68	76088.91	76202.96	75783.38
STANDARD DEV. (csq estimated)	77406.30	75427.71	75930.69	75572.13
COEFF. OF VAR. (csq known)	12.85	12.64	12.52	12.39
COEFF. OF VAR. (csq estimated)	12.77	12.37	12.33	12.10
ESTIMATE OF C-SQUARED	.35	.33	.34	.33

The value given is the average over 100 simulations.

Table 6.20 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters      Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	600996.93	600996.93	600996.93	600996.93
TRUE STD DEV	93529.55	92452.39	91909.07	91362.53
TRUE COEFF. OF VARIATION	15.55	15.37	15.28	15.19
3-DAY TOTAL (one sample)	601830.00	604866.67	598755.00	597956.67
STANDARD DEV. (one sample)	112318.70	112681.50	111790.33	110571.18
Coeff. OF VAR. (one sample)	18.40	18.27	18.33	18.23
3-DAY TOTAL (two sample)	603970.00	596753.33	599592.50	589971.67
STANDARD DEV. (csq known)	93962.89	91711.32	91603.50	89230.00
STANDARD DEV. (csq estimated)	92454.64	90996.46	92236.67	91400.54
Coeff. OF VAR. (csq known)	15.42	15.25	15.13	15.00
Coeff. OF VAR. (csq estimated)	14.92	14.86	15.04	15.05
ESTIMATE OF C-SQUARED	.50	.51	.53	.54

The value given is the average over 100 simulations.

Table 6.20 (continued)

## SIMULATION OF SAMPLING FOR 3 DAYS - TWENTY-FOUR 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to uniform perturbations.

Mean Density of Organisms=250.0/ 1000 cubic meters Value of c-squared=1.0

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE 3-DAY TOTAL	600996.93	600996.93	600996.93	600996.93
TRUE STD DEV	129979.06	129206.12	128817.92	128428.54
TRUE COEFF. OF VARIATION	21.61	21.48	21.41	21.35
3-DAY TOTAL (one sample)	632900.00	603733.33	596575.00	602816.67
STANDARD DEV. (one sample)	139546.26	128629.15	127829.57	128426.55
COEFF. OF VAR. (one sample)	21.25	20.63	20.82	20.68
3-DAY TOTAL (two sample)	605405.00	592306.67	605167.50	601176.67
STANDARD DEV. (csq known)	133210.42	126399.54	129707.87	128029.47
STANDARD DEV. (csq estimated)	132288.45	125631.05	130780.88	126248.32
COEFF. OF VAR. (csq known)	21.56	20.96	21.10	21.01
COEFF. OF VAR. (csq estimated)	21.08	20.50	20.90	20.35
ESTIMATE OF C-SQUARED	1.07	1.02	1.10	1.06

The value given is the average over 100 simulations.

Table 6.21Simulation of 18-Day Entrainment Season

SIMULATION OF SAMPLING FOR 18 DAYS - 144 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/8, 50/4, 100/4, 250/2

Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	944854.79	944854.79	944854.79	944854.79
TRUE STD DEV	30738.49	25097.87	21735.39	17746.88
TRUE COEFF. OF VARIATION	3.25	2.66	2.30	1.88
SEASON TOTAL (one sample)	951750.00	945973.33	951365.00	943323.33
STANDARD DEV. (one sample)	93390.15	91299.63	90769.90	89032.07
COEFF. OF VAR. (one sample)	9.80	9.64	9.53	9.42
SEASON TOTAL (two sample)	948960.00	945903.33	949760.00	943813.33
STANDARD DEV. (csq known)	30805.19	25111.79	21791.74	17737.09
STANDARD DEV. (csq estimated)	31810.22	26221.75	22482.49	18325.67
COEFF. OF VAR. (csq known)	3.25	2.66	2.30	1.88
COEFF. OF VAR. (csq estimated)	3.35	2.77	2.37	1.94
ESTIMATE OF C-SQUARED	0.00	0.00	0.00	0.00

The value given is the average over 100 simulations.

Table 6.21 continued

## SIMULATION OF SAMPLING FOR 18 DAYS - 144 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/8, 50/4, 100/4, 250/2

Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	944854.79	944854.79	944854.79	944854.79
TRUE STD DEV	41351.35	37349.46	35178.21	32863.82
TRUE COEFF. OF VARIATION	4.38	3.95	3.72	3.48
SEASON TOTAL (one sample)	945750.00	942746.67	938410.00	945523.33
STANDARD DEV. (one sample)	94962.62	93142.42	91792.73	91675.79
COEFF. OF VAR. (one sample)	10.01	9.85	9.76	9.66
SEASON TOTAL (two sample)	941815.00	944053.33	942245.00	945190.00
STANDARD DEV. (csq known)	41224.74	37305.71	35112.45	32893.47
STANDARD DEV. (csq estimated)	40981.40	37220.66	34456.39	32927.34
COEFF. OF VAR. (csq known)	4.38	3.95	3.72	3.47
COEFF. OF VAR. (csq estimated)	4.32	3.92	3.63	3.45
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.21 continued

SIMULATION OF SAMPLING FOR 18 DAYS - 144 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/8, 50/4, 100/4, 250/2

Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	944854.79	944854.79	944854.79	944854.79
TRUE STD DEV	49749.50	46476.46	44750.26	42954.74
TRUE COEFF. OF VARIATION	5.26	4.92	4.73	4.54
SEASON TOTAL (one sample)	941090.00	934826.67	942385.00	945956.67
STANDARD DEV. (one sample)	97170.73	93558.99	94106.47	93988.62
COEFF. OF VAR. (one sample)	10.27	9.96	9.95	9.87
SEASON TOTAL (two sample)	942575.00	942776.67	940532.50	946643.33
STANDARD DEV. (csq known)	49719.28	46414.12	44497.18	43155.30
STANDARD DEV. (csq estimated)	49303.60	45206.61	44616.21	42326.99
COEFF. OF VAR. (csq known)	5.27	4.92	4.72	4.54
COEFF. OF VAR. (csq estimated)	5.19	4.76	4.69	4.43
ESTIMATE OF C-SQUARED	.10	.09	.10	.10

The value given is the average over 100 simulations.

Table 6.21 continued

## SIMULATION OF SAMPLING FOR 18 DAYS - 144 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/8, 50/4, 100/4, 250/2

Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	944854.79	944854.79	944854.79	944854.79
TRUE STD DEV	77751.73	75699.27	74651.88	73589.59
TRUE COEFF. OF VARIATION	8.22	8.00	7.89	7.78
SEASON TOTAL (one sample)	928690.00	938673.33	941915.00	931543.33
STANDARD DEV. (one sample)	103544.82	102558.36	103980.38	100218.82
COEFF. OF VAR. (one sample)	11.01	10.81	10.92	10.67
SEASON TOTAL (two sample)	935840.00	941813.33	942077.50	941593.33
STANDARD DEV. (csq known)	76929.06	75202.85	74257.39	73145.30
STANDARD DEV. (csq estimated)	75447.60	75173.16	74379.58	71156.29
COEFF. OF VAR. (csq known)	8.18	7.94	7.84	7.73
COEFF. OF VAR. (csq estimated)	7.94	7.86	7.78	7.47
ESTIMATE OF C-SQUARED	.32	.34	.34	.32

The value given is the average over 100 simulations.

Table 6.21 continued

## SIMULATION OF SAMPLING FOR 18 DAYS - 144 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/8, 50/4, 100/4, 250/2

Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	944854.79	944854.79	944854.79	944854.79
TRUE STD DEV	92712.70	90998.32	90128.90	89251.01
TRUE COEFF. OF VARIATION	9.80	9.62	9.53	9.43
SEASON TOTAL (one sample)	934450.00	941373.33	929490.00	936900.00
STANDARD DEV. (one sample)	109867.51	109574.10	108382.12	106943.07
COEFF. OF VAR. (one sample)	11.57	11.43	11.44	11.24
SEASON TOTAL (two sample)	934475.00	944113.33	937785.00	941418.33
STANDARD DEV. (csq known)	91139.72	90785.03	89754.52	88792.01
STANDARD DEV. (csq estimated)	91972.20	89923.37	90028.54	88373.30
COEFF. OF VAR. (csq known)	9.70	9.54	9.50	9.37
COEFF. OF VAR. (csq estimated)	9.70	9.38	9.45	9.22
ESTIMATE OF C-SQUARED	.53	.51	.52	.51

The value given is the average over 100 simulations.

Table 6.21 continued

## SIMULATION OF SAMPLING FOR 18 DAYS - 144 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/8, 50/4, 100/4, 250/2

Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	944854.79	944854.79	944854.79	944854.79
TRUE STD DEV	127461.51	126219.98	125594.62	124966.13
TRUE COEFF. OF VARIATION	13.47	13.34	13.28	13.21
SEASON TOTAL (one sample)	958880.00	922193.33	918240.00	942323.33
STANDARD DEV. (one sample)	133072.31	122166.15	120520.75	124311.24
COEFF. OF VAR. (one sample)	13.48	12.92	12.79	12.84
SEASON TOTAL (two sample)	948935.00	938376.67	929012.50	944265.00
STANDARD DEV. (csq known)	128896.08	125723.52	121647.51	124982.25
STANDARD DEV. (csq estimated)	128190.27	123248.86	123324.43	125472.73
COEFF. OF VAR. (csq known)	13.40	13.20	12.97	13.07
COEFF. OF VAR. (csq estimated)	13.18	12.89	12.97	13.05
ESTIMATE OF C-SQUARED	1.05	1.02	1.12	1.08

The value given is the average over 100 simulations.

Table 6.22

Simulation of 34-Day Entrainment Season

SIMULATION OF SAMPLING FOR 34 DAYS - 272 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/10, 50/10, 100/8, 250/6

Value of c-squared=0.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	2320279.09	2320279.09	2320279.09	2320279.09
TRUE STD DEV	48169.28	39330.05	34060.82	27810.54
TRUE COEFF. OF VARIATION	2.08	1.70	1.47	1.20
SEASON TOTAL (one sample)	2327440.00	2321626.67	2321045.00	2321400.00
STANDARD DEV. (one sample)	153064.50	150103.22	148552.18	147357.17
COEFF. OF VAR. (one sample)	6.57	6.46	6.40	6.34
SEASON TOTAL (two sample)	2321970.00	2320270.00	2321660.00	2319085.00
STANDARD DEV. (csq known)	48186.82	39329.97	34070.96	27803.39
STANDARD DEV. (csq estimated)	49089.71	40292.51	35034.27	28575.48
COEFF. OF VAR. (csq known)	2.08	1.70	1.47	1.20
COEFF. OF VAR. (csq estimated)	2.11	1.74	1.51	1.23
ESTIMATE OF C-SQUARED	0.00	0.00	0.00	0.00

The value given is the average over 100 simulations.

Table 6.22 continued

## SIMULATION OF SAMPLING FOR 34 DAYS - 272 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/10, 50/10, 100/8, 250/6

Value of c-squared= .05

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	2320279.09	2320279.09	2320279.09	2320279.09
TRUE STD DEV	66430.77	60329.27	57034.26	53536.85
TRUE COEFF. OF VARIATION	2.86	2.60	2.46	2.31
SEASON TOTAL (one sample)	2328610.00	2317713.33	2321615.00	2321783.33
STANDARD DEV. (one sample)	156616.70	152930.94	152081.34	151042.60
COEFF. OF VAR. (one sample)	6.72	6.59	6.54	6.50
SEASON TOTAL (two sample)	2325270.00	2320403.33	2318430.00	2323265.00
STANDARD DEV. (csq known)	66568.43	60323.10	56971.62	53668.29
STANDARD DEV. (csq estimated)	66503.38	60510.11	57110.68	53562.62
COEFF. OF VAR. (csq known)	2.86	2.60	2.46	2.31
COEFF. OF VAR. (csq estimated)	2.85	2.60	2.45	2.29
ESTIMATE OF C-SQUARED	.05	.05	.05	.05

The value given is the average over 100 simulations.

Table 6.22 continued

## SIMULATION OF SAMPLING FOR 34 DAYS - 272 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/10, 50/10, 100/8, 250/6

Value of c-squared= .10

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	2320279.09	2320279.09	2320279.09	2320279.09
TRUE STD DEV	80658.63	75712.54	73114.12	70419.90
TRUE COEFF. OF VARIATION	3.48	3.26	3.15	3.03
SEASON TOTAL (one sample)	2324740.00	2323893.33	2324500.00	2321886.67
STANDARD DEV. (one sample)	159450.56	156837.44	155689.61	154270.06
COEFF. OF VAR. (one sample)	6.85	6.74	6.68	6.63
SEASON TOTAL (two sample)	2324535.00	2325800.00	2325390.00	2324370.00
STANDARD DEV. (csq known)	80787.37	75900.29	73341.46	70539.91
STANDARD DEV. (csq estimated)	81447.74	76039.73	73781.47	71570.18
COEFF. OF VAR. (csq known)	3.47	3.26	3.15	3.03
COEFF. OF VAR. (csq estimated)	3.49	3.26	3.16	3.07
ESTIMATE OF C-SQUARED	.10	.10	.10	.10

The value given is the average over 100 simulations.

Table 6.22 continued

## SIMULATION OF SAMPLING FOR 34 DAYS - 272 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/10, 50/10, 100/8, 250/6

Value of c-squared= .33

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	2320279.09	2320279.09	2320279.09	2320279.09
TRUE STD DEV	127561.46	124492.97	122930.00	121346.91
TRUE COEFF. OF VARIATION	5.50	5.36	5.30	5.23
SEASON TOTAL (one sample)	2325820.00	2328093.33	2325510.00	2328600.00
STANDARD DEV. (one sample)	174018.33	172738.96	170295.90	171296.33
COEFF. OF VAR. (one sample)	7.45	7.38	7.29	7.33
SEASON TOTAL (two sample)	2329760.00	2323266.67	2329982.50	2327271.67
STANDARD DEV. (csq known)	128288.78	124743.11	123358.58	122228.59
STANDARD DEV. (csq estimated)	130150.80	125344.49	124362.21	123913.60
COEFF. OF VAR. (csq known)	5.49	5.35	5.28	5.24
COEFF. OF VAR. (csq estimated)	5.55	5.36	5.29	5.27
ESTIMATE OF C-SQUARED	.35	.34	.34	.35

The value given is the average over 100 simulations.

Table 6.22 continued

## SIMULATION OF SAMPLING FOR 34 DAYS - 272 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/10, 50/10, 100/8, 250/6

Value of c-squared= .50

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	2320279.09	2320279.09	2320279.09	2320279.09
TRUE STD DEV	152472.81	149915.08	148619.71	147312.95
TRUE COEFF. OF VARIATION	6.57	6.46	6.40	6.35
SEASON TOTAL (one sample)	2334760.00	2335673.33	2338165.00	2347490.00
STANDARD DEV. (one sample)	186216.57	183610.51	182332.15	182491.59
COEFF. OF VAR. (one sample)	7.92	7.82	7.74	7.72
SEASON TOTAL (two sample)	2332955.00	2328290.00	2333447.50	2340266.67
STANDARD DEV. (csq known)	153690.54	150349.59	149516.37	149075.54
STANDARD DEV. (csq estimated)	154240.45	149647.15	151143.75	149851.30
COEFF. OF VAR. (csq known)	6.56	6.44	6.39	6.35
COEFF. OF VAR. (csq estimated)	6.54	6.40	6.42	6.36
ESTIMATE OF C-SQUARED	.51	.50	.52	.51

The value given is the average over 100 simulations.

Table 6.22 continued

## SIMULATION OF SAMPLING FOR 34 DAYS - 272 3-HOUR PERIODS

Channel volume=100,000 cubic meters per 3-hour period.

Mean density subjected to random perturbations.

Mean Density(per 1000 cubic meters)/No. of Days: 10/10, 50/10, 100/8, 250/6

Value of c-squared=1.00

	Volume of Sample in cubic meters			
	100	150	200	300
TRUE SEASON TOTAL	2320279.09	2320279.09	2320279.09	2320279.09
TRUE STD DEV	210180.01	208331.97	207401.77	206467.39
TRUE COEFF. OF VARIATION	9.05	8.97	8.93	8.89
SEASON TOTAL (one sample)	2342240.00	2357720.00	2318155.00	2318063.33
STANDARD DEV. (one sample)	212585.96	213809.20	205037.91	204160.27
COEFF. OF VAR. (one sample)	8.96	8.93	8.71	8.71
SEASON TOTAL (two sample)	2344975.00	2350770.00	2332317.50	2340256.67
STANDARD DEV. (csq known)	213940.30	212537.74	208649.70	208090.62
STANDARD DEV. (csq estimated)	214003.98	214355.56	210062.12	206664.11
COEFF. OF VAR. (csq known)	9.07	8.98	8.89	8.84
COEFF. OF VAR. (csq estimated)	9.05	9.05	8.91	8.74
ESTIMATE OF C-SQUARED	1.05	1.07	1.06	1.01

The value given is the average over 100 simulations.

resulting estimate of  $c^2$  can be used effectively in achieving our goal of entrainment estimation. Note that although the 56, 40, or 24 paired observations were viewed here as data from 7, 5, or 3 days, they could in practice be scattered over an entrainment sampling season of several months.

In Tables 6.9 to 6.12, 6.13 to 6.16, or 6.17 to 6.20, factors that affect the accuracy of estimation of  $c^2$  include the mean organism density, the value of  $c^2$ , and the pattern of daily variation (diel or uniform). Low organism density, high values of  $c^2$ , and diel variation increase the variability of  $\hat{c}^2$ .

Thus, although the estimation of  $c^2$  is generally quite satisfactory, factor combinations that increase the inherent difficulty of this estimation may require special attention.

## 6.5 STRUCTURE OF THE SIMULATION STUDY OF SEASONAL ENTRAINMENT

The simulation of entrainment sampling presented here is structured as a series of 24-hour days, each of which is individually described by the study of daily entrainment in Sections 6.1 to 6.3. The behavior of entrainment sampling and estimation is examined for two simulated seasons, a short season of 18 days and a long season of 34 days. Organism density is zero during the rest of the entrainment sampling period of about three months.

The short entrainment season consists of 18 days, of which 8 have organism density  $\lambda = 10$ , 4 have  $\lambda = 50$ , 4 have  $\lambda = 100$ , and 2 have  $\lambda = 250$  (organism density  $\lambda$  is expressed here in organisms per 1000 cubic meters, as in Sections 6.1 to 6.3). This seasonal set of  $\lambda$ 's would be a good approximation to the density pattern of a species-life stage combination appearing only in a brief spike during the entrainment sampling period. That is, this type of organism has density zero for part of the entrainment sampling period, then might have 4 days with  $\lambda = 10$ , 2 days with  $\lambda = 50$ , 2 days with  $\lambda = 100$ , 2 days with  $\lambda = 250$ , then 2 days with  $\lambda = 100$ , 2 days with  $\lambda = 50$ , 4 days with  $\lambda = 10$ , followed by density  $\lambda = 0$  for the remainder of the entrainment season. It is important to note, however, that this plausible pattern is only one pattern out of many that generate the specified 18  $\lambda$ 's of our short entrainment season. This is because the time order of the 18 days with these  $\lambda$ 's is immaterial, i.e., has no effect whatsoever on the analysis to be presented. Scattering the 18 specified  $\lambda$ 's randomly, systematically, or arbitrarily throughout the overall entrainment period of three months or so and setting  $\lambda$  equal to zero on every remaining day would produce the same analysis as the spiked entrainment pattern just described.

The long entrainment season consists of 34 days, of which 10 have organism density  $\lambda = 10$ , 10 have  $\lambda = 50$ , 8 have  $\lambda = 100$ , and 6 have  $\lambda = 250$ . This seasonal set of  $\lambda$ 's would be a good approximation to the density pattern of a species-life stage combination appearing in a long spike occurring at some point in the entrainment sampling period. For example, this would be the case for an organism whose density is zero for part of

the entrainment sampling period of three months, then rises to  $\lambda = 10$  for 5 days, then to  $\lambda = 50$  for 5 days,  $\lambda = 100$  for 4 days,  $\lambda = 250$  for 6 days, then falls to  $\lambda = 100$  for 4 days,  $\lambda = 50$  for 5 days,  $\lambda = 10$  for 5 days, and finally to zero for the rest of the sampling period. As with the 18-day entrainment season, this single spike is only one of a multiplicity of possible seasonal patterns resulting in the specified 34  $\lambda$ 's; any distribution of the 34 days across the 3-month entrainment sampling period, with all remaining days having density zero, will produce the same analysis as the single long spike entrainment pattern. This class of distributions includes everything from double spike patterns (two spikes occurring at different times) to randomly located days with nonzero density.

As in Sections 6.1 to 6.3, we investigate the behavior of entrainment sampling and estimation over a range of sample volumes ( $v = 100, 150, 200,$  and  $300$  cubic meters per 3-hour period), temporal patterns (uniform and diel), and values of  $c^2$  (.0, .05, .10, .33, .50, and 1.00). For any combination of these values, our analysis of an entrainment season proceeds as follows.

We begin by introducing some necessary notation, much of which coincides with previously introduced notation. Let

$T$  = true total entrainment over the entire season,

$T(j)$  = true daily total entrainment for day  $j$ ,

$m$  = number of days in the entire season,

$m_\lambda$  = number of days for which mean organism density is  $\lambda$ .

Then

$$T = \sum_{j=1}^m T(j) = T(1) + T(2) + \dots + T(m) \quad (6.1)$$

and

$$\hat{T} = \sum_{j=1}^m \hat{T}(j), \quad (6.2)$$

where  $\hat{T}$  and  $\hat{T}(j)$  are estimates of  $T$  and  $T(j)$ , respectively. From (6.2) to (6.3) below, the analysis applies to  $\hat{T}_1$ ,  $\hat{T}_2^c$ , or  $\hat{T}_2^c$  of Section 6.2, any of which may be what is meant by  $\hat{T}$  in (6.2) to (6.3).

The expected value of  $\hat{T}$ ,  $E(\hat{T})$ , and the simulation estimate of  $E(\hat{T})$  are

$$E(\hat{T}) = \sum_{j=1}^m E(\hat{T}(j)) = m_{10}D_{10} + m_{50}D_{50} + m_{100}D_{100} + m_{250}D_{250},$$

$$\hat{E}(\hat{T}) = \sum \hat{E}(\hat{T}(j)) = m_{10}d_{10} + m_{50}d_{50} + m_{100}d_{100} + m_{250}d_{250},$$

where

$D_{\lambda}$  = expected value of  $\hat{T}(j)$  when mean organism density is  $\lambda$ ,

$d_{\lambda}$  = simulation estimate of  $D_{\lambda}$ .

The variance of  $\hat{T}$  and its simulation estimate are

$$\text{Var}(\hat{T}) = \sum \text{Var}(\hat{T}(j)) = m_{10}U_{10} + m_{50}U_{50} + m_{100}U_{100} + m_{250}U_{250},$$

$$\hat{\text{Var}}(\hat{T}) = \sum \hat{\text{Var}}(\hat{T}(j)) = m_{10}u_{10} + m_{50}u_{50} + m_{100}u_{100} + m_{250}u_{250},$$

where

$U_{\lambda}$  = variance of  $\hat{T}(j)$  when mean organism density is  $\lambda$ ,

$u_{\lambda}$  = simulation estimate of  $U_{\lambda}$ .

The coefficient of variation of  $\hat{T}$  and its simulation estimate are

$$CV(\hat{T}) = [\text{Var}(\hat{T})]^{1/2}/E(\hat{T}),$$

$$\hat{C}V(\hat{T}) = [\hat{\text{Var}}(\hat{T})]^{1/2}/\hat{E}(\hat{T}).$$

(6.3)

An example of the calculations can be constructed using Tables 6.1 to 6.4, in which the variation in organism density within a day is uniform. Take sample volume  $v = 150$  and quadratic variance parameter  $c^2 = .10$ , so all figures will come from the second column of the third page of Tables 6.1 to 6.4. Take  $V = 100,000$  and consider the short, 18-day simulated season. Simulation estimates for the true entrainment are

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$$\begin{aligned}\hat{E}(T) &= 8(8,036.39) + 4(40,174.97) + 4(80,680.24) + 2(199,403.12) \\ &= 946,518.2\end{aligned}$$

$$\begin{aligned}\hat{\text{Var}}(T) &= 8(2,495.99)^2 + 4(6,971.71)^2 + 4(11,906.71)^2 + 2(25,966.30)^2 \\ &= 2,159,835,133\end{aligned}$$

$$\hat{\text{SD}}(T) = [\text{Var}(T)]^{1/2} = 46,474$$

$$\hat{\text{CV}}(T) = 46,474/946,518 = 4.91\%$$

Simulation estimates for the one-sample estimator  $\hat{T}_1$  are

$$\hat{E}(\hat{T}_1) = 8(8,306.67) + 4(40,046.67) + 4(80,240.00) + 2(198,546.67) = 944,693.4$$

$$\begin{aligned}\hat{\text{Var}}(\hat{T}_1) &= 8(3,268.03)^2 + 4(12,024.79)^2 + 4(23,209.48)^2 + 2(55,481.17)^2 \\ &= 8,974,862,755\end{aligned}$$

$$\hat{\text{SD}}(\hat{T}_1) = 94,736$$

$$\hat{\text{CV}}(\hat{T}_1) = 10.03\%$$

Simulation estimates for the two-sample estimator with  $c^2$  estimated,  $\hat{T}_2^c$ , are (with the superscript omitted for brevity)

$$\hat{E}(\hat{T}_2) = 8(8,170.00) + 4(39,636.67) + 4(80,520.00) + 2(201,320.00) = 948,626.7$$

$$\begin{aligned}\hat{\text{Var}}(\hat{T}_2) &= 8(3,354.24)^2 + 4(9,130.54)^2 + 4(15,239.32)^2 + 2(27,660.44)^2 \\ &= 2,882,621,829\end{aligned}$$

$$\hat{\text{SD}}(\hat{T}_2) = 53,690$$

$$\hat{\text{CV}}(\hat{T}_2) = 5.66\%$$

These numbers are not identical to the entries in the second column of the third page of Table 6.21 (page 6.136), which reports the simulation results from the situation just described. However, the numbers are quite close to the table entries. The slight discrepancies are due to the fact that Table 6.21 was produced by a second simulation run, based on different random numbers from the ones that led to Tables 6.1 to 6.4.

The effects of erroneously assuming that the data are Poisson rather than negative binomial are easily shown. The Poisson assumption gives

$$\hat{\text{Var}}(\hat{T}_1) = (V/v)\hat{T}_1 = (666.667)(944,693.4) = 629,795,914.9$$

$$\hat{\text{Var}}(\hat{T}_2) = (V/v)\hat{T}_2/2 = (333.333)(948,626.7) = 316,208,583.8$$

The first of these is much lower than  $\hat{\text{Var}}(\hat{T}_1) = 8,974,862,755$  given above, which is approximately equal to the variance of  $\hat{T}_1$ . The second is much lower than  $\hat{\text{Var}}(\hat{T}_2) = 2,882,621,829$  given above, which is approximately equal to the variance of  $\hat{T}_2$ . The Poisson assumption thus gives estimated variances, standard deviations, and coefficients of variation that are substantially smaller than those obtained from the correct calculations. This produces confidence intervals whose true confidence levels are substantially lower than their nominal confidence levels, which demonstrates the severe shortcomings of using Poisson-based methods in situations when the Poisson assumption fails to hold.

## 6.6 CONCLUSIONS FROM THE SIMULATION STUDY

The simulation study is based on a reasonable approximation of the actual entrainment conditions encountered in practice. The study does not reflect true entrainment conditions exactly: the entrainment seasons of organisms of various species and life stages will not be exactly 3 days, 5 days, 7 days, 18 days, or 34 days in length; organism densities will not behave exactly as specified here; and discharge channel volume and sample volume will not be identical in all 3-hour periods or p.s.u.'s. These approximations are inevitable in a simulation study of a phenomenon as complex as entrainment sampling. We can draw general conclusions from our simulations on the basis of the strong similarities observed between uniform and diel variation of organism density and among 3-day, 5-day, 7-day, 18-day, and 34-day entrainment seasons.

A major conclusion from this study is that the gain from drawing samples larger than 150 cubic meters per 3-hour period would be small relative to the cost involved. This can be seen both in the daily simulation study (Tables 6.1 to 6.8) and the seasonal study (Tables 6.9 to 6.22). The present level of sampling effort is therefore adequate for the type of entrainment estimation considered in this project.

A second conclusion is that, as expected, estimation of entrainment is more difficult when the density of organisms is low. That is, the coefficient of variation of the estimate of seasonal entrainment increases as organism density throughout the season decreases.

A third conclusion is that under many entrainment conditions there are substantial benefits from estimating  $c^2$  and using these estimates to calculate confidence intervals, coefficients of variation, and standard deviations. Estimating  $c^2$  gives shorter confidence intervals and smaller coefficients of variation and standard deviations than the one-sample estimation method, which does not estimate  $c^2$  from paired observations. It also avoids the necessity (discussed in Section 4.5) of using the empirical conservative upper bound,  $c^2 \leq 1$ .

A final conclusion is that, if the entrainment process conforms to the assumptions (e.g., paired observations are replicates from a negative binomial process), the estimation techniques developed in earlier chapters provide an effective means of obtaining entrainment estimates with acceptable margins of error, as measured by standard deviations and coefficients of variation of these estimates.

## 7. SUM-QUOTA SAMPLING IN ENTRAINMENT ESTIMATION

### 7.0 INTRODUCTION

It is possible that nearly all of the entrainment information contained in a sample could be obtained from an analysis of only part of the sample's contents. For example, it might be possible to extract 95% of a sample's information from an analysis of 50% of the sample volume, that is, sorting and identifying ichthyoplankton from only half of the full sample. This is quite a natural prospect: a sample collected during a p.s.u. gives accurate information about the entrainment in the entire discharge channel flow during the p.s.u., which is a much larger volume of cooling water; similarly, a fraction of the sample's contents, if properly analyzed, may give accurate information about the entrainment present in the entire sample. This idea could be exploited by dividing each sample into several equal smaller volumes, called subsamples or splits, and performing laboratory work on some but not all of these. For instance, a sample of volume 160 cubic meters might be divided into eight splits, each of volume 20 cubic meters, and laboratory identification of ichthyoplankton might be performed on four of these splits. The advantages of such a procedure are clear: identification is performed for only part of each sample, so for a given cost we could process larger samples, more samples per p.s.u., or more p.s.u.'s in a noncontinuous sampling plan, giving more accurate entrainment estimates. Alternatively, we could leave the level of accuracy unchanged, choosing instead to lower the cost of estimation.

This general approach of subdividing each sample for analysis leads to three-stage sampling designs, which were described in Section 3.2. As was noted there, the number of splits analyzed from each sample may be either fixed or adaptive. With a fixed sampling plan, the same number of splits is selected at random from each sample and analyzed. With an adaptive sampling plan, splits are selected at random from a given sample and analyzed until certain conditions are met, at which time any remaining splits from the sample are discarded and laboratory analysis begins on the next sample.

The observed subsample counts obtained during a sampling period are subject to two major sources of variation. The first is the variation in the discharge channel, which was shown in Chapter 4 to be adequately modeled by the negative binomial distribution. The second source is the variation among subsamples induced by the splitting of the samples. In order to utilize sample splitting in entrainment estimation, it is necessary to know how split or subsample ichthyoplankton counts are generated from sample counts. (This discussion of counts applies to a particular species-life stage combination; overall entrainment estimates are obtained by summing over these combinations.) In particular, it is important to know how much variation occurs among split counts within a given sample. A low

level of variation among splits leads to great improvements in the efficiency of entrainment estimates. A high level of variation among splits has the opposite result.

To illustrate the key role of variation among splits in estimation procedures for three-stage sampling designs, two extreme situations will now be described. It can be virtually guaranteed that neither of these will be observed in practice, but the true situation could be close to one of them. First, if among-splits variance is zero, which means that each split has exactly the same number of ichthyoplankton, then analyzing a single split gives full information about the entire sample, since the exact sample count can be found by multiplying the split count by the number of splits in the sample. This would allow the cost of analysis to be cut greatly without any change in entrainment estimates or their estimated variances. Second, if extreme clumping of organisms causes most splits from each sample to have organism counts of zero, so each sample's entrainment is concentrated in only a few of the splits created from it, then analyzing only some splits will give very uncertain information about the whole sample. This is because the splits not processed could have either very high or very low counts, so the variability introduced by leaving some splits unprocessed would be quite large.

A model intermediate between these two extremes assumes thorough mixing of the sample before splitting. This means that any organism is allocated to a split independently of all other organisms, with an equal likelihood of being placed in each split. Achieving this complete randomization of the sample is the goal of plankton splitting devices, such as the Pneumatic Mixing Rotational Depositing Plankton Splitter (EA, 1983a, p. 3-5). Consequently, the treatment of three-stage sampling plans in this chapter will be based on this assumption of complete randomization of each sample, which produces split counts with uniform multinomial variation. This model for variation due to splitting will be discussed in Section 7.1.

A crucial question, therefore, is whether splitting devices achieve uniform multinomial variation in split counts. More research on this question is needed, but available information leads to the conclusion that splits exhibiting uniform multinomial variation are very difficult, perhaps impossible, to obtain with currently used splitting devices. These devices produce splits with a greater level of variation among split counts than is consistent with the uniform multinomial model, suggesting that organism clumping may be present. This raises the possibility that variation among splits may be great enough to offset the advantages of splitting. Until more is known about among-splits variation in ichthyoplankton entrainment counts, estimation based on sample splitting must be viewed with caution, and the use of such methods as questionable.

Several additional concerns can be raised about the use of sample splitting in estimating entrainment. One is the potential for errors to occur because of incorrect execution of the necessary laboratory procedures. The division of the sample into splits and the termination of split analysis at the correct time for each sample are steps at which mistakes can be made. A second concern is the additional cost of the splitting of samples: the operation of splitting is labor intensive, additional record-keeping

and quality control are needed, and so on. Finally, it is possible that other uses for the entrainment data, not addressed in this study, could require data from whole samples rather than from the partial samples constituted by those splits that have been analyzed.

If the uncertainties concerning among-splits variation can be resolved, then entrainment estimation techniques based on split counts are likely to provide greater efficiency than techniques based on whole-sample counts. If a splitting device is available that achieves uniform multinomial variation among splits, sum-quota sampling plans will be much more efficient than whole-sample plans in cost and effort expended for a specified level of accuracy of estimation. Sum-quota sampling is an adaptive form of split selection, in which splits from a given sample are selected in random order and analyzed until the total number of ichthyoplankton counted in the sample reaches a specified threshold. Any remaining splits from this sample are then discarded and processing of the next sample's splits is begun. Sum-quota sampling plans have been used for entrainment data in some recent studies (EA, 1983a, p. 3-5).

The remainder of this chapter examines the use of sum-quota sampling in entrainment estimation. Ichthyoplankton counts from subsamples within a sample will be assumed to follow the uniform multinomial distribution, which would be the consequence of a thorough uniform mixing of the sample before splitting. If it can be assured that such mixing is achieved, sum-quota sampling will be much more efficient than whole-sample analysis. We strongly recommend that when splitting devices are available that produce uniform mixing of the sample, all sampling be done on a sum-quota basis.

#### 7.1 SAMPLE SPLITTING AND SUM-QUOTA SAMPLING: ESTIMATION FOR A SINGLE SAMPLE

We begin by considering the analysis of a single sample. This sample may be a single subunit drawn during a p.s.u. of the entrainment season, or it may be a composite sample formed by mixing several of these subunits together. The sample is split into  $L=8$  subsamples of equal volume, called subsamples or splits, for analysis. These are processed in random sequence until a minimum of  $Q=200$  ichthyoplankton have been counted; that is, the processing sequence terminates with the  $\ell^{\text{th}}$  subsample if the total count in the  $\ell$  subsamples or splits is at least  $Q=200$ . The value  $Q=200$  is used here because it was used in the subsampling done at Indian Point and Roseton. Of course, other values of the quota  $Q$  could be used.

Define

$Z_i(\ell)$  = the total count in the first  $\ell$  subsamples processed from the sample  $i$ , which may be either a subunit from a single p.s.u. or a composite of several such subunits.

In discussions of a single sample,  $Z_{(\ell)}$  will be written instead of  $Z_{i(\ell)}$ , for convenience and to avoid drawing attention away from  $\ell$ , the number of subsamples processed from the sample.

It will be assumed that the mechanism used to split the sample provides a uniform mixing of the sample before splitting. This mixing results in a thorough randomization of the sample, allowing us to model the variation in the subsamples with a uniform multinomial distribution. The rest of this paragraph is a formal statistical explanation of the appropriateness of this model for variation in the splits. It is included for completeness, and may be skipped by all but the reader interested in a mathematical proof. For a sample divided into  $L$  splits, let  $X_1, X_2, \dots, X_L$  denote the counts of organisms present in these splits (either total organisms or a particular species-life stage combination). The randomization of the sample produced by the splitter, e.g., the Pneumatic Mixing Rotational Depositing Plankton Splitter (EA, 1983a, p. 3-5) should be independent of the pattern of organisms present in the sample. Thus  $X_1$  to  $X_L$  are independent non-negative integer-valued random variables, and the conditional distribution of  $X_1, X_2, \dots, X_L$  given the value of their sum  $Z_{(L)} = \sum X_i$  is a multinomial with parameters  $\sum X_i, 1/L, 1/L, \dots, 1/L$ .

The total count  $Z_{(\ell)}$  is composed of counts of many different species-life stage combinations. These  $Z_{(\ell)}$  organisms are sorted and counted by type, resulting in a vector of totals by type,  $(Z_{(\ell;1)}, Z_{(\ell;2)}, \dots, Z_{(\ell;C)})$ , whose components sum to  $Z_{(\ell)}$ :

$$Z_{(\ell)} = Z_{(\ell;1)} + Z_{(\ell;2)} + \dots + Z_{(\ell;C)}, \quad (7.1)$$

where  $Z_{(\ell;j)}$  is the total count of the  $j^{\text{th}}$  species-life stage combination observed in the  $\ell$  processed subsamples. The objective of this subsampling procedure is to estimate the composite sample totals by type, namely,  $(Z_{(L;1)}, Z_{(L;2)}, \dots, Z_{(L;C)})$ , where  $Z_{(L;j)}$  is the total count of the  $j^{\text{th}}$  species-life stage combination in the entire sample. If  $Z_{(L)} < Q$  then  $\ell=L$  and, aside from sorting and counting errors, this vector of total counts is then exactly measured.

If  $Z_{(L)} \gg Q$  then subsample (split) processing will terminate at some split number  $\ell < L$ , at a savings in laboratory cost as compared to the cost of processing all  $Z_{(L)}$  organisms. This number  $n$  will not be known in advance since  $Z_{(L)}$  is not known in advance; however, the amount of processing effort is known in advance to be "Q plus overshoot." And this excess over Q has an expected value equal to approximately half of the average abundance per split,

$$E(Z_{(\ell)}) = Q + E(\text{excess}) = Q + \frac{1}{2}(Z_{(L)}/L). \quad (7.2)$$

Since cost as measured by  $Z_{(\ell)}$  is essentially predetermined under this sampling scheme while sample size as measured by  $\ell$  is not fixed in advance, this sampling procedure has been referred to as "fixed cost sampling" to distinguish it from fixed-sample-size procedures (Pathak, 1976). Actual processing costs are not directly proportional to the total number  $Z_{(\ell)}$  of organisms processed, however, and the sampling procedure might be better referred to as "sum-quota sampling" where Q is the predetermined quota imposed on the sample sum  $Z_{(\ell)}$  of an observed non-negative variable.

Sum-quota sampling can be viewed as a generalization of conventional fixed-size sampling, where a sample of predetermined size is collected. In view of this fact it is, perhaps, not surprising that statistical estimation formulas developed under the assumption of fixed-n sampling do carry over almost intact to the more general case of sum-quota or fixed-cost sampling. Pathak (1976) provided mathematical proof that conventional mean and standard error formulas do, indeed, require no modification when applied to the pre-terminal data of a sum-quota sample (ignoring the data from the terminal sampling unit). He also demonstrated, however, that modifications are required if the data from the terminal sampling unit are to be utilized in the estimation procedure. This need can be appreciated by noting that sum-quota sampling will tend to terminate with a sampling unit having a large value of the sum-quota variable. The data from the terminal sampling unit are thus "size-biased" with respect to the sum-quota variable, and with respect to any other variables that are correlated with the sum-quota variable. As a case in point, the terminal ( $\ell^{\text{th}}$ ) one-eighth subsample from a composite sample tends to have a larger-than-average total number of organisms (unless  $Z_{(8)} < 200$ ) and hence is also biased upward with respect to the dominant types in the composite.

The modifications in the statistical analysis needed to adjust for the effects of this size-bias are simple but do require use of the separate data from each of the  $\ell$  subsamples included in the sum-quota sample. There are three different candidates for the estimate of  $Z_{(L)}$ , which we will denote by  $\hat{Z}$ ,  $\hat{Z}_{\text{pre}}$ , and  $\hat{Z}_{\text{adj}}$ .

$\hat{Z}$  is simply the usual estimate of the total count in the L subsamples (splits) based on observing the counts in only  $\ell$  subsamples:

$$\begin{aligned}\hat{Z} &= \frac{L}{\ell} \times (\text{total count in the } \ell \text{ subsamples}) \\ &= \frac{L}{\ell} Z_{(\ell)}.\end{aligned}\quad (7.3)$$

$\hat{Z}_{\text{pre}}$  is obtained by the same calculation except that the count in the last ( $\ell^{\text{th}}$ ) subsample analyzed is ignored:

$$\begin{aligned}\hat{Z}_{\text{pre}} &= \frac{L}{\ell-1} \times (\text{total count in the first } \ell-1 \text{ subsamples}) \\ &= \frac{L}{\ell-1} Z_{(\ell-1)}.\end{aligned}\quad (7.4)$$

This estimator is an unbiased estimator of  $Z_{(L)}$ ; the average value of the estimator is exactly equal to the target value  $Z_{(L)}$ ,

$$E(\hat{Z}_{\text{pre}}) = Z_{(L)}.\quad (7.5)$$

$\hat{Z}_{\text{pre}}$  will, however, have a larger variance than  $\hat{Z}$ , since it is based on fewer observations ( $\ell-1$  rather than  $\ell$  observations). Thus, in choosing between  $\hat{Z}$  and  $\hat{Z}_{\text{pre}}$ , we must choose between an unbiased estimator with a larger variance and a biased estimator with a smaller variance.

$\hat{Z}_{\text{adj}}$  is an attempt to combine the good points of both  $\hat{Z}_{\text{pre}}$  and  $\hat{Z}$ . The adjustment algorithm exploits the unbiasedness of  $\hat{Z}_{\text{pre}}$  by averaging over all possible pre-terminal estimates that could have resulted from different orderings of the observed subsample counts. More precisely, if we let  $X_j$  denote the total count in the  $j^{\text{th}}$  subsample, the adjusted estimator is obtained by averaging all possible pre-terminal estimates  $[L/(\ell-1)][Z_{(\ell)} - X_j]$  that can be formed using observed counts  $X_j$  that exceed  $Z_{(\ell)} - Q$ , that is, all observed  $X_j$  that could possibly have been the terminal observation.

(To be a possible terminal observation, a count  $X_j$  must exceed  $Z_{(\ell)} - Q$ . This is because if it does not, then the sum of the preceding  $\ell - 1$  observations must equal or exceed the quota  $Q$ , which would result in the cessation of counting before the  $\ell^{\text{th}}$  subsample. For instance, with  $Q=200$  and  $L=8$ , sampling would terminate after the sequence of split counts 20, 52, 46, 39, 25, 49, saving the cost of processing two splits. Then  $\ell=6$  and  $Z_{(\ell)}=231$ . Neither  $X_1=20$  nor  $X_5=25$  is a possible terminal count; for example, sampling could not end with an observation of 20 that results in  $Z_{(6)}=231$ , since this would imply  $Z_{(5)}=211$ , i.e., the sum of the first five observations would exceed the quota  $Q=200$ .)

Letting  $\bar{X}_t$  denote the average of all split counts that are greater than  $Z_{(\ell)} - Q$ , then the formula for the adjusted, unbiased estimator of  $Z_{(L)}$  becomes:

$$\hat{Z}_{\text{adj}} = L \left( \frac{Z_{(\ell)} - \bar{X}_t}{\ell - 1} \right) \quad (7.6)$$

If  $\ell=1$  then no adjustment is made and  $\hat{Z}_{\text{adj}}=LZ_{(1)}$ ; likewise, if  $\ell>1$  and all  $\ell$  observations exceed  $Z_{(\ell)} - Q$  then  $\bar{X}_t=Z_{(\ell)}/\ell$  and  $Z_{\text{adj}}=LZ_{(\ell)}/\ell$ . An outcome in which none of the  $\ell$  observations exceed  $Z_{(\ell)} - Q$  can occur only with  $\ell=L$ , and then  $Z_{(L)}$  is not to be adjusted, so  $\hat{Z}_{\text{adj}}=Z_{(L)}$ .

A small numerical example will serve both to illustrate the size-bias of the terminal observation and to demonstrate the adjustment needed for estimating the total count. In order to reduce the arithmetic we consider a 3-way split instead of an 8-way split. Suppose we have set a quota of  $Q=8$  and, unknown to us, the numbers of organisms in the  $L=3$  subsamples are 1, 3, and 5, respectively, so the estimation target is  $Z_{(L)}=9$ .

For this simple example, the possible outcomes of a sum-quota sampling strategy are:

Subsample Sequence	$\hat{Z}$ (usual estimate)	$\hat{Z}_{\text{pre}}$ (pre-terminal estimate)	$\hat{Z}_{\text{adj}}$ (adjusted estimate)
1,3,5	9	6	7.5
3,1,5	9	6	7.5
1,5,3	9	9	7.5
5,1,3	9	9	7.5
3,5	12	9	12
5,3	12	15	12
Average	10	9	9

Each of the possible sample sequences is equally likely to be realized, and on average the usual estimate  $\hat{Z}$  exceeds the true value of  $Z_{(L)}=9$ . The adjusted estimate  $\hat{Z}_{adj}$ , which averages to exactly the true value, may at first glance appear strange, since in four of the six possible outcomes this estimate of the total number of organisms is actually less than the observed number!

For example, in the first subsample sequence (1,3,5), either 3 or 5 could have been the terminal observation, but 1 could not have been. Thus

$$\bar{X}_t = (3+5)/2 = 4, \text{ and}$$

$$\hat{Z}_{adj} = 3 \left( \frac{9 - 4}{2} \right) = 7.5 \quad (7.7)$$

When  $l=L$ , the adjusted estimate will be less than the known total count  $Z_{(L)}$  unless all  $L$  observations exceed  $Z_{(L)} - Q$ . A rational justification for deliberately committing such a known error of underestimation in a sample is the concomitant knowledge that overestimation will by chance occur in other samples to compensate in the long run; indeed, if the adjustment algorithm is not strictly adhered to, the laboratory will, in the long run, overestimate abundance in the samples when using the sample splitting strategy. Since annual entrainment totals are the ultimate estimation target and since these are compiled from the counts in numerous samples, there is compelling reason to apply mean-unbiased estimation methods to the individual samples.

In the above numerical example showing the deliberate adjustment from a known value of  $Z_{(L)}=9$  down to an estimate of  $\hat{Z}_{adj}=7.5$ , the concomitant knowledge of the chance for compensating overestimation is also plainly revealed in the remaining rows of the table. With all  $L$  subsample counts (1,3,5) in hand, the remainder of the table could be constructed after the fact, showing the exact chance and the exact amount of overestimation that might have occurred and that, we must assume, will occur among some of the other samples counted incompletely by the split sample, sum-quota method.

Therefore, in deciding among the three estimators, we must assess the amount of bias that may be present in our situation and must weigh this against the amount of variance reduction that is possible. Since these quantities depend on the unknown parameters of the problem, we must examine a range of likely values of these parameters. Using both theoretical calculations and Monte Carlo simulations, we can arrive at a reasonable choice.

## 7.2 VARIANCE AND BIAS IN SUBSAMPLING ESTIMATION

From the three candidate estimators  $\hat{Z}$ ,  $\hat{Z}_{pre}$ , and  $\hat{Z}_{adj}$ , we must choose the one that is optimal for our purposes. We know that  $\hat{Z}_{pre}$  and  $\hat{Z}_{adj}$  are unbiased,  $\hat{Z}$  is biased, and

$$\text{Variance}(\hat{Z}) < \text{Variance}(\hat{Z}_{pre}) \quad (7.8)$$

$$\text{Variance}(\hat{Z}_{adj}) < \text{Variance}(\hat{Z}_{pre});$$

however, the variance of  $\hat{Z}$  and  $\hat{Z}_{adj}$  are not directly comparable. In making a practical decision as to which estimator is best, we must consider, among other things,

- i) The magnitude of the bias of  $\hat{Z}$
- ii) The magnitude of variance reduction possible
- iii) Ease of calculation.

Such comparisons could be carried out on a theoretical level, but the expressions involved are quite complicated, making direct comparisons extremely difficult. For example, if we calculate  $\hat{Z}$  based on  $q$  out of  $L$  possible subsamples, and the underlying distribution is Poisson( $\lambda$ ), then (see Appendix V) the expected value of  $\hat{Z}$  is given by

$$E(\hat{Z}) = \lambda \left[ L + L \sum_{i=2}^L \frac{e^{-(i-1)\lambda} [(i-1)\lambda]^{Q-1}}{i(Q-1)!} \right] \quad (7.9)$$

where  $Q$  is the chosen quota. Variance calculations are even more involved, so rather than attempt theoretical comparisons, it was decided that a more fruitful attack would be through a simulation study.

### 7.2.1 OUTLINE OF SUM-QUOTA ESTIMATION SIMULATION STUDY

#### 7.2.1.1 Poisson( $\lambda$ ) distribution

Fix  $\lambda$  and calculate the vectors  $f$  and  $F$  giving the Poisson pmf (probability mass function) and cdf (cumulative distribution function) respectively:

$$\underline{f} = [f(0), f(1), f(2), \dots, f(R)]$$

$$\underline{F} = [F(0), F(1), F(2), \dots, F(R)]$$

where  $R$  is the first integer making  $P[\text{Poisson}(\lambda) \text{ variable} \leq R]$  greater than or equal to .99999:

$$F(R) \geq .99999 > F(R-1);$$

then round  $\underline{F}$  to 5 decimal places.

#### 7.2.1.2. Poisson sample

Generate  $L=8$  uniform random numbers  $U_1, \dots, U_L, 0 \leq U_i \leq .99999$ ; using the Poisson( $\lambda$ ) cdf  $F$ , calculate the vector  $\underline{X}=(X_1, \dots, X_L)$  such that

$$F(X_j-1) < U_j \leq F(X_j);$$

compute  $Z_L = X_1 + \dots + X_L$ .

#### 7.2.1.3. Finite population sampling

Calculate  $M=1000$  random permutations of the vector  $\underline{X}$ . For each permutation compute  $\underline{Z}=(Z_{(1)}, Z_{(2)}, \dots, Z_{(L)})$  and

compute  $\ell$  such that  $Z_{(\ell-1)} < Q \leq Z_{(\ell)}$ , with  $\ell=L$  if  $Z_{(L)} < Q$

compute  $\hat{Z} = LZ_{(\ell)}/\ell$

compute  $X_\ell$ , with  $X_\ell=0$  if  $\ell=L$  and  $Z_{(L)} < Q$

compute  $\hat{Z}_{\text{pre}} = L(Z_{(\ell)} - X_\ell)/(\ell-1) = LZ_{(\ell-1)}/(\ell-1)$

order the vector  $(X_1, \dots, X_\ell)$  and compute the number  $k$  of  $X_j$ 's exceeding  $Z_{(\ell)} - Q$ ;

if  $k=0$  set  $D=0$  and  $\bar{X}_k=0$ ; otherwise set  $D=1$  and set  $\bar{X}_k$  equal to the mean of these  $k$   $X_j$ 's that satisfy  $X_j > Z_{(\ell)} - Q$

compute  $\hat{Z}_{adj} = L(Z_{(\ell)} - \bar{X}_k) / (\ell - D)$

compute the unit vectors  $\underline{D} = [D(0), \dots, D(L)]$  and  $\underline{E} = [E(1), \dots, E(L)]$ , where

$$D(i) = \begin{cases} 1 & \text{for } i=k \\ 0 & \text{for } i \neq k \end{cases} \quad \text{and } E(i) = \begin{cases} 1 & \text{for } i = \ell \\ 0 & \text{for } i \neq \ell \end{cases}$$

compute the unit vector  $\underline{W} = [W(0), \dots, W(K)]$  where

$$W(i) = \begin{cases} 1 & \text{if } i = X_\ell \\ 0 & \text{otherwise} \end{cases}$$

For the  $M$  permutations of  $\underline{X}$  calculate the mean and the covariance matrix (dividing by  $M-1$ ) of

$$Y = (Z_{(L)}, \ell, \hat{Z}, X_\ell, \hat{Z}_{pre}, k, \bar{X}_k, \hat{Z}_{adj}, D)$$

and the mean of

$$C = (\underline{D}, \underline{E}, \underline{W})$$

#### 7.2.1.4 Replication

Repeat the sampling of Sections 7.2.1.2 and 7.2.1.3  $10 \cdot M$  times, and compute the mean and covariance matrix of  $\bar{Y}$  and the mean of  $\bar{C}$ .

## 7.2.2 RESULTS OF THE SIMULATION STUDY

Simulation according to the preceding program consisted of generating pseudorandom data  $X_1, X_2, \dots, X_8$  representing the number of ichthyoplankton in each one-eighth portion of a sample, and then accumulating the counts sequentially:

$$\begin{aligned} &X_1 \\ &X_1 + X_2 \\ &X_1 + X_2 + X_3 \\ &\vdots \end{aligned}$$

until the cumulative count first reaches or exceeds the sum-quota  $Q=200$  (or until all 8 portions have been counted in cases where the grand total  $Z_{(8)} = X_1 + X_2 + X_3 + \dots + X_8$  is less than 200). The total count  $Z_{(8)}$  is always known in this artificial situation, so the estimates of  $Z_{(8)}$  obtained by simulated sum-quota sampling can be compared to the actual  $Z_{(8)}$  in order to assess bias and variance of the sampling error in the estimates.

This computer-based experiment was undertaken to provide empirical verification of various theoretical properties of the sum-quota sampling procedure and, in particular, to determine the adequacy of certain theoretical approximations concerning bias and variance. The bias and sampling variance of the unadjusted estimator  $\hat{Z} = 8\bar{X}$  are of special concern, first, to ascertain whether the bias is negligible and, second, to determine whether its sampling variance is less than that of the adjusted unbiased estimator. Both the record keeping and the calculations required for the bias adjustment are a nuisance to be avoided if possible.

As an illustration of the simulator output (Table 7.1) we consider a case in which  $Z_{(8)}$  is Poisson distributed with a mean value of 400, so  $X_1, X_2, \dots, X_8$  are independent Poisson variables each having expected value  $\lambda=50$ . Total variance of an estimator such as  $\hat{Z} = 8\bar{X}$  is then comprised of two components corresponding to the sampling error,  $\hat{Z} - Z_{(8)}$ , and the Poisson error,  $Z_{(8)} - 400$ , respectively:

$$\hat{Z} - 400 = (\hat{Z} - Z_{(8)}) + (Z_{(8)} - 400) \quad (7.10)$$

Because these two components of the estimation error are uncorrelated, the variance of the estimation error is the sum of the two variance components: the sampling error variance of the sum-quota  $\hat{Z}$  as an estimate of

Table 7.1

Example of Output from Simulation Study

```

SIM
ENTER Q
Q: 200
ENTER L
L: 8
ENTER LAMDA
L: 50
ENTER NUMBER OF PERMUTATIONS
Q: 100
ENTER NUMBER OF X VECTORS
Q: 100
ENTER NUMBER OF REPLICATES
Q: 100
THE FREQUENCY DISTRIBUTION OF K IS
      0      1      2      3      4      5      6      7      8
0.0000  0.0318  0.0398  0.0563  0.5885  0.2828  0.0008  0.0000  0.0000

```

```

THE FREQUENCY DISTRIBUTION OF LN IS
      1      2      3      4      5      6      7      8
0.0000  0.0000  0.0000  0.4875  0.5096  0.0029  0.0000  0.0000

```

Q: Q is the preassigned quota for the total count; counting of successive subsamples, each a one-eighth portion of the sample, continues until the cumulative count first equals or exceeds the quota Q.

L: L=8 is the number of subsamples into which the sample is split.

LAMDA:  $\lambda$  is the expected count per subsample.

# PERMUTATIONS: A sample of L=8 counts is to be simulated by generating a vector  $X=(X_1, X_2, \dots, X_8)$  of independent Poisson counts from a Poisson distribution with parameter  $\lambda$ . The sum-quota sampling rule is to be applied to 100 independent permutations of this vector.

# X VECTORS: = NUMBER OF REPLICATES = number of simulated X-vectors.  
 K is the number of terminal candidates in a sum-quota sample.  
 LN is the number of observations in a (sum-quota) sample.  
 XLN is the terminal observation in the sample.

Table 7.1 (continued)

THE MEAN OF THE Y VECTOR IS

ZL	LN	Z1	XLN	Z2	K	XKBAR	Z3
399.2100	4.5154	401.4229	51.4194	398.9238	4.0531	51.3766	399.0352

THE COVARIANCE MATRIX OF YM IS

ZL	LN	Z1	XLN	Z2	K	XKBAR	Z3
425.0161	-6.2199	431.7611	54.5079	430.5687	-6.2727	55.1665	428.7786
-6.2199	0.0982	-6.3536	-0.8044	-6.3284	0.0991	-0.8088	-6.3167
431.7611	-6.3536	443.8540	57.5881	439.3045	-6.7051	57.9258	438.3728
54.5079	-0.8044	57.5881	8.3268	55.1320	-1.0060	8.1377	55.6357
430.5687	-6.3284	439.3045	55.1320	438.9545	-6.3642	56.0778	436.3895
-6.2727	0.0991	-6.7051	-1.0060	-6.3642	0.1629	-1.0314	-6.3156
55.1665	-0.8088	57.9258	8.1377	56.0778	-1.0314	8.2254	55.8988
428.7786	-6.3167	438.3728	55.6357	436.3895	-6.3156	55.8988	435.5521
0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

THE MEAN COVARIANCE OF Y WITHIN X VECTORS IS

LN	Z1	XLN	Z2	K	XKBAR	Z3	D
0.1599	-4.3882	0.0526	-5.5722	-0.0752	0.0778	-5.6336	0.0000
-4.3882	312.3722	37.2248	315.1348	-3.1208	35.8901	318.6008	0.0000
0.0526	37.2248	44.6872	-57.2185	-2.2721	12.2368	22.1572	0.0000
-5.5722	315.1348	-57.2185	545.6664	0.6180	20.4329	352.7891	0.0000
-0.0752	-3.1208	-2.2721	0.6180	0.6299	-2.2979	0.6689	0.0000
0.0778	35.8901	12.2368	20.4329	-2.2979	12.2389	20.4874	0.0000
-5.6336	318.6008	22.1572	352.7891	0.6689	20.4874	357.0124	0.0000
0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

THE MEANS OF Z1, Z2 AND Z3 FOR LN ARE

	LN							
	1	2	3	4	5	6	7	8
Z1	0.0000	0.0000	0.0000	422.7635	381.4606	321.8391	0.0000	0.0000
Z2	0.0000	0.0000	0.0000	422.5739	376.7943	311.9448	0.0000	0.0000
Z3	0.0000	0.0000	0.0000	422.7635	376.8528	308.2345	0.0000	0.0000

The Y-vector is a vector of summary statistics calculated for each permutation of each X-vector. ZL is the total of the L=8 observations in the X-vector. XBAR=XKBAR is the mean of the k terminal candidates. D may be ignored.

For each X-vector the mean and covariance of the Y-vector were computed first over permutations within X-vectors and then among X-vectors. The latter is called the covariance matrix of YM, where YM is the mean for a fixed X-vector.

Output variable:	ZL	LN	Z1	XLN	Z2	K	XBAR	Z3
Equivalent term in text:	$Z_{(L)}$	$l$	$\hat{Z}$	$X_l$	$\hat{Z}_{pre}$	$k$	$\bar{X}_k$	$\hat{Z}_{adj}$

the actual total  $Z_{(8)}$ , and the Poisson variance of  $Z_{(8)}$  about its average value 400:

$$\text{Var}(\hat{Z} - 400) = \text{Var}(\hat{Z} - Z_{(8)}) + \text{Var}(Z_{(8)} - 400). \quad (7.11)$$

Since Poisson variance is equal to the Poisson mean, the second component of variance is  $L\lambda=8(50)=400$ . An approximation to the sum-quota sampling error variance component is given by

$$\begin{aligned} \text{Var}(\hat{Z} - Z_{(8)}) &= L^2 \left( \frac{\lambda}{Q + \lambda/2} - \frac{1}{L} \right)^2 \lambda = \frac{(L\lambda)^2}{Q + \lambda/2} - L\lambda \\ &= \frac{(400)^2}{200 + 50/2} - 400 = 711 - 400 = 311 \end{aligned} \quad (7.12)$$

which is very close to the mean of the empirical variances of  $\hat{Z}$ , 312.3722, listed for  $Z1=\hat{Z}$  in the mean covariance matrix. The empirical value of the Poisson variance of  $ZL=Z_{(8)}$  is the leading entry in "the covariance matrix of YM," namely, 425.0161 as compared to the expected value of 400. Such a discrepancy between an empirical variance and a theoretical variance is well within the range of chance variation.

The expected value for  $\hat{Z}$  is approximately

$$E(\hat{Z}) = L\lambda \left( 1 + \frac{1}{Q-1} \right) = 400 \left( 1 + \frac{1}{199} \right) = 402 \quad (7.13)$$

as compared to the mean of all the 10,000 simulated values of  $Z1=\hat{Z}$  listed in "the mean of the Y vector" as 401.4229. At  $Q=200$  the approximate bias is

$$\text{percent bias} = \frac{100}{Q}\% = 0.5\% \quad (7.14)$$

and the empirical bias is even smaller. Interestingly, this is a situation where the empirical bias is more relevant than the theoretical bias because the latter is due primarily to extremely large but extremely rare values of  $\hat{Z}$  that simply do not occur in 10,000 trials due to their extremely small probability of occurrence.

A hint of this phenomenon is found in the last lines of output of simulation in Table 7.1, where the 10,000 values of  $\hat{Z}=Z1$  were sorted by  $\ell$ , the number of one-eighth portions needed to reach the sum-quota of  $Q=200$ . The line labeled Z1 shows the average value of  $\hat{Z}$  within each sample size class that is represented among the 10,000 trials (with zero printed for empty sample size classes). Since

$$\begin{aligned}\hat{Z} = Z1 = L\bar{X} &= L\left(\frac{X_1 + X_2 + \dots + X_\ell}{\ell}\right) \\ &= L\left(\frac{Q + \text{excess over } Q}{\ell}\right) \\ &= 8\left(\frac{200 + \text{excess}}{\ell}\right)\end{aligned}\tag{7.15}$$

and since the amount of excess over 200 is only weakly correlated with  $\ell$ , then it is clear that as  $\ell$  increases the class mean must decrease or, equivalently, if  $\ell$  is small then the class mean must be large. Only the chance combination of several unduly large counts can produce a small  $\ell$  and hence an unduly large  $\bar{X}$ , and it is the very rare combinations (not even seen in the 10,000 trials) that mainly contribute the 0.5% positive bias in  $\hat{Z}$ .

The small empirical bias in  $\hat{Z}$  found in this simulation experiment and numerous other simulation experiments at different parameter settings is of negligible magnitude and does not detract from the advantages of this estimator over the exactly unbiased estimators  $\hat{Z}_{\text{pre}}=Z2$  and  $\hat{Z}_{\text{adj}}=Z3$ .

Moreover, the empirical evidence consistently shows that the sampling error variance of  $\hat{Z}$  is smaller than that of the adjusted estimator Z3, as well as that of Z2. Thus,  $\hat{Z}$  is seen to have the advantage of better performance as well as greater simplicity.

The precision of this estimator may be measured by its coefficient of variation (CV) or relative standard error

$$CV = \frac{\text{standard error}}{\text{mean}}$$

which in the present context becomes

$$CV = \frac{\sqrt{\text{Var}(\hat{Z})}}{L\lambda}\tag{7.16}$$

If the numerator includes both components of variance, the sampling error variance  $\text{Var}(\hat{Z} - Z_{(g)})$  and the Poisson variance component  $\text{Var}(Z_{(g)} - L\lambda) = L\lambda$ , then the theoretical approximation of the CV becomes

$$CV = \frac{1}{\sqrt{Q + \lambda/2}} = \frac{1}{\sqrt{200 + 50/2}} = .06667 \quad (7.17)$$

The empirical estimate derived from this simulation is

$$\frac{\sqrt{312.3722 + 425.0161}}{401.4229} = .06765$$

### 7.3 VARIANCE OF THE ESTIMATE OF ENTRAINMENT ABUNDANCE

The results of the simulation study discussed in Section 7.2 show that the variance-bias trade-off in subsampling is favorable to  $\hat{Z} = (L/l)Z_{(l)}$ .

To calculate the overall variance of  $\hat{T}$  for the entrainment season, however, we must also take into account the negative binomial variation in the discharge channel. As in Chapter 5,  $T$  represents total entrainment over  $N$  sampling periods,  $T_i$  represents entrainment over sampling period  $i$ , and  $\hat{T}$  and  $\hat{T}_i$  represent estimates of these. A sampling period may be either a single p.s.u. or a composite of several p.s.u.'s, composition making the most practical sense for p.s.u.'s in which entrainment is very low and the corresponding counts are small.

Sum-quota ( $Q$ ) Poisson subsampling of a negative binomial( $K, p$ ) sample achieves a coefficient of variation of approximately

$$CV = \sqrt{\frac{1}{K} + \frac{1}{Q}} \quad (7.18)$$

in the estimation of

$$\mu_i = \text{number of organisms per volume } v_i$$

by the estimator  $\hat{Z}$  for sampling period  $i$

$$\hat{\mu}_i = \hat{Z}_i = \hat{Z}_i(L),$$

where  $v_i$  is the volume sampled in sampling period  $i$ . With subsampling of the  $L=8$  splits of the sample whose volume is  $v_i$ , the number of splits sorted and identified is  $\ell_i$ . There is a subsampling error component in the estimate

$$\hat{\mu}_i = \hat{Z}_i = Z_i + (\hat{Z}_i - Z_i),$$

where  $Z_i$  denotes the (unknown) complete sample count of all  $L$  splits while

$$\hat{Z}_i = (L/\ell_i)Z_i(\ell)$$

is based only on the observed sum of  $\ell_i$  counts. Random splitting induces Poisson variance among the subsamples ( $S_i^2 \cong Z_i/L$  and  $\sigma_i^2 \cong \mu_i/L$ ), hence

$$\text{Var}(\hat{Z}_i - Z_i) \cong \frac{L - \ell_i}{\ell_i} \mu_i = \frac{L}{\ell_i} \mu_i - \mu_i.$$

When  $n_i$  is based on sum-quota sampling, we have

$$(\ell_i/L)\mu_i \cong Q$$

and hence

$$\text{Var}(\hat{Z}_i - Z_i) \cong \mu_i^2/Q - \mu_i.$$

Adding this subsampling variance component to the sampling variance component

$$\text{Var}(Z_i) = \mu_i + (1/K)\mu_i^2$$

gives

$$\text{Var}(\hat{\mu}_i) = \text{Var}(\hat{Z}_i) = \text{Var}(Z_i) + \text{Var}(\hat{Z}_i - Z_i) \cong \left(\frac{1}{Q} + \frac{1}{K}\right)\mu_i^2.$$

Note that  $\text{Var}(Z_i) = \text{Var}(\hat{Z}_i)$  when  $Q = \mu_i$ .

The estimated entrainment for sampling period  $i$  is  $\hat{T}_i = V_i \hat{\mu}_i / v_i$ , where  $V_i$  is the total volume discharged in that sampling period. This estimator has variance

$$\text{Var}(\hat{T}_i) = (V_i^2 / v_i^2) \mu_i^2 \left( \frac{1}{K} + \frac{1}{Q} \right) \quad (7.19)$$

If sampling is carried out over  $N$  sampling periods, then the estimate of total entrainment is obtained by summing over these  $N$  periods, which gives

$$\hat{T} = \sum_{i=1}^N \hat{T}_i = \sum_{i=1}^N (V_i / v_i) \hat{\mu}_i = \sum_{i=1}^N (V_i / v_i) \hat{Z}_i \quad (7.20)$$

$$\text{Var}(\hat{T}) = \sum_{i=1}^N (V_i^2 / v_i^2) \mu_i^2 \left( \frac{1}{K} + \frac{1}{Q} \right) = \sum_{i=1}^N T_i^2 \left( \frac{1}{K} + \frac{1}{Q} \right) \quad (7.21)$$

and

$$[\text{CV}(\hat{T})]^2 = \frac{1}{N} \left( \frac{1}{K} + \frac{1}{Q} \right) \left( 1 + [\text{CV}(T_i)]^2 \right) \quad (7.22)$$

where

$$\begin{aligned} [\text{CV}(T_i)]^2 &= \frac{1}{N} \left[ \sum_{i=1}^N T_i^2 - N \left( \frac{1}{N} \sum_{i=1}^N T_i \right)^2 \right] / \left[ \frac{1}{N} \sum_{i=1}^N T_i \right]^2 \\ &= \sigma_T^2 / \bar{T}^2 \end{aligned} \quad (7.23)$$

Note that if there is no variation among the  $N$  sampling periods in discharge channel flow rate ( $V_i = V$  for each  $i$ ) over this period then  $\text{CV}(T_i) = \text{CV}(\mu_i / v_i)$ .

If, in addition, the sampling gear intake rate is the same for all sampling periods ( $v_i = v$  for each  $i$ ), this reduces to  $\text{CV}(T_i) = \text{CV}(\mu_i)$ . If the latter conditions are satisfied, so that  $T = (V/v) \Sigma \mu_i$  and

$$\hat{T} = (V/v) \sum_{i=1}^N \hat{\mu}_i$$

then

$$\frac{\text{Var}(\hat{T})}{\hat{T}^2} = \left( \frac{1}{K} + \frac{1}{Q} \right) \frac{\sum_{i=1}^N \mu_i^2}{\left( \sum_{i=1}^N \mu_i \right)^2} = \frac{1}{N} \left( \frac{1}{K} + \frac{1}{Q} \right) \left( 1 + [\text{CV}(\mu_i)]^2 \right). \quad (7.24)$$

The estimated variance of  $\hat{T}$  is given by

$$\hat{\text{Var}}(\hat{T}) = \sum_{i=1}^N \hat{T}_i^2 \left( \frac{1}{K} + \frac{1}{Q} \right). \quad (7.25)$$

The estimated coefficient of variation of  $\hat{T}$  is given by

$$[\hat{\text{CV}}(\hat{T})]^2 = \frac{1}{N} \left( \frac{1}{K} + \frac{1}{Q} \right) (1 + [\text{CV}(\hat{T}_i)]^2) \quad (7.26)$$

where  $[\text{CV}(\hat{T}_i)]^2$  is obtained by substituting  $\hat{T}_i$  for  $T_i$  throughout the equation (7.23). These formulas are closely related to those in Section 5.7, where subsampling is not performed. For example, when  $1/u_i$  is replaced by  $1/Q$  and  $c^2$  by  $1/K$  in the approximate expression for  $\text{CV}(\hat{T})$  in (5.49), the result (after some routine algebra) is (7.22).

The effect of the sample volume  $v$  on  $\hat{\text{CV}}(\hat{T})$  under sum-quota sampling is seen to be reflected mainly through  $Q$ , the sum-quota invoked in the subsampling process. This means that if the sum-quota  $Q$  almost always causes laboratory analysis to terminate long before the entire sample has

been processed, then increasing  $v$  will have little or no effect on the accuracy of the estimation of  $T$ ; as long as  $Q$  remains fixed, a larger  $v$  will result only in reaching  $Q$  after processing a smaller fraction of the sample. Thus it is appropriate to view the sum-quota  $Q$  as an implicit function of the sample volume  $v$ , with the choice of the sum-quota  $Q(v)$  based on the value of  $v$ .

#### 7.4 THE ESTIMATION OF ENTRAINMENT REDUCTION UNDER SUM-QUOTA SAMPLING

The analysis of Section 5.8, in which the estimation of entrainment reduction under fixed-sample-size sampling was discussed, can be extended to the case of sum-quota sampling. If subsampling is performed, the treatment of Section 5.8 remains valid after the term  $1/\hat{\mu} = 1/\hat{\mu}_i = V_i/v_i \hat{T}_i$  in (5.49), (5.51), (5.52), (5.54), and (5.55) is replaced by  $1/Q$  and  $c^2$  is replaced by  $1/\hat{K}$ , giving

$$\hat{C}V(\Sigma \hat{T}_i) = (1/\Sigma \hat{T}_i) [\Sigma \hat{T}_i^2]^{1/2} \left[ \frac{1/Q + 1/\hat{K}}{1 + 1/Q + 1/\hat{K}} \right]^{1/2} < (Q^{-1} + c^2)^{1/2}$$

#### 7.5 OPTIMIZATION OF SUM-QUOTA SAMPLING

In this section we describe some of the operating characteristics of sum-quota sampling, and compare them to those of fixed-sample-size sampling, that is, whole-sample plans. Furthermore, we describe some techniques to optimize the sum-quota sampling plan in which the sum-quota  $Q = 200$  and the number of splits per sample  $L = 8$ ; these values were used in a 1982 study at Bowline Point (EA, 1983a, p. 3-5).

Sum-quota strategies that have been used impose a quota of  $Q=200$  on the total number of ichthyoplankton counted, irrespective of taxon and life stage. Such a strategy can be optimized, however, by imposing double-sampling quotas, with larger quotas on counting and sorting of life stages and smaller quotas on species identification. To illustrate the potential savings, we consider here the operating characteristics of a sum-quota procedure for estimating the relative numbers in a mixture of two types of organisms. (An example might be the sorting of counted eggs into those of species A and those not of species A.)

The procedure considered here is to continue sorting into two piles, called the "success" pile and the "failure" pile, until either the number in the success pile reaches the predetermined quota  $Q_S$  or the number in the failure pile reaches the predetermined quota  $Q_F$ . Sorting terminates as soon as one of these quotas is attained, so the total number examined under this strategy can be at most  $Q_S + Q_F$ . The total number available for sorting, say  $U$ , is presumed known and may represent the outcome  $U = Q + \text{excess}$  of a previous stage of sum-quota sampling. The objective of the present stage is to estimate the numbers  $T_S = Up_S$  and  $T_F = U(1-p_S)$  by estimating the fraction  $p_S$  of type S.

Sorting at this stage might proceed in sequential batches, where a "batch" might represent one portion of a split sample, or the number observable in one field on a microscope slide. If such batch sorting is practical then sorting may terminate with an excess over the quota; i.e., at termination the sample counts  $t_S$  and  $t_F$  will satisfy either  $t_S < Q_S$ ,  $t_F \geq Q_F$  or  $t_S \geq Q_S$ ,  $t_F < Q_F$  (unless the last batch happened to bring both counts over their respective quotas or unless  $U < Q_S + Q_F$  and  $T_S < Q_S$ ,  $T_F < Q_F$ , in which case the entire lot would be sorted giving  $t_S < Q_S$ ,  $t_F < Q_F$ ,  $t_S + t_F = U$ ).

The graph in Figure 7.1 illustrates the operating characteristics of such a procedure, expressed in terms of the standard error of the estimator  $\hat{p}_S = t_S / (t_S + t_F)$  and the expected value of the sample size  $t_S + t_F$ . Batch size of 1 was assumed in these calculations, and other nuisance complications were removed by assuming  $T_S > Q_S$  and  $T_F > Q_F$  so at termination either  $t_S = Q_S$  and  $t_F < Q_F$  or  $t_S < Q_S$  and  $t_F = Q_F$ . The probability distribution of the outcome  $(t_S, t_F)$  is then a mixture of inverse hypergeometric distributions with parameters  $(T_S, T_F)$  and  $(Q_S, Q_F)$  or (if  $U \gg Q_S + Q_F$ ) negative binomials with parameters  $p_S$  and  $(Q_S, Q_F)$ . Operating characteristics were based on the latter,

Figure 7.1: Operating Characteristics of Sum-Quota Sampling in Sorting to Estimate the Proportion( $p$ ) of Organisms of a Given Type

(Graph is symmetric about  $p = 1/2 = 50$  percent)

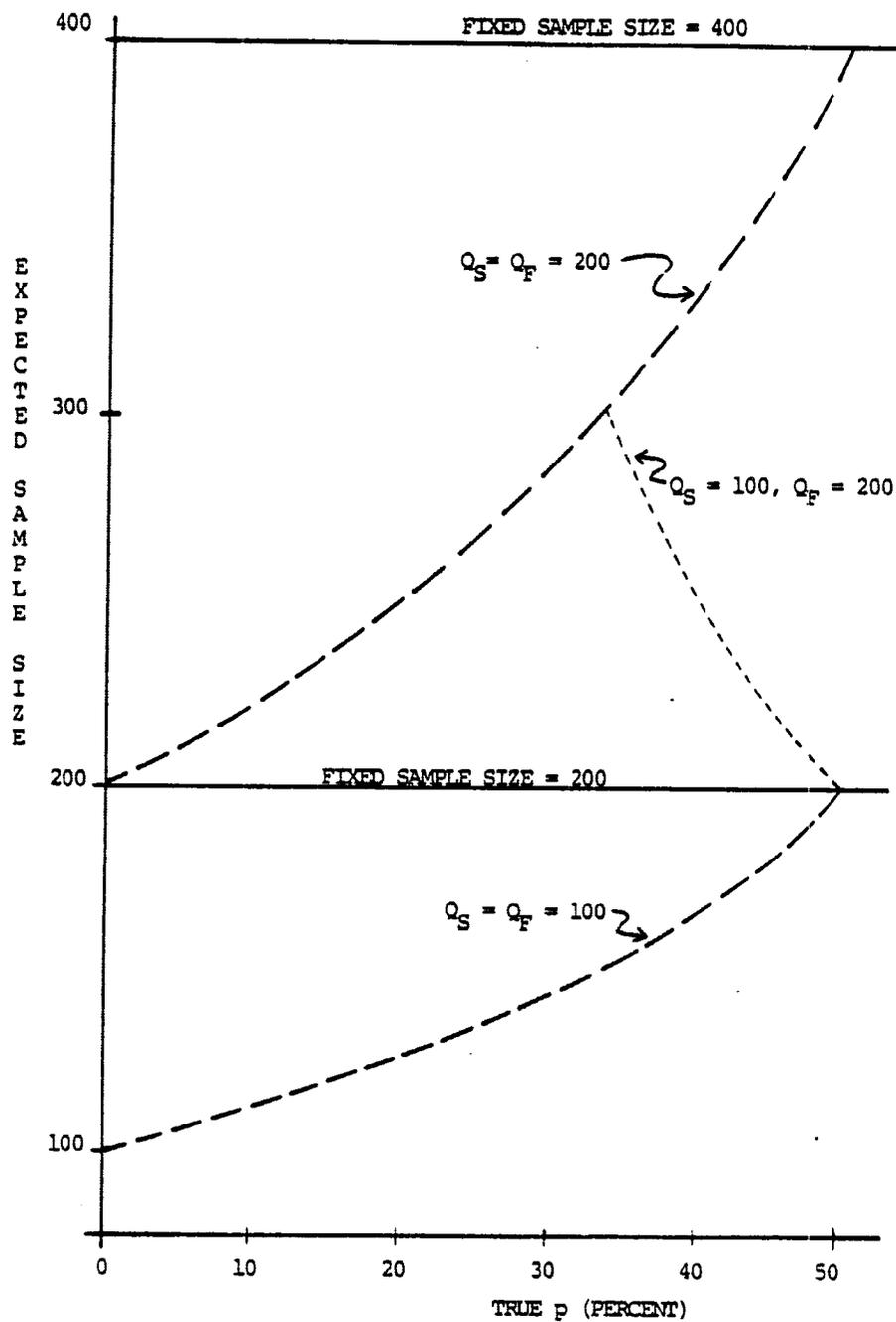
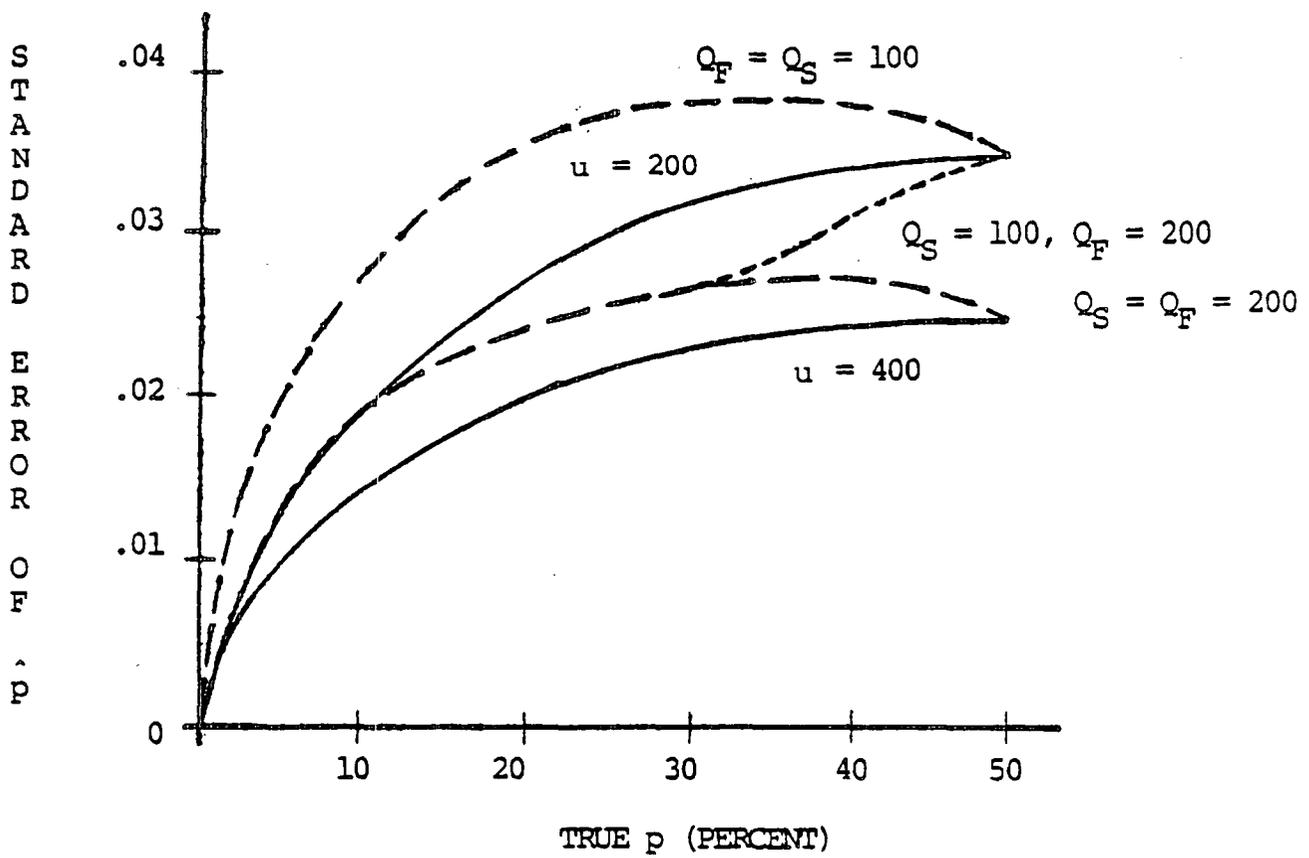


Figure 7.1 (continued)



$$P(t_S, t_F) = \begin{cases} \binom{Q_S + t_F - 1}{t_F} p_S^{Q_S} (1-p_S)^{t_F} & \text{if } t_S = Q_S \text{ and } t_F < Q_F \\ \binom{Q_F + t_S - 1}{t_S} p_S^{t_S} (1-p_S)^{Q_F} & \text{if } t_S < Q_S \text{ and } t_F = Q_F \end{cases} \quad (7.27)$$

Five different scenarios are depicted in Figure 7.1, including two fixed sample size procedures and three sum-quota procedures, two with  $Q_S = Q_F$  and one with  $Q_F = 2Q_S$ . The fixed sample sizes have been fixed at  $u = Q_S + Q_F$ , the maximum possible sample size,  $t_S + t_F \leq Q_S + Q_F = u$ , of the sum-quota procedure, so the standard error  $\sqrt{p_S(1-p_S)/u}$  of the fixed sample size estimator  $t_S/u$  constitutes a lower bound on the standard error of the sum-quota estimator while  $Q_S + Q_F$  constitutes an upper bound on the expected sample size of the sum-quota procedure.

In the case  $Q_S = Q_F$  the standard error of  $\hat{p}_S$  is largest when the two types are present in the ratio 1 to 2 (or 2 to 1), i.e., when either  $p_S = 1/3$  or  $p_S = 2/3$  (because the graph is symmetric about  $p_S = 1/2$ , only the left half of the graph is shown in this figure). The standard error in this case is

$$\text{S.E.}(\hat{p}_S) = \sqrt{p_S(1-p_S)^2/Q_F} \quad \text{for } p_S < \frac{1}{2} \quad (7.28)$$

which is seen to have a maximum at  $p_S = 1/3$ . Expected sample size increases from  $Q_F$  at  $p_S = 0$  to  $Q_S + Q_F$  at  $p_S = 1/2$  (and then decreases to  $Q_S = Q_F$  at  $p_S = 1$ ).

Prior information (from previous samples) can be exploited in choosing  $Q_S$  and  $Q_F$ . If prior indications are that  $p_S < 1/3$  then the choice  $Q_F = 2Q_S$  is appropriate.

## 8. ENTRAINMENT ESTIMATION UNDER NONCONTINUOUS SAMPLING PLANS

### 8.0 INTRODUCTION

The decision not to sample the discharge flow on some days in the entrainment season incurs a loss of accuracy in the estimates of entrainment abundance. This choice should not be made without careful consideration of the consequences, because the failure to collect data at certain times is irrevokable. If noncontinuous sampling is judged to be preferable because of cost and other practical limitations, care should be taken to choose a level and pattern of daily sampling that will minimize the concomitant information loss.

This chapter treats the analysis of noncontinuous sampling plans. Plans of this type present more difficulties than continuous sampling plans. To obtain unbiased entrainment estimates from noncontinuous sampling, more assumptions are needed than for continuous sampling.

### 8.1 PRELIMINARY CONSIDERATIONS AND AN EXAMPLE

Any departure from continuous sampling at a constant rate may be viewed as adding an amount of random error or "noise" to the estimation process. This decreases the accuracy of estimation. Changes of any kind in the sampling design may be compared to a standard consisting of a fixed level of continuous sampling effort.

Introducing intermittent rather than continuous sampling decreases the accuracy of estimation. It will be shown later that when sampling is intermittent, the estimated variance of  $\hat{T}$ , the estimated seasonal entrainment, increases by a factor of at least  $m/h$ , where  $m$  is the number of days in the entrainment season and  $h$  is the number of sampling days, over the estimated variance of  $\hat{T}$  under continuous sampling. The key issues are:

- (1) to quantify the bias in estimates of entrainment abundance and to identify conditions and assumptions that result in unbiased estimates,
- (2) to quantify the increase in the variance of estimated entrainment abundance, allowing the comparison of various patterns of missing p.s.u.'s, and
- (3) to determine how to arrange the days on which sampling is omitted so as to minimize the problems of bias and increased variance for estimates of entrainment abundance.

To demonstrate the application of these concepts, we now consider a hypothetical example. Its conditions represent an oversimplification of

actual entrainment conditions at the three power plants; nevertheless, the example provides an informative view of the difficulties involved.

Consider a sampling season of  $m$  days, on each of which we can estimate the entrainment  $T(j)$ ,  $j=1, \dots, m$ , by an unbiased estimator  $\hat{T}(j)$  with variance  $\sigma^2$ . Continuous sampling would result in an unbiased estimator of the total entrainment  $T = \sum T(j)$  given by

$$\hat{T} = \sum_{j=1}^m \hat{T}(j), \quad (8.1)$$

which has variance of  $m\sigma^2$ .

Now assume that  $r$  out of the  $m$  days are omitted from the sampling schedule, and that these occur in groups of  $g$  consecutive days. For example, if  $r = 24$  and  $g = 6$ , then four gaps of six consecutive days occur within the sampling season. To estimate  $T(j)$  for days on which no sampling occurs, linear interpolation is used. For instance, if days 1 and 5 are sampled but days 2, 3, and 4 are not, then

$$\begin{aligned} \hat{T}(2) &= 3\hat{T}(1)/4 + \hat{T}(5)/4, \\ \hat{T}(3) &= 2\hat{T}(1)/4 + 2\hat{T}(5)/4, \\ \hat{T}(4) &= \hat{T}(1)/4 + 3\hat{T}(5)/4. \end{aligned} \quad (8.2)$$

We now make

Assumption 8.1. During each gap in the sampling process, there is a linear trend in the daily entrainment  $T(j)$ . In other words, if day  $k$  is sampled and is then followed by a gap of  $g$  days, making day  $k+g+1$  the next day in the season on which sampling occurs, then the change in daily entrainment from one day to the next is the same throughout this time period:

$$T(k+1) - T(k) = T(k+2) - T(k+1) = \dots = T(k+g+1) - T(k+g). \quad (8.3)$$

An equivalent way of expressing this relationship is

$$T(k+j) = T(k) + \frac{j}{g+1}[T(k+g+1) - T(k)] \text{ for } j=0,1,\dots,g,g+1. \quad (8.4)$$

We also assume that there are at least two consecutive sampled days between any pair of gaps. (This is solely for convenience; if only one sampled day were to separate a pair of sampling gaps, then an even greater loss of precision would result than the loss when gaps are separated by at least two sampled days. The computations are easily done, but would complicate the example now being presented.)

Let  $\hat{T}(r,g)$  denote the estimator  $\hat{T}$  when  $r$  days are omitted from the sampling schedule in groups of  $g$  consecutive days. That is,  $\hat{T}(r,g)$  is calculated from (8.1), where linear interpolation is employed to calculate  $\hat{T}(j)$  for each unsampled day, using the closest sampled days before and after day  $j$ . When Assumption 8.1 holds, it is routine to show that  $\hat{T}(r,g)$  is unbiased and that

$$\text{Var}[\hat{T}(r,g)] = [m + r(g + 2)/2]\sigma^2. \quad (8.5)$$

Thus each single day missed adds an extra  $1.5\sigma^2$  to the variance of  $\hat{T}$ , each weekend or other two-day gap adds an extra  $4\sigma^2$ , and each six-day gap adds  $24\sigma^2$ . Clearly several small gaps are preferable to a single large one. In fact, if the total number of missed days  $r$  is fixed in advance, it is an optimal policy to arrange that all gaps should be one day in length. The effects of various combinations of  $r$  and  $g$  when  $m = 80$  are shown in Table 8.1. When  $r$  is fixed, notice the heavy penalty for increasing the value of  $g$ .

Without Assumption 8.1, the estimator  $\hat{T}(r,g)$  is not unbiased. No unbiased estimator of  $T$  can be found without either (i) an assumption about how daily entrainment  $T(j)$  behaves on days when no sampling is done, such as Assumption 8.1, or (ii) random selection of the days on which sampling occurs. If the sampling schedule is determined systematically, rather than randomly, and we do not know the relationship between  $T(j)$  on unsampled days and  $T(j)$  on sampled days, then we have no basis on which to calculate estimates  $\hat{T}(j)$  for unsampled days. In a sampling gap of six days, or even two or one, there could be a sharp spike, an "inverted spike" (a drop to near zero followed by a return to higher levels), or some other pattern substantially different from daily entrainment  $T(j)$  immediately before and after the gap; neither linear interpolation nor any other method can be expected to perform satisfactorily over the range of possibilities.

Table 8.1

Variance of  $\hat{T}(r,g)$  when  $m = 80$

$m$  = number of days in sampling season

$g$  = length of gap = number of consecutive days in each gap

$r$  = total number of days omitted from sampling schedule

Variances are written as multiples of  $\sigma^2$ : table entry 80 means  $80\sigma^2$ , etc.

		<u>g</u>					
		1	2	3	4	5	6
	0	80	80	80	80	80	80
	12	98	104	110	116	122	128
r	24	116	128	140	152	164	176
	36	134	152	170	188	206	224
	48	152	176	200	224	248	272

## 8.2 ENTRAINMENT ESTIMATION UNDER NONCONTINUOUS SAMPLING

The example of Section 8.1 can be generalized to the entrainment sampling situation. Consider an entrainment season of  $m$  days, of which  $h$  are sampling days. A sampling day is defined to be a day on which samples are drawn from the discharge channel during 24 consecutive one-hour p.s.u.'s. No sampling takes place on the remaining  $m - h$  days of the entrainment season. These unsampled days may form gaps of several different lengths.

Let  $d_j$  denote the day of the season on which the  $j^{\text{th}}$  sampling day occurred, for example,  $d_2 = 5$  if the second day of sampling was on day 5 of the season. On each of the  $h$  sampling days, the daily entrainment  $T(d_j)$ ,  $j=1, \dots, h$ , can be estimated by

$$\hat{T}(d_j) = \sum_{i=1}^{24} (V_i/v_i)X_i = \sum_{i=1}^{24} R_i X_i \quad (8.6)$$

where  $i$  indexes the 24 one-hour p.s.u.'s in the  $j^{\text{th}}$  sampling day and  $V_i$ ,  $v_i$ ,  $X_i$ , and  $R_i$  are as defined in Chapter 5. The estimate  $\hat{T}(d_j)$  is unbiased for the true daily entrainment  $T(d_j)$ . Using the continuous sampling methods of Chapter 5, the estimated variance of  $\hat{T}(d_j)$  can be found to be

$$\hat{\text{Var}}(\hat{T}(d_j)) = \sum_{i=1}^{24} R_i^2 X_i (1 + \hat{c}^2 X_i) / (1 + \hat{c}^2) \quad (8.7)$$

if an estimate of  $c^2$  can be obtained from replicate sampling. Otherwise take  $c^2 = 1$ , giving

$$\hat{\text{Var}}(\hat{T}(d_j)) = \sum_{i=1}^{24} R_i^2 X_i (1 + X_i) / 2. \quad (8.8)$$

For the  $m - h$  days on which sampling does not occur, linear interpolation between the closest preceding day and the closest succeeding day on which sampling is performed is used to estimate  $T(j)$ . If day  $k$  is sampled and is followed by a gap of  $g$  days, making day  $k+g+1$  the next sampling day, this interpolation gives

$$\hat{T}(k+j) = \hat{T}(k) + \frac{j}{g+1} [\hat{T}(k+g+1) - \hat{T}(k)] \text{ for } j=1, \dots, g. \quad (8.9)$$

We now give formulas for estimating the total seasonal entrainment  $T$ . Let  $j=1, \dots, h$  index the  $h$  sampling days, so  $j=3$  represents the third day on which sampling took place, regardless of which day in the season this

was. Define for  $j=1, \dots, h$

$d_j$  = the day of the season on which the  $j^{\text{th}}$  sampling day occurred,

$$w_j = \begin{cases} (1 + d_2 - d_1)/2 & \text{for } j=1 \\ (d_{j+1} - d_{j-1})/2 & \text{for } j=2, \dots, h-1 \\ (1 + d_h - d_{h-1})/2 & \text{for } j=h. \end{cases} \quad (8.10)$$

For example, if days 1, 8, 15, 16, and 18 of the season are the first five sampling days, then  $d_1=1$ ,  $d_2=8$ ,  $d_3=15$ ,  $d_4=16$ , and  $d_5=18$ . The weights  $w_j$  are given by

$$\begin{aligned} w_1 &= (1 + 8 - 1)/2 = 4, \\ w_2 &= (15 - 1)/2 = 7, \\ w_3 &= (16 - 8)/2 = 4, \\ w_4 &= (18 - 15)/2 = 1.5, \end{aligned} \quad (8.11)$$

and so on. Then formula (8.1) for  $\hat{T}$  can be rewritten in an equivalent form making explicit use of only the  $h$  sampling days, by using (8.9) to estimate the daily entrainment for each unsampled day:

$$\hat{T} = \sum_{j=1}^h w_j \hat{T}(d_j). \quad (8.12)$$

The mean, variance, and estimated variance of this estimator are

$$E(\hat{T}) = \sum_{j=1}^h w_j E(\hat{T}(d_j)) = \sum_{j=1}^h w_j T(d_j), \quad (8.13)$$

$$\text{Var}(\hat{T}) = \sum_{j=1}^h w_j^2 \text{Var}(\hat{T}(d_j)), \quad (8.14)$$

$$\hat{\text{Var}}(\hat{T}) = \sum_{j=1}^h w_j^2 \hat{\text{Var}}(\hat{T}(d_j)). \quad (8.15)$$

The estimator  $\hat{T}$  of (8.12) is biased except under special circumstances. It is appropriate to condition all calculations on the dates when entrainment sampling occurred, since these are chosen systematically in order to sample when entrainment levels are expected to be high. Conditional on these dates, the estimator  $\hat{T}$  of (8.12) cannot be guaranteed to be unbiased. Unbiasedness would require

$$E(\hat{T}) - T = E(\hat{T}) - [T(1) + T(2) + \dots + T(m)] = 0, \quad (8.16)$$

where  $E(\hat{T})$  is given by the right-hand side of (8.13). This is because the linear interpolation that led to the weights  $w_j$  in (8.12) will unavoidably miss entrainment spikes or other departures from linearity occurring during sampling gaps.

The estimator  $\hat{T}$  of (8.12), however, is unbiased (conditional on the sampling dates) when Assumption 8.1 of Section 8.1 holds, that is, when there is a linear trend in daily entrainment during each sampling gap. The bias in  $\hat{T}$  will be small if Assumption 8.1 is approximately true, that is, if actual daily entrainment in each sampling gap is nearly linear. This might be quantified as

Assumption 8.2. Whenever sampling on day  $k$  of the entrainment season is followed by a sampling gap of  $g$  unsampled days, the daily entrainment levels satisfy

$$|T(k+j) - \{T(k) + \frac{j}{g+1}[T(k+g+1) - T(k)]\}| \leq U \text{ for } j=1, \dots, g. \quad (8.17)$$

Under this assumption, the magnitude of the bias in  $\hat{T}$  of (8.12) is bounded by

$$|E(\hat{T}) - T| = \left| \sum_{k=1}^m E(\hat{T}(k)) - \sum_{k=1}^m T(k) \right| \leq (m-h)U, \quad (8.18)$$

where  $m-h$  is the number of days that are unsampled.

We now demonstrate that noncontinuous sampling results in a larger value of  $\text{Var}(\hat{T})$  than continuous sampling with the same level of sampling effort, that is, with the same total volume sampled over the course of the entrainment season. To simplify the presentation, we treat the noncontinuous sampling plan in which sampling occurs every second day throughout the season. This plan's regularity minimizes the mathematical complexity of the reasoning, but the same reasoning with more elaborate sets of constants applies to any noncontinuous sampling plan, e.g., sampling every third day, etc.

Let the subscript "ij" denote p.s.u.  $i$  on day  $j$ , where  $i=1, \dots, 24$  and  $j=1, \dots, m$ . (The choice of 24 p.s.u.'s is made for concreteness, but 8 or some other number of p.s.u.'s would be equally acceptable.) Assume that  $R_{ij}$  and  $\mu_{ij}$  change slowly from day to day, that is,  $R_{ij}$  is approximately equal to  $R_{i,j+1}$  for all  $i$  and  $j$  and the same holds for  $\mu_{ij}$ . This assumption of slow change makes conditions as favorable as possible to the use of noncontinuous sampling; if the day-to-day change is not slow, then variability among days is greater (than for slow change), which makes noncontinuous sampling more questionable because of its use of sampled days to estimate entrainment for adjacent unsampled days.

Let ' (prime) denote quantities associated with the noncontinuous, every-second-day sampling plan, and the absence of a prime denote quantities associated with the continuous sampling plan. We are comparing plans with the same total volume sampled over the season, which means that daily sample volume under noncontinuous sampling must be twice as large as under continuous sampling, because sampling occurs on only half as many days. Therefore  $v'_{ij} = 2v_{ij}$  for every  $i$  and  $j$  (where  $j$  denotes a day on which the noncontinuous sampling plan operates), and consequently

$$R'_{ij} = V_{ij}/v'_{ij} \approx V_{ij}/(2v_{ij}) = R_{ij}/2$$

$$\mu'_{ij} = \lambda_{ij}v'_{ij} \approx \lambda_{ij}(2v_{ij}) = 2\mu_{ij} \quad (8.19)$$

In every-second-day sampling, the days of sampling are

$$d_1 = 1, d_2 = 3, d_3 = 5, \dots, d_h = 2h - 1 = m, \quad (8.20)$$

and (8.10) gives

$$w_1 = w_h = 3/2, w_2 = w_3 = w_4 = \dots = w_{h-1} = 2. \quad (8.21)$$

For continuous sampling, using (5.65) and  $\text{Var}(\hat{T}(j)) \approx \text{Var}(\hat{T}(j+1))$ ,

$$\begin{aligned}\text{Var}(\hat{T}(j)) &= \sum_{i=1}^{24} R_{ij}^2 (\mu_{ij} + c^2 \mu_{ij}^2) \\ \text{Var}(\hat{T}) &= \text{Var}(\hat{T}(1)) + \text{Var}(\hat{T}(2)) + \dots + \text{Var}(\hat{T}(m)) \\ &\approx 2\text{Var}(\hat{T}(1)) + 2\text{Var}(\hat{T}(3)) + 2\text{Var}(\hat{T}(5)) + \dots + 2\text{Var}(\hat{T}(m)) \\ &= \sum_j \sum_i R_{ij}^2 (2\mu_{ij} + 2c^2 \mu_{ij}^2)\end{aligned}\quad (8.22)$$

where  $i$  ranges from 1 to 24 and  $j$  ranges over the odd-numbered days 1, 3, 5, ... of the season. (The coefficients of  $\text{Var}(\hat{T}(1))$  and  $\text{Var}(\hat{T}(m))$  should be made 1.5, rather than 2, for a better approximation; this could be done but will not, to simplify the presentation.)

For noncontinuous sampling, applying (5.65) to each day gives

$$\begin{aligned}\text{Var}'(\hat{T}(j)) &= \sum_{i=1}^{24} R_{ij}^2 (\mu_{ij} + c^2 \mu_{ij}^2) \\ &= \sum_{i=1}^{24} (R_{ij}/2)^2 (2\mu_{ij} + 4c^2 \mu_{ij}^2) \\ &= \sum_{i=1}^{24} R_{ij}^2 (\frac{1}{2}\mu_{ij} + c^2 \mu_{ij}^2);\end{aligned}\quad (8.23)$$

Then (8.14) gives (with  $j$  used rather than  $d_j$  to denote the  $j^{\text{th}}$  sampling day, as in (8.23)).

$$\begin{aligned}
\text{Var}'(\hat{T}) &= \sum_{j=1}^h w_j^2 \text{Var}'(\hat{T}(j)) \\
&\approx \sum_{j=1}^h 4\text{Var}'(\hat{T}(j)) \\
&= \sum_{ji} \Sigma R_{ij} (2\mu_{ij} + 4c^2 \mu_{ij}^2). \tag{8.24}
\end{aligned}$$

Comparing (8.22) and (8.24), we see that

$$\text{Var}(\hat{T}) < \text{Var}'(\hat{T}) < 2\text{Var}(\hat{T}). \tag{8.25}$$

An analogous argument involving  $X_{ij}$ 's instead of  $\mu_{ij}$ 's shows that

$$\hat{\text{Var}}(\hat{T}) < \hat{\text{Var}}'(\hat{T}) < 2\hat{\text{Var}}(\hat{T}). \tag{8.26}$$

If  $\mu_{ij} \ll c^2 \mu_{ij}^2$  for some one-hour p.s.u. organism counts, these p.s.u.'s will be the ones with the largest values of  $\mu_{ij}$ , because the discrepancy between  $\mu_{ij}$  and  $c^2 \mu_{ij}^2$  (measured by the ratio  $c^2 \mu_{ij}^2 / \mu_{ij} = c^2 \mu_{ij}$ ) increases as  $\mu_{ij}$  increases. When the summands in (8.22) and (8.24) make large contributions to the variances in these equations, which happens when  $\mu_{ij}$  is large, these contributions come mostly from the term  $c^2 \mu_{ij}^2$  and hardly at all from the term  $\mu_{ij}$ . Because  $c^2 \mu_{ij}^2$  is multiplied by 4 in (8.24) and by 2 in (8.22), this implies that  $\text{Var}'(\hat{T}) \approx 2\text{Var}(\hat{T})$ ; this approximation is excellent when the organism counts are large enough to make the ratio  $c^2 \mu_{ij}^2 / \mu_{ij}$  much greater than 1 for a substantial number of p.s.u.'s. This will be the case unless the sample organism counts are almost uniformly very low. Such counts indicate low underlying means, hence (Cassie, quoted in Section 4.2.1) a low value of  $c^2$  and Poisson-like behavior. Even in this case, the inequalities (8.25) and (8.26) hold.

There is, in fact, another term contributing to the variance of  $\hat{T}$  when sampling is noncontinuous. Consider the every-second-day sampling plan, and imagine letting the sample volume  $v_{ij}$  increase to its limit,  $V_{ij}$ , so  $\hat{T}_j = T_j$  for each sampling day; that is, our sample consists of the entire discharge flow on every second day, so entrainment is known exactly on these days. There would still be error in our estimate of the seasonal total  $T$ , however. This systematic sampling of days amounts to partitioning the season into two equal-sized clusters of days and randomly selecting one of the two clusters for complete ( $v_{ij} = V_{ij}$ ) enumeration. The estimator of  $T$  is then (essentially) 2 times the enumerated entrainment in the selected half-season, and its standard error is essentially the absolute difference in entrainment between the two halves (the odd-days half and the even-days half). Thus, if  $T_1$  and  $T_2$  are the odd-days total entrainment and the even-days total entrainment, so  $T = T_1 + T_2$ , then the sampling distribution of  $\hat{T}$  would be

$$P(\hat{T} = 2T_1) = P(\hat{T} = 2T_2) = .5. \quad (8.27)$$

Therefore the variance of the estimator of  $T$  is

$$\begin{aligned} \text{Var}(\hat{T}) &= .5(2T_1 - T)^2 + .5(2T_2 - T)^2 = .5(T_1 - T_2)^2 + .5(T_2 - T_1)^2 \\ &= (T_1 - T_2)^2. \end{aligned} \quad (8.28)$$

The standard error of the estimate is (essentially) the absolute difference in entrainment between the two half-seasons,

$$\text{S.E.}(\hat{T}) = |T_1 - T_2|. \quad (8.29)$$

A lower bound on the expected magnitude of this absolute difference can be calculated by assuming a Poisson model for daily entrainment. If each of  $T_1$  and  $T_2$  is a sum of Poisson entrainments, then the expected value of their absolute difference is

$$E(|T_1 - T_2| | T) = (2/\pi)^{1/2} (T/4)^{1/2}, \quad (8.30)$$

which is derived by using the normal approximation to the binomial distribution of  $T_1$ ,

$$P(T_1|T) = \frac{T!}{T_1!T_2!} (1/2)^{T_1}(1/2)^{T_2}, \quad (8.31)$$

with  $E(T_1|T) = T/2$  and  $\text{Var}(T_1|T) = T/4$ .

This contribution to the variance of  $\hat{T}$  is more complicated to calculate for other noncontinuous sampling plans, which lack the regularity that made the mathematics (relatively) easy for the situation of every-second-day sampling. It will suffice here to point out that the addition of the term treated in (8.28) and (8.30) to the variance of  $\hat{T}$  under noncontinuous sampling gives continuous sampling an even larger advantage over noncontinuous sampling. The formula (8.15), which does not include this component of variance, will be used to estimate the variance of  $\hat{T}$  under noncontinuous sampling. Further analytic work is needed to extend the treatment of this additional component of variance from the every-second-day sampling plan to more general noncontinuous sampling plans.

A more detailed mathematical treatment shows that the variance inequality (8.25) generalizes to

$$\text{Var}(\hat{T}) < \text{Var}'(\hat{T}) < (m/h)\text{Var}(\hat{T}) \quad (8.32)$$

for noncontinuous sampling plans with gaps of no more than one day.

$\text{Var}'(\hat{T})$  is close to  $(m/h)\text{Var}(\hat{T})$  unless the organism counts are almost uniformly low. For noncontinuous sampling plans with gaps longer than one day, both the upper bound and the typical behavior of  $\text{Var}'(\hat{T})$  are even larger than in (8.32). Thus, noting that  $m/h$  is the reciprocal of the proportion of days sampled, we see that the number of experimental units (days) is much more influential than the size of each unit's sample; cutting the number of experimental units in half makes the variance almost twice as great, even when each experimental unit doubles in size (volume sampled).

A comparison of two sampling plans, one continuous and the other noncontinuous, expending the same amount of sampling effort as measured by the total volume of all samples gathered over the whole season, reveals that the noncontinuous plan loses on both important criteria. First, the noncontinuous plan gives an estimate of  $T$  that is biased, while the continuous plan provides an unbiased estimate of  $T$ . Second, the variance of the noncontinuous plan's estimate of  $T$  is greater than the variance of the continuous plan's estimate of  $T$ .

A natural interpretation of the inequality (8.32) is based on partitioning the variability of entrainment into two components, a within-p.s.u.'s variance and an among-p.s.u.'s variance. Continuous sampling gives relatively precise information about among-p.s.u.'s variance, because no p.s.u. is unsampled, but spreading the sampling effort across all p.s.u.'s lowers the amount of information about within-p.s.u.'s variance that is obtained. In contrast to this, a noncontinuous sampling plan with the same sampling effort concentrates the sampling on only some of the p.s.u.'s, drawing a larger sample from each of these than under continuous sampling. This reduces the amount of information provided by the sampling plan about variance among p.s.u.'s, because some p.s.u.'s are not sampled, making their contents completely unknown. However, the volume of the sample drawn from each sampled p.s.u. is larger, giving more information about variance within p.s.u.'s. The superiority of continuous over noncontinuous sampling makes it clear that when a smaller number of p.s.u.'s are sampled, the increase in knowledge about within-p.s.u.'s variance is heavily outweighed by the greater loss of information about among-p.s.u.'s variance. An intuitive interpretation of these findings is that variance among p.s.u.'s is a much greater component of overall variability than variance within p.s.u.'s.

The variance and estimated variance of  $\hat{T}$  are given by (8.14) and (8.15). Large weights  $w_j$  will produce large terms in these equations, which will increase  $\text{Var}(\hat{T})$  and  $\hat{\text{Var}}(\hat{T})$ , except when  $w_j^2$  is multiplied by a small value of  $\text{Var}(\hat{T}(d_j))$  or  $\hat{\text{Var}}(\hat{T}(d_j))$ . Two points are relevant here: first, equation (8.10) makes it clear that a long sampling gap immediately before and/or after the  $j^{\text{th}}$  sampling day is what causes  $w_j$  to be large. Second, small values of  $\text{Var}(\hat{T}(d_j))$  and  $\hat{\text{Var}}(\hat{T}(d_j))$  occur in entrainment sampling when entrainment itself is very low, as the formulas for these quantities show.

The result of these considerations is that sampling gaps will affect the variance and estimated variance of  $\hat{T}$  only slightly, if at all, when these gaps occur during portions of the entrainment season known to have organism density of zero, or virtually zero. However, sampling gaps that occur when organism density is greater than zero will increase  $\text{Var}(\hat{T})$  and  $\hat{\text{Var}}(\hat{T})$ . Longer gaps will produce greater increases, all other things being equal, as in the example of Section 8.1.

This discussion of entrainment estimation under noncontinuous sampling plans can be summarized as follows:

- (1) In general, interpolation-based estimators of seasonal entrainment will be biased; noncontinuous sampling data do not provide any information about the degree and direction of bias present. Under Assumption 8.1, however, these estimators will be unbiased. If a suitable approximation to this assumption, like Assumption 8.2, seems more realistic, then bounds on the magnitude of bias can be obtained.

- (2) The variance and estimated variance of estimated entrainment abundance, given by (8.14) and (8.15), are determined by the weight and the variance of the daily entrainment estimate for each day on which sampling occurs. Large weights are attached to sampling days that are adjacent to long sampling gaps. The variances of daily entrainment estimates increase with organism densities, which are closely connected to observed organism counts. Sampling gaps during the entrainment season will not increase the variance and estimated variance of  $\hat{T}$  if organism density is known to be zero throughout these gaps. On the other hand, the increase in variance can be very great if gaps occur in high-density periods.
- (3) If gaps are necessary, they should be concentrated in portions of the entrainment season in which the density of organisms is zero, or as close to it as possible. When density is zero, Assumption 8.1 is satisfied, so  $\hat{T}$  is unbiased; and daily entrainment estimates have variance equal to zero, so the variance of  $\hat{T}$  is not increased.
- (4) For much of the entrainment season, it is not possible to be certain in advance (when sampling schedules are being decided upon) whether a given day's organism density will be high, moderate, or low. We may know, for example, only that an entrainment spike of 7 to 10 days in duration will probably occur at some point between mid-June and mid-July. During such periods, it is best to have no sampling gaps. If they are necessary, sampling gaps should be as infrequent and as short as possible. Many short gaps are preferable to a few long ones containing the same total number of days; gaps should be one day in length, two days if this is unavoidable, but no longer. Short gaps make Assumption 8.1 or a suitable approximation to it more tenable, reducing or eliminating bias in entrainment estimates, and avoid the large weights that would inflate the variances of these estimates.

The simultaneous estimation of entrainment abundance for many species and life stages of ichthyoplankton calls for a conservative approach to the scheduling of gaps. Long sampling gaps during periods of nonzero organism density introduce bias that cannot be estimated without restrictive assumptions, and also increase the variance of  $\hat{T}$  much more than shorter gaps. The penalty for having long gaps when any important species-life stage combination is present will therefore be great.

### 8.3 NONCONTINUOUS SAMPLING AT ROSETON

The methods of Section 8.2 will now be applied to 1983 data from the Roseton Generating Station. Tables 8.2 and 8.3 present daily and seasonal entrainment estimates for striped bass yolk-sac larvae and juveniles. Sampling was performed on 35 days during the 1983 season: May 3, 10; daily from May 17 to 27, May 31 to June 3, June 7 to 10, June 14 to 22; June 28, July 5, 12, 19, and 26. For each of these dates, the chronological day of the

Table 8.2  
Estimates of Daily Entrainment of Striped Bass  
Yolk-Sac Larvae and Juveniles, Roseton 1983

OBS	ST_DATE	J	D	W	TLAR1	V1LAR1	V2LAR1	TJUV1	V1JUV1	V2JUV1
1	03MAY83	1	1	4.0	0	0.00000E+00	0.00000E+00	0	0.00000E+00	0.00000E+00
2	10MAY83	2	8	7.0	7,438	1.99291E+07	1.21095E+07	0	0.00000E+00	0.00000E+00
3	17MAY83	3	15	4.0	2,089	4.36307E+06	4.36307E+06	0	0.00000E+00	0.00000E+00
4	18MAY83	4	16	1.0	2,022	4.09045E+06	4.09045E+06	0	0.00000E+00	0.00000E+00
5	19MAY83	5	17	1.0	0	0.00000E+00	0.00000E+00	0	0.00000E+00	0.00000E+00
6	20MAY83	6	18	1.0	0	0.00000E+00	0.00000E+00	0	0.00000E+00	0.00000E+00
7	21MAY83	7	19	1.0	0	0.00000E+00	0.00000E+00	0	0.00000E+00	0.00000E+00
8	22MAY83	8	20	1.0	0	0.00000E+00	0.00000E+00	0	0.00000E+00	0.00000E+00
9	23MAY83	9	21	1.0	1,415	2.00270E+06	2.00270E+06	0	0.00000E+00	0.00000E+00
10	24MAY83	10	22	1.0	0	0.00000E+00	0.00000E+00	0	0.00000E+00	0.00000E+00
11	25MAY83	11	23	1.0	8,540	1.21589E+07	1.21589E+07	0	0.00000E+00	0.00000E+00
12	26MAY83	12	24	1.0	2,738	3.74838E+06	3.74838E+06	0	0.00000E+00	0.00000E+00
13	27MAY83	13	25	2.5	4,193	5.86254E+06	5.86254E+06	0	0.00000E+00	0.00000E+00
14	31MAY83	14	29	2.5	5,592	7.81683E+06	7.81683E+06	0	0.00000E+00	0.00000E+00
15	01JUN83	15	30	1.0	2,719	3.69527E+06	3.69527E+06	0	0.00000E+00	0.00000E+00
16	02JUN83	16	31	1.0	5,506	9.42151E+06	7.79677E+06	0	0.00000E+00	0.00000E+00
17	03JUN83	17	32	2.5	16,627	3.65684E+07	2.46375E+07	0	0.00000E+00	0.00000E+00
18	07JUN83	18	36	2.5	8,413	1.18018E+07	1.18018E+07	0	0.00000E+00	0.00000E+00
19	08JUN83	19	37	1.0	23,510	1.08673E+08	4.14969E+07	0	0.00000E+00	0.00000E+00
20	09JUN83	20	38	1.0	3,483	4.85215E+06	4.85215E+06	0	0.00000E+00	0.00000E+00
21	10JUN83	21	39	2.5	15,184	3.06656E+07	2.21070E+07	0	0.00000E+00	0.00000E+00
22	14JUN83	22	43	2.5	29,749	7.99128E+07	4.49228E+07	0	0.00000E+00	0.00000E+00
23	15JUN83	23	44	1.0	11,177	4.50720E+07	1.90875E+07	0	0.00000E+00	0.00000E+00
24	16JUN83	24	45	1.0	74,548	3.13457E+08	1.29522E+08	0	0.00000E+00	0.00000E+00
25	17JUN83	25	46	1.0	41,485	1.54874E+08	6.88722E+07	0	0.00000E+00	0.00000E+00
26	18JUN83	26	47	1.0	64,083	3.75192E+08	1.21318E+08	0	0.00000E+00	0.00000E+00
27	19JUN83	27	48	1.0	63,927	8.61950E+08	1.79968E+08	0	0.00000E+00	0.00000E+00
28	20JUN83	28	49	1.0	134,258	2.74965E+09	4.83091E+08	0	0.00000E+00	0.00000E+00
29	21JUN83	29	50	1.0	150,108	1.15906E+09	4.42476E+08	0	0.00000E+00	0.00000E+00
30	22JUN83	30	51	3.5	143,961	1.89695E+09	5.09346E+08	0	0.00000E+00	0.00000E+00
31	28JUN83	31	57	6.5	14,294	4.55625E+07	3.54191E+07	0	0.00000E+00	0.00000E+00
32	05JUL83	32	64	7.0	0	0.00000E+00	0.00000E+00	2,315	5.35725E+06	5.35725E+06
33	12JUL83	33	71	7.0	0	0.00000E+00	0.00000E+00	9,121	2.60741E+07	2.14225E+07
34	19JUL83	34	78	7.0	0	0.00000E+00	0.00000E+00	6,857	1.56727E+07	1.56727E+07
35	26JUL83	35	85	4.0	0	0.00000E+00	0.00000E+00	0	0.00000E+00	0.00000E+00

°J, D, and W are  $j$ ,  $d_j$  and  $w_j$  of Section 8.2

°TLAR1 and TJUV1 are estimated daily entrainment of yolk-sac larvae and juveniles from (8.6)

°V1LAR1 and V1JUV1 are estimated variance of TLAR1 and TJUV1 from (8.8)

°V2LAR1 and V2JUV1 are estimated variance of TLAR1 and TJUV1 from (8.7) with  $c^2 = .0626$ .

Table 8.3

Estimates of Total Entrainment of Striped Bass  
Yolk-Sac Larvae and Juveniles, Roseton 1983:  
Variance and Standard Deviation for Selected Sampling Ratios

Yolk-Sac Larvae

Sampling Ratio	Estimated Season Entrainment	Variance from (8.4)	Std dev from (8.4)	Variance from (8.3)	Std dev from (8.3)
Actual	1,460,628	3.319644E+10	182,199	1.076977E+10	103,778
1,000	1,460,628	3.129446E+10	176,902	7.189979E+09	84,794
2,000	1,460,628	3.322113E+10	182,267	1.081624E+10	104,001
4,000	1,460,628	3.707446E+10	192,547	1.806875E+10	134,420
8,000	1,460,628	4.478113E+10	211,616	3.257378E+10	180,482

Juveniles

Sampling Ratio	Estimated Season Entrainment	Variance from (8.4)	Std dev from (8.4)	Variance from (8.3)	Std dev from (8.3)
Actual	128,049	2.308096E+09	48,043	2.080168E+09	45,609
1,000	128,049	1.731408E+09	41,610	9.947608E+08	31,540
2,000	128,049	2.179578E+09	46,686	1.838280E+09	42,875
4,000	128,049	3.075918E+09	55,461	3.525317E+09	59,374
8,000	128,049	4.868598E+09	69,775	6.899392E+09	83,063

season that was the  $j^{\text{th}}$  sampling day is listed in Table 8.2 as D, and the weight to be attached to this day in the estimate of seasonal entrainment is listed as W. (D and W are  $d_j$  and  $w_j$  of Section 8.2).

Each sampling day consisted of 24 consecutive one-hour p.s.u.'s, which began at 9:00 AM on the specified date and ended at 9:00 AM on the following date. For each sampling day, the 24 samples from these p.s.u.'s were used to compute an estimate of the day's entrainment,  $\hat{T}(d_j)$ , from formula (8.6) and two estimates of the variance of  $\hat{T}(d_j)$  from (8.7) and (8.8). These appear in Table 8.2 for striped bass yolk-sac larvae (TLAR1, V1LAR1 from (8.8), V2LAR1 from (8.7)) and juveniles (TJUV1, V1JUV1 from (8.8), V2JUV1 from (8.7)). The value  $\hat{c}^2 = .0626 = 1/15.97$  was used, based on 1982 replicate sampling data from Roseton (Table 4.8) for which  $\hat{K}^+ = 15.97$  was obtained for striped bass yolk-sac larvae. This will illustrate the variance reduction that results from using (8.7) rather than (8.8). Replicate data did not allow calculation of  $\hat{K}^+$  and thus  $\hat{c}^2$  for juveniles (see Section 4.3), so

$\hat{c}^2 = .0626$  was again used for illustrative purposes.

For yolk-sac larvae, the daily estimated variance V2LAR1 is often substantially lower than V1LAR1, in particular when the latter takes on its largest values on June 20, 21, and 22. For juveniles, the estimated daily entrainment and the estimates of its variance are positive only on the last three sampling dates, which all have weights of 7.0 because sampling at this stage of the season is on a weekly basis.

For yolk-sac larvae, the daily sampling information in Table 8.2 is good up to the June 20-22 entrainment peak, but is scanty after this peak. The lack of information about the pattern following the observed peak makes it possible that substantial bias is present in estimated seasonal entrainment, unless Assumption 8.1 or a similar assumption can be supported by other means. (These data provide no evidence for or against such an assumption.)

Bias in the seasonal estimate is even more serious for the juveniles. Sampling only once per week during their presence raises a strong possibility that the daily entrainment level peaked at a level markedly higher than was reached on any of the three days when juveniles were observed. This could have occurred during one of the six-day sampling gaps. Furthermore, the data tell us nothing about entrainment after the last sampled day, July 26. This is especially important because entrainment is high (relative to the maximum daily level) on July 19, so there may be some uncertainty concerning whether the drop to zero entrainment on July 26 signals the end of nonzero entrainment levels for the rest of the season.

Seasonal entrainment estimates appear in Table 8.3. In the rows with sampling ratio listed as Actual,  $\hat{T}$  and the two variance estimates for each life stage have been computed from Table 8.2 using (8.12) and (8.15), with  $V_1$  corresponding to (8.8) and  $V_2$  to (8.7). To examine the influence of the sampling ratio  $R_i = V_i/v_i$ , the estimation of  $T$  has been performed for constant sampling ratio of  $R_i = 1,000$  for every p.s.u.  $i$ . This was achieved by changing the sample volume  $v_i$  to  $V_i/1,000$  for each p.s.u. and adjusting the organism counts proportionally. This procedure has been repeated for sampling ratios of 2,000, 4,000, and 8,000.

Two aspects of the estimated variances in Table 8.3 should be mentioned. First, variances and standard deviations obtained from (8.7) are smaller than those obtained from (8.8). For yolk-sac larvae, the differences are sizeable; for instance, with the actual sampling ratios,

$$\text{Std dev (8.3)/Std dev (8.4)} = 103,778/182,199 = 57.0\%.$$

Second, variances and standard deviations increase with the sampling ratio, but not proportionally to the change in  $R_i$ . For example, for yolk-sac larvae, as  $R_i$  increases from 1,000 to 2,000, a change of 100% (which reduces the sample volume in each p.s.u. by 50%), the standard deviation from (8.7) increases only 22.7%, from 84,794 to 104,001. Computations of this kind can be used to evaluate the precision attained by adopting various sampling ratios.

The sampling plan at Roseton would be improved by eliminating sampling gaps of six days in length, except perhaps in those portions of the entrainment season when it can be documented that entrainment levels are virtually zero. This might be done by shifting from continuous sampling at mid-season (e.g., June 14 to 22) to gaps of one day, then possibly to gaps of two days. Longer gaps cannot be recommended except as a byproduct of sporadic sampling in portions of the entrainment season in which the density of organisms is zero or nearly zero (as noted near the end of Section 8.2). The cost in bias and variance of encountering entrainment above the near-zero level on a sampling day adjoining a long gap is very great.

A further possibility for the Roseton Generating Station, noted in Section 1.3, would be to supplement the sampling at Roseton by the concurrent and additional sampling that was done at Danskammer Point. Sampling data from Danskammer could be used in estimating the entrainment at Roseton during periods when sampling was not performed at Roseton. This approach has not been considered in this report.

## 9. SAMPLING DESIGN RECOMMENDATIONS FOR FUTURE ENTRAINMENT ABUNDANCE ESTIMATION

### 9.0 INTRODUCTION

This chapter draws on the material developed and presented in earlier chapters, using it to recommend sampling design features for future entrainment abundance estimation. General conclusions are presented. Specific features of recommended sampling designs are then treated.

### 9.1 CONCLUSIONS ON SAMPLING DESIGNS

It is useful to discuss briefly the characteristics of the sampling designs used at Bowline Point, Indian Point, and Roseton in recent years. In the first design, a p.s.u. corresponds to a three-hour period and sampling is continuous, i.e., sampling occurs over 8 p.s.u.'s per day, 7 days per week. In the second design, a p.s.u. corresponds to a one-hour period and a full day of p.s.u.'s is sampled on systematically chosen days during the entrainment season.

A basic issue concerning these designs is that the variability of entrainment estimates has not been properly addressed. More specifically, these designs provide point estimators of entrainment abundance and entrainment reduction, but the estimated variances of these estimators are needed. These estimated variances have until now been calculated under the assumption of Poisson variation in the observed data from the discharge channel, which gives unrealistically short confidence intervals when the data are not Poisson. In some cases, even this has not been done, and the subject of estimated variances of entrainment estimators has gone unmentioned. This is a key point, because a comparison of the designs recommended here to other designs used previously can be judged in either of two ways:

- (i) in terms of the estimated variances of estimators obtained in connection with previous designs, or
- (ii) after incorporating methodology from this study to aid in the estimation of variances from data collected under previous designs.

The first of these comparisons is easy: in entrainment studies prior to this report, estimated variances and standard errors of estimates have been calculated either not at all or under the often unrealistic Poisson assumption. By this standard, the improvement is unbounded.

The second comparison requires more work. Calculating for entrainment estimators from earlier studies the estimated variances developed here

makes those studies useful in a way they had not been before, because the estimated variance or standard error of any estimator provides essential information about the precision of that estimator. The estimators of entrainment we have dealt with, both the standard fixed-sample-volume estimator and the adaptive, more efficient sum-quota sampling estimator, are either exactly unbiased or very nearly so. Thus the criterion of low bias is satisfied by these estimators, making low variance and low coefficient of variation the only remaining issues.

Our conclusions cover six specific topics bearing on the sampling design: continuity of sampling, equality of the sampling fractions  $v_i/V_i$ , the sample volume's relationship to confidence interval width, replicate sampling, sample duration, and sum-quota versus fixed-volume sampling. These will now be discussed.

1. Continuous sampling, or as nearly continuous sampling as possible, is an important property of a good sampling design. The reasons for this were discussed in Chapter 8. Electing not to sample on some days can lead, except under assumptions that may be considered unrealistic, to bias in  $\hat{T}$ , the estimator of total entrainment abundance. It also introduces a major source of additional variability to entrainment estimation. Scheduling a gap of  $g$  days in the sampling plan increases the estimated variance of  $\hat{T}$  by a little more than  $g^2/2$ ; thus, if there must be gaps in the sampling, many small gaps (one day apiece, perhaps two days if this is unavoidable) are much better than a few large gaps. There is one exception to this guideline, which has to do with the fact that some species-life stage combinations are more important than others for us to estimate accurately. It may happen that all of these important species-life stage combinations are known to have densities that are zero (or virtually zero) in a part of the entrainment season. For instance, we may know in advance from long biological experience that all of the combinations we are strongly concerned about will register zero entrainment during the first four weeks of the entrainment season. In this case, only sporadic sampling would be necessary during this period. Naturally, if our "knowledge" turns out to be incorrect and a sizeable level of entrainment takes place during one or more of our sampling gaps, then we will fail to observe the evidence of this entrainment peak. This danger is present whenever gaps in the sampling schedule exist; it can be avoided only by ensuring that we have full coverage of the entrainment season except for any time interval during which we are absolutely certain no entrainment of any consequence will occur.

2. Equality or near equality of the sampling fractions  $v_i/V_i$  across all p.s.u.'s provides important advantages, which were discussed in Section 5.9. Inequality of these sampling fractions can lead to inaccuracy in the computation of confidence intervals, that is, to confidence intervals whose nominal and true confidence levels differ substantially. This is a critical issue, as the quality of the confidence intervals obtained from entrainment abundance sampling is a primary consideration; a design that produces confidence intervals we must be cautious in believing cannot be highly regarded.

Substantial inequality among the sampling fractions  $v_i/V_i$  is necessary to erode seriously the accuracy of entrainment confidence intervals. Small to moderate inequalities, e.g., sampling ratios differing among p.s.u.'s by a factor of two, will cause only a minor loss of accuracy. As the imbalance in  $v_i/V_i$  increases, though, the accuracy of entrainment confidence intervals decreases; for example, sampling ratios differing among p.s.u.'s by a factor of six can cause major degradation of confidence interval accuracy. Consequently, if equality of the sampling fractions cannot be accomplished exactly, it should be arranged to the greatest extent possible.

The equality of sampling fractions is for all p.s.u.'s at a given plant, not necessarily from one plant to another. To make the sampling fractions in all p.s.u.'s equal, which means making the sample volume proportional to the p.s.u. volume (plant discharge flow volume during the p.s.u.), the sampling gear intake rate could be regulated by a servomechanism with feedback linked to discharge flow rate. The complexity of entrainment sampling prevents any attempt to improve the accuracy of estimation by using sampling fractions that differ from one p.s.u. to another. This complexity encompasses the need to obtain entrainment estimates for many different species and life stages; the relative costs of collecting, sorting, and identifying samples of various sizes and compositions; the year-to-year variation in entrainment patterns; and other similar factors. A related consideration is that  $v_i/V_i$  must be the same for any collection of samples that we wish to combine into a single composite sample.

3. The total volume of water sampled,  $\sum v_i$ , must be adequate to yield acceptable confidence intervals for the entrainment-related quantities of interest. When this volume is divided among the p.s.u.'s so that  $v_i$  is proportional to the  $V_i$ , the approximate length of the confidence interval for  $\hat{T}$  can be found by applying these volumes or approximations of them in the formulas in Chapter 5 for obtaining confidence intervals. Historical or projected data for the  $\lambda_i$ 's, the mean density of ichthyoplankton in the sampling periods, may be used in these formulas. One may proceed in the reverse direction, instead: starting with a decision that the total volume of water sampled is determined (perhaps by cost considerations), it is possible to obtain the approximate length of the resulting confidence interval in this manner.

4. Replicate sampling offers great potential for decreasing the length of the confidence intervals. The quadratic variance, negative binomial model for entrainment data is supported by a variety of empirical and theoretical considerations. By using replicate observations to estimate  $c^2$ , the quadratic variance parameter of (5.8), and then using formula (5.14) instead of (5.13), we can often obtain dramatic reductions in  $\hat{S}$ .

Recall that  $c^2$  differs among species-life stage combinations, so it will be most beneficial to schedule replicate sampling at times when the species-life stage combinations of primary importance are being entrained. It is not possible to know at present what the optimal pattern of replicate sampling should be. This is because our current knowledge of its properties comes from a relatively small number of sampling experiments. More information must be collected in order to gain a better understanding of  $c^2$ , the negative binomial model, and related aspects of the entrainment process.

A reasonable way to gather further information would be to schedule a limited amount of replicate sampling on a weekly basis, e.g., two consecutive days per week, Tuesdays and Wednesdays say. Such a schedule would allow us to investigate whether  $c^2$  is constant over hours in the day, days within the week, and weeks within the season. Data analyzed in this study have been consistent with the hypothesis that  $c^2$  is constant for each species and life stage of ichthyoplankton, i.e., that  $c^2$  has the same value for every p.s.u. in the sampling season. Additional replicate sampling data may further support this hypothesis. If instead it appears that  $c^2$  varies over the sampling season, these changes may occur in ways we can take advantage of. Replicate sampling should receive special attention until the behavior of  $c^2$  and related aspects of entrainment are more fully understood.

5. Sample duration, or the time interval corresponding to a p.s.u., should be short; as shown in Section 4.3, shorter p.s.u.'s give better performance. The difference between p.s.u.'s of one hour and three hours, for example, is a complex function of the entrainment pattern; this difference does not appear to be great. Thus, while a sample duration of one hour is preferable, durations of two or three hours also perform well.

Sample durations of less than one hour are not recommended for three reasons. First, short sample durations lead to great variability in the data, producing observed counts that are relatively high in noise and low in signal. Second, and more important, the justification of the negative binomial model of Section 4.2 has been largely empirical, based on analysis of one-, two-, and three-hour samples. For sample durations shorter than one hour, it cannot be taken for granted that the negative binomial model will perform as well as it does with one- to three-hour samples. Third, short durations will increase the number of p.s.u.'s and samples in the season, which in turn will increase administrative and record-keeping costs.

6. If a splitting device is able to divide a sample into subsamples whose organism counts are a random division of the sample's ichthyoplankton contents, in the sense of Sections 7.0 and 7.1, then the three-stage adaptive sampling method of sum-quota sampling will outperform fixed-volume sampling plans. Under these conditions, sum-quota sampling is a more efficient source of information per dollar spent on processing the samples than fixed-volume sampling, in which the entire sample of volume  $v_i$  or a fixed fraction of the sample is always analyzed. At present, splitting devices that successfully produce a random division of the ichthyoplankton in a sample do not appear to be available, so the use of sum-quota sampling methods is not appropriate. When the problem of organism clumping in splits has been overcome, however, sum-quota sampling should improve greatly on fixed-volume methods. More remains to be done in order to obtain recommendations of appropriate values of the parameters (the quota  $Q$ , the number of splits, and so on) for optimizing the performance of sum-quota sampling.

## 9.2 RECOMMENDATIONS FOR ENTRAINMENT ABUNDANCE SAMPLING DESIGNS

The specific features recommended for an entrainment abundance sampling design will now be listed. These were discussed in greater detail in Section 9.1.

An effective sampling design should have

1. Continuous sampling, or as nearly continuous sampling as possible;
2. Equality of the sampling fractions, or as close to equality as possible, in all p.s.u.'s;
3. Adequate total volume sampled from the discharge flow to yield acceptable confidence intervals for the entrainment quantities of interest;
4. Sufficient replicate sampling to allow precise estimation of the quadratic variance parameter  $c^2$ , leading to shorter confidence intervals;
5. P.s.u.'s of duration one to three hours.

The techniques developed in Chapter 5 can be used to obtain estimates of entrainment abundance and entrainment reduction from a design of this kind. If improved splitting devices become available,

6. Laboratory analysis of samples by sum-quota sampling should be employed. The techniques developed in Chapter 7 will then be needed.

### 9.3 RECOMMENDATIONS FOR ADDITIONAL STUDIES RELATED TO ENTRAINMENT ESTIMATION

Several additional studies would provide information with far-reaching implications for entrainment sampling. These studies would examine the assumptions currently adopted and the proper use of historical estimates of entrainment obtained from designs different from those recommended here.

#### 9.3.1 A STUDY OF REPLICATE SAMPLING

The replicate sampling done to date has been analyzed under Assumption 3.1'. This treats two simultaneously drawn samples from different locations in the discharge channel as a random sample of two pump or net volumes chosen from the much larger collection of pump or net volumes constituting the discharge flow during the p.s.u. Two simultaneous samples can be thought of as two independent draws from a common underlying distribution of organism counts. It is important to assess whether this assumption is true. If it is, the position of the sampling gear in the discharge channel cross-section should not influence the sample organism counts. This allows us to think of each sample count as having been randomly chosen from the set of all counts that could have been obtained from different locations of the sampling gear in the discharge channel cross-section; viewing sample counts as random samples of one or two observations from this set, we can make inferences about the set and hence about total entrainment during the p.s.u. being considered.

On the other hand, systematic spatial nonrandomness may be present, that is, the position of the sampler in the cross-section may influence the sample organism counts. For example, counts may exhibit a consistent tendency to be higher at some depths than at others. The presence of such nonrandomness would create difficulties, perhaps great ones, for entrainment estimation.

There are other reasons to suspect that systematic spatial nonrandomness may occur. In a closed discharge pipe, like those at Bowline Point and Roseton, the velocity of flow at a cross-sectional location is strongly influenced by the distance of that location from the pipe's wall. Velocity of flow at a point in the cross-section increases as the point moves away from the wall of the pipe toward the center of the pipe. This means that a greater volume of cooling water will pass by the location of a sampling orifice as the orifice moves closer to the center of the pipe's circular cross-section. The effects of this situation on entrainment sampling are not clear, though, because ichthyoplankton density could be related to flow velocity at a cross-sectional location, to the location itself, or to both. These complex circumstances are potentially present in the open discharge channel of Indian Point as well, although perhaps to a lesser degree.

Sampling gear presently in use draw all samples from one or two fixed locations in the discharge channel cross-section. This does not permit an investigation of the effect, if any, of this location. Sampling gear that allow the intake to occur at any of a large number of cross-sectional locations would let us address the question of whether location has an effect on sample organism counts. Such a gear could consist of a fixed horizontal beam with a vertical crossbeam mounted to it, capable of sliding from left to right and also of being raised and lowered. Mounting the sampler intake pipe to the bottom end of the crossbeam would give it access to a wide range of positions in the discharge channel. Repeating this arrangement by having two vertical crossbeams with sampler intake pipes, to be used simultaneously, would make it possible to take replicate observations. A carefully designed and analyzed series of paired observations could reveal whether each pair is a true set of replicates, i.e., a random sample of size two from a common distribution, or a simultaneous pair with systematic spatial nonrandomness present. If the latter is the case, it may be possible to determine the pattern of the nonrandomness and to use this information to improve entrainment estimates.

### 9.3.2 A STUDY OF ENTRAINMENT ABUNDANCE AND PLANT OPERATING FACTORS

The difficulties in relating flow rate to entrainment were discussed in Section 4.7. The simplest model, given by (4.17), is based on the hypothesis that entrainment during a time interval is directly proportional to the flow rate, or equivalently that ichthyoplankton density in the discharge channel is independent of the flow rate there. Thus, doubling the flow rate in a time interval would double the number of organisms entrained. It is, however, possible that a more complicated relationship, such as (4.19), holds rather than (4.17). To determine the links connecting entrainment abundance, species abundance in the river, and flow rate, observe that the first of these is a function of the other two. To examine the relationship between entrainment abundance and flow rate, it is necessary to hold species abundance constant. We cannot measure entrainment simultaneously at two or more different flow rates at a given plant, which is ideally what we would like.

Instead, we can sample plant entrainment at flow rate  $f_1$  for a brief period of length  $t$ ; then change the plant operating conditions to attain flow rate  $f_2$  and wait for any residual effects of the earlier flow rate  $f_1$  and the changeover to pass; then sample plant entrainment at flow rate  $f_2$  for a time interval of length  $t$ . The time interval between sampling

at flow rates  $f_1$  and  $f_2$  must be as short as possible, in order to keep river conditions (species abundance, tide, etc.) as similar as possible in the two sampling periods. Likewise,  $t$  must be small enough to make the sampling periods similar, thereby reducing the bias resulting from differences between the sampling periods to an acceptable level. However,  $t$  must also be large enough to allow collection of sufficient sample volumes to provide acceptably accurate, that is, low-variance entrainment estimates. If practical considerations prevent us from finding a value of  $t$  that is satisfactory on both of these criteria, the first is more important: we can keep  $t$  small to avoid bias, taking several replications with this  $t$  rather than one with a larger  $t$ .

This experiment should be repeated for a variety of river conditions, plant operating conditions, and choices of  $f_1$  and  $f_2$ . Several independent runs at each combination of conditions should be performed, with as much prespecification as possible of the times and conditions at which the runs are to be made. If feasible, a more elaborate changeover design of the form  $f_1$ , then  $f_2$ , then  $f_1$ , then  $f_2$  could be more informative.

A critical requirement is to make adjacent time intervals comparable by making both  $t$  and the changeover period between flow rates small, so bias is low. An analysis would proceed by testing the hypothesis that the average density  $\bar{\lambda}$  is independent of the flow rate  $f$ . Suitable variables among the run conditions may be used as covariates.

### 9.3.3 USE OF HISTORICAL ENTRAINMENT ESTIMATES

Many past entrainment studies lack the replicate sampling necessary for the estimation of the quadratic variance parameter  $c^2$ . The classification model of Section 4.5.1 leads to the bound  $c^2 \leq 1$ . Unless one adopts this model, however, no bound can be put on  $c^2$ , and consequently no confidence intervals can be derived for the entrainment abundance and entrainment reduction from these past data sets.

While  $c^2$  is known to vary from one species and life stage of ichthyoplankton to another, it may well be the case that for a given species and life stage,  $c^2$  is constant across p.s.u.'s within a particular entrainment season and also from one season to the next. Replicate sampling data analyzed in this project are consistent with the hypothesis that  $c^2$  remains unchanged throughout a season. Replicate sampling data from another year or two will be necessary in order to assess whether  $c^2$  for a given species and life stage remains the same over several years. If it does, the estimated value  $\hat{c}^2$  obtained from replicate sampling data spanning

several years can logically be applied to studies from previous years without replicate sampling. This approach would result in confidence intervals for entrainment estimates from these previous years.

#### 9.3.4 A STUDY OF PLANT FLOW VOLUMES AND SAMPLE VOLUMES

Throughout this report, it has been assumed that the discharge flow volume  $V_i$  and the sample volume  $v_i$  in each p.s.u.,  $i=1, \dots, N$ , are known. As noted at the end of Section 5.9, if they are not known exactly and must be estimated, there will be a random amount of difference between our data value for  $V_i$  (for example) and the underlying, unknown true value of  $V_i$ . The same is true for  $v_i$ . The presence of such random errors could have serious statistical consequences for entrainment sampling methods. A study of the relationship between the values of  $V_i$  and  $v_i$  determined by standard measurement methods and the true values of  $V_i$  and  $v_i$  would provide information on how much or how little error is included in the observed values recorded for  $V_i$  and  $v_i$ . The magnitude of the effect of these errors on entrainment estimation could then be investigated.

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## APPENDIX I

### DERIVATION OF A THEORETICAL MODEL FOR POISSON VARIATION IN THE DISCHARGE CHANNEL

Define

- A = cross-sectional area of water in the channel
- a = cross-sectional area of water in the sampler
- W = velocity of water in the channel
- w = velocity of water in the sampler
- $\Theta(u)$  = mean cross-sectional density of ichthyoplankton in both the channel and the sampler at time u.

At any instant, du, the mean cross-sectional ichthyoplankton count in the channel is  $A\Theta(u)du$ , and thus, the mean count during a time interval  $(u_0, u_1)$  is

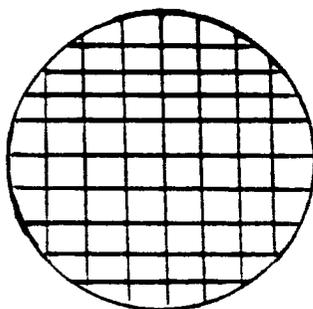
$$T(u_0, u_1) = AW \int_{u_0}^{u_1} \Theta(u) du$$

Similarly, in the sampler, the mean count is

$$t(u_0, u_1) = aw \int_{u_0}^{u_1} \Theta(u) du$$

By definition,  $AW=f$ =flowrate in the channel, and  $aw=f$ =flowrate sampler. Thus  $T(u_0, u_1) = \frac{F}{f}t(u_0, u_1)$ .

Consider a cross-section, at any instant du, divided



area of subregion =  $\delta$   
area of cross-section = a

into m subregions, each with area  $\delta$ . Let  $Y_i, i=1, \dots, m$  denote the number of ichthyoplankton in the  $i^{\text{th}}$  subregion. Make the following assumptions:

$$1. P(Y_i > 1) = o(\delta)$$

The notation  $o(\delta)$  denotes a function that satisfies  $\lim_{\delta \rightarrow 0} o(\delta)/\delta = 0$ . Thus, we are assuming that there will be no more than one observation in any sufficiently small subregion.

$$2. P(Y_i = 1) = \theta_0 \delta + o(\delta), \text{ for some constant } \theta_0$$

This assumption can be interpreted as saying that, for small subregions, the probability that the region has an observation is proportional to the area of the region.

$$3. Y_1, \dots, Y_m \text{ are independent for every } m.$$

Let  $T_m = \sum_{i=1}^m Y_i$ . Then  $P(\text{one or more } Y_i > 1) = o(\delta)$  by assumption 1, so

$$\begin{aligned} P(T_m = t) &= P\left(\sum_{i=1}^m Y_i = t\right) = P\left(\sum_{i=1}^m Y_i = t \mid Y_1 \leq 1, Y_2 \leq 1, \dots, Y_m \leq 1\right) \\ &\quad \times P(Y_1 \leq 1, Y_2 \leq 1, \dots, Y_m \leq 1) + o(\delta) \\ &= P\left(\sum_{i=1}^m Y_i = t \mid Y_1 \leq 1, Y_2 \leq 1, \dots, Y_m \leq 1\right) [1 - o(\delta)]^m + o(\delta), \end{aligned}$$

where the last equality follows from assumptions 1 and 3. To evaluate the conditional probability, note that if  $Y_i \leq 1$ , then  $Y_i$  is either 0 or 1. Using assumptions 2 and 3, we can treat these conditional  $Y_i$ 's as binomial variables, and use the binomial probability formula to obtain

$$P(T_m = t) = \binom{m}{t} [\theta_0 \delta + o(\delta)]^t [1 - \theta_0 \delta + o(\delta)]^{m-t} [1 - o(\delta)]^m + o(\delta)$$

Now let  $m \rightarrow \infty$ ,  $\delta \rightarrow 0$ , and  $m\delta \rightarrow a$ . That is, take smaller and smaller subregions, but always cover the entire cross-section. Then, using Stirling's formula,

$$\begin{aligned}
\lim P(T_m=t) &= \lim \frac{m^{m+1/2} e^{-m}}{t!(m-x)^{m-t+1/2} e^{-(m-t)}} \\
&\quad \times [\theta_0 \delta + o(\delta)]^t [1 - \theta_0 \delta + o(\delta)]^{m-t} [1 - o(\delta)]^m \\
&= \lim (m-t)^t [\theta_0 \delta + o(\delta)]^t [1 - \theta_0 \delta + o(\delta)]^{m-t} / t! \\
&= (\theta_0 a)^t \exp(-\theta_0 a) / t!
\end{aligned}$$

This is the Poisson distribution.

Therefore, the distribution in a cross-section of width  $du$  at time  $u_0$  is  $\text{Poisson}(\theta_0 a)$ . To get the distribution of the counts for a sample taken from time  $u_0$  to  $u_1$ , we must integrate over all cross-sections. Since the value  $\theta_0 a$  is a function of time, write  $\theta_0 a = a\theta(u)du$ . The above argument shows that the number of ichthyoplankton,  $T(u)$ , in a cross-section of width  $du$  is  $\text{Poisson}(a\theta(u)du)$ . If sampling is done during  $(u_0, u_1)$ , and the velocity of water is  $w$ , the total number of ichthyoplankton is a Poisson variable with parameter

$$aw \int_{u_0}^{u_1} \theta(u) du = t(u_0, u_1).$$

The same argument applies in the channel, the only difference being that the cross-sectional area is  $A$  and the velocity is  $W$ . Thus, the variation in the channel is Poisson, with parameter

$$AW \int_{u_0}^{u_1} \theta(u) du = T(u_0, u_1),$$

if assumptions 1 to 3 hold.

#### I.4

The Poisson distribution is closely related to the concept of complete spatial randomness. Consider an individual organism located somewhere in the cross-section of the discharge channel at which the sampling gear has been placed. It is plausible to hypothesize that the organism is equally likely to appear at any position in the cross-section of the sampler. In other words, no point in this cross-section is more likely to be the organism's position than any other point. A slightly different way to express this situation (which is also more correct in formal mathematical terms) is: the probability of the organism's appearing at some point in any specified subregion of the cross-section is directly proportional to the area of the subregion. Any two subregions of the same cross-sectional area are equally likely to contain the organism no matter what their shapes and locations in the cross-section of the sampler are.

A second plausible hypothesis is that each organism's position in this cross-section is determined independently of all other organisms. In other words, information about the location of any organism(s) in the cross-section at which the sampling gear is situated is not of any value or help in predicting where any other organism(s) will appear.

If both of these assumptions are met, we can say that the distribution of organisms is spatially random, or that this distribution is characterized by complete spatial randomness. Assume further that a fixed number of individual organisms is present in the discharge channel in a cross-section of incremental width  $dt$ , from which the sampler draws a very small fraction of the cross-section's volume. Then a derivation similar to the one given earlier in this appendix establishes that the count of organisms sampled has a Poisson distribution. This result can be extended from the incremental cross-section of width  $dt$  to a time interval  $(t_0, t_1)$  without difficulty.

## APPENDIX II

### DERIVATION OF A THEORETICAL MODEL FOR NEGATIVE BINOMIAL MODEL VARIATION IN THE DISCHARGE CHANNEL

If a particular ichthyoplankton count is observed with Poisson variation, i.e.,  $X \sim \text{Poisson}(\lambda v)$  where  $v$  = volume of water sampled and  $\lambda$  = mean density, and if in addition  $\lambda$  itself is subject to variation according to the density function

$$f(\lambda) = \frac{\beta^K}{\Gamma(K)} \lambda^{K-1} e^{-\beta\lambda} \quad \text{for } \lambda > 0,$$

which is a Gamma( $K, \beta$ ) distribution, we then have

$$\begin{aligned} P(X=x) &= \int_0^{\infty} P(X=x|\lambda) f(\lambda) d\lambda \\ &= \int_0^{\infty} \frac{e^{-\lambda v} (v\lambda)^x}{x!} \frac{\beta^K}{\Gamma(K)} \lambda^{K-1} e^{-\beta\lambda} d\lambda \\ &= \frac{v^x \beta^K}{x! \Gamma(K)} \int_0^{\infty} \lambda^{x+K-1} e^{-(\beta+v)\lambda} d\lambda \\ &= \frac{v^x \beta^K}{x! \Gamma(K)} \frac{\Gamma(x+K)}{(\beta+v)^{x+K}} \end{aligned}$$

Using the fact that  $\Gamma(n) = (n-1)!$ , this last expression can be rearranged to give

$$P(X=x) = \binom{K+x-1}{x} \left(\frac{\beta}{\beta+v}\right)^K \left(\frac{v}{\beta+v}\right)^x \quad \text{for } x=0, 1, 2, \dots \quad (\text{II.1})$$

which is the negative binomial distribution. For convenience, let  $p = \beta/(\beta+v)$  and  $q = 1-p = v/(\beta+v)$ . It is well known that the negative binomial distribution (II.1) has mean  $E(X) = \mu = Kq/p$  and variance  $\text{Var}(X) = Kq/p^2$ . (See, for example, Mood, Graybill, and Boes, 1974, Appendix B). Routine algebra shows that

II.2

$$\begin{aligned}
 \text{Var}(X) &= Kq/p + (Kq/p)^2 - Kq/p \\
 &= \mu + \mu(1-p)/p \\
 &= \mu + \mu(Kq/p)(1/K) \\
 &= \mu + (1/K)\mu^2
 \end{aligned}
 \tag{II.2}$$

The density of the negative binomial distribution with parameters  $K$  and  $p$  is obtained from (II.1) by replacing  $\beta/(\beta+v)$  with  $p$  and replacing  $v/(\beta+v)$  with  $q = 1-p$ , giving

$$P(X=x) = \binom{K+x-1}{x} p^K q^x \quad \text{for } x=0,1,2,\dots
 \tag{II.3}$$

It is sometimes convenient to use the parameters  $c^2$  of (4.30) and  $\mu = Kq/p$  instead of  $K$  and  $p$ . Comparing (4.30) and (II.2) shows that

$$\begin{aligned}
 c^2 &= 1/K \\
 K &= 1/c^2;
 \end{aligned}
 \tag{II.4}$$

comparing the formulas for  $E(X)$  and  $\text{Var}(X)$  given above (II.2) and using (4.30), we obtain

$$\text{Var}(X) = \mu/p = \mu + c^2 \mu^2,
 \tag{II.5}$$

from which it follows immediately that

$$p = 1/(c^2 \mu + 1).
 \tag{II.6}$$

A standard interpretation of the negative binomial  $(K,p)$  distribution is that it describes the number of failures occurring before the  $K$ th success in a series of independent Bernoulli (success-failure) trials in which the probability of success is  $p$  at every trial. Although this interpretation makes sense only when  $K$  is an integer, and some calculations for the negative binomial distribution are easier when  $K$  is an integer, none of the results in this study require  $K$  to be an integer.

There are numerous methods for simulating a negative binomial random variable, several of which appear in Rubinstein (1981, Sec. 3.7 and 3.7.4). The most routine of these is the general algorithm for generating random

### II.3

values from any discrete density known as the inverse transform algorithm (Rubinstein's Algorithm IT-2). It begins by calculating  $p_0 = P(X=0)$ ,  $p_1 = P(X=1)$ ,  $p_2 = P(X=2)$ , ... using (II.3). It then generates a uniform random number,  $U$ , from the unit interval  $(0,1)$ , and transforms  $U$  to a negative binomial  $X$  by returning the value

$$X = 0 \quad \text{if} \quad U < p_0$$

$$X = 1 \quad \text{if} \quad p_0 \leq U < p_1$$

$$X = 2 \quad \text{if} \quad p_1 \leq U < p_2$$

etc.

This algorithm was used in the simulation study of Chapter 6 to generate negative binomial random values. The code for this simulation study appears in Appendix VIII.

## APPENDIX III

### DERIVATION OF THE BETA-BINOMIAL DISTRIBUTION

Given that we observe a pair of independent random variables  $(X, Y)$ , where both  $X$  and  $Y$  follow the negative binomial distribution given in Appendix II, the conditional distribution of  $X$ , conditioned on the fact that  $X+Y=t$ , is given by

$$\begin{aligned}
 P(X=x | X+Y=t) &= \frac{P(X=x) P(Y=t-x)}{P(X+Y=t)} \\
 &= \frac{\binom{K+x-1}{x} \left(\frac{\beta}{\beta+v}\right)^K \left(\frac{v}{\beta+v}\right)^x \binom{K+t-x-1}{t-x} \left(\frac{\beta}{\beta+v}\right)^K \left(\frac{v}{\beta+v}\right)^{t-x}}{\binom{2K+t-1}{t} \left(\frac{\beta}{\beta+v}\right)^{2K} \left(\frac{v}{\beta+v}\right)^t} \\
 &= \frac{\binom{K+x-1}{x} \binom{K+t-x-1}{t-x}}{\binom{2K+t-1}{t}} \quad \text{for } x=0, 1, 2, \dots
 \end{aligned}$$

Since both  $X$  and  $Y$  are negative binomial with parameters  $K$  and  $\beta/(\beta+v)$ , it follows that  $X+Y$  is also negative binomial with parameters  $2K$  and  $\beta/(\beta+v)$ . Thus,

$$\begin{aligned}
 P(X=x | X+Y=t) &= \frac{P(X=x, X+Y=t)}{P(X+Y=t)} \\
 &= \frac{P(X=x, Y=t-x)}{P(X+Y=t)} \\
 &= \frac{P(X=x) P(Y=t-x)}{P(X+Y=t)}
 \end{aligned}$$

which is the beta-binomial distribution.

## APPENDIX IV

### THE LIMIT OF NEGATIVE BINOMIAL SUMS

Let  $X_i$  be a negative binomial random variable with probability mass function

$$P(X_i = x) = \binom{K_i + x - 1}{x} p^{K_i} (1-p)^x$$

or, for short, write  $X_i \sim \text{NB}(K_i, p)$ .

If  $X_1, \dots, X_n$  are independent random variables, and  $X_i \sim \text{NB}(K_i, p)$ , then it is true that  $T = \sum_{i=1}^n X_i \sim \text{NB}(\sum_{i=1}^n K_i, p)$ . The probability generating function of  $T$  is given by

$$M_T(s) = \left( \frac{p}{1 - (1-p)s} \right)^{\sum_{i=1}^n K_i}$$

Now let  $n \rightarrow \infty$  so that  $\sum_{i=1}^n K_i \rightarrow \infty$ ,  $p \rightarrow 1$ , and  $(1-p) \sum_{i=1}^n K_i \rightarrow \lambda$ .

Then

$$M_T(s) = \left( \frac{1 - \lambda / \sum K_i}{1 - \lambda s / \sum K_i} \right)^{\sum K_i},$$

and passing to logarithms, it follows that

$$\lim M_T(s) = e^{-\lambda + \lambda s},$$

## IV.2

the probability generating function of the Poisson distribution.

Thus, in this precise sense, the Poisson is the limit of negative binomials, or, for large  $n$ ,  $\sum_{i=1}^n X_i$  is approximately Poisson.

Translating this back to the case of sampling in the discharge channel will give some indication why it is not expected that the Poisson approximation will work. For discharge sampling,  $p = \beta / (\beta + v)$  (see Appendix II), where  $v$  = volume sampled and  $\beta$  is a parameter describing the variation in mean density. In order for the above limit argument to apply,  $\beta / (\beta + v)$  must be constant throughout the entire sampling period. While this may be approximately true for  $v$ , it is almost certainly false for  $\beta$ . The other two limit conditions, that  $p \rightarrow 1$  and  $(1-p) \sum K_i \rightarrow \text{constant}$  are also suspect. Hence, although it is possible to model sums of negative binomials as Poissons, this theory does not seem to apply here.

APPENDIX V

CALCULATION OF BIAS IN SUBSAMPLING

A sample is randomly divided into  $L=8$  subsamples, and these are processed in random order until  $Q=200$  organisms have been counted, i.e., the processing sequence terminates with the  $\ell^{\text{th}}$  subsample if the total count,  $T_\ell$ , in the  $\ell$  subsamples is at least  $Q=200$ . The usual estimate,  $\hat{T}$ , of the total count in the  $L$  subsamples is then  $\hat{T} = (L/\ell)T_\ell$ .

Let  $f_i(x)$  denote the probability that the total in  $i$  subsamples,  $T_i$ , equals  $x$ , i.e.,

$$f_i(x) = P(T_i=x).$$

Let  $f_X(x)$  denote the probability that a single subsample count equals  $x$ , i.e.,

$$f_X(x) = P(X=x) = f_1(x),$$

and let  $E_L(\ )$  denote expectation over  $L$  subsamples. Then we have

$$\begin{aligned} E_L(T_\ell/\ell) &= \sum_{t_1=Q}^{\infty} t_1 f_1(t_1) + \sum_{\ell=2}^{L-1} \frac{1}{\ell} \sum_{t_{\ell-1}=0}^{Q-1} f_{\ell-1}(t_{\ell-1}) \sum_{x_\ell=Q-t_{\ell-1}}^{\infty} (t_{\ell-1}+x_\ell) f_X(x_\ell) \\ &\quad + \frac{1}{L} \sum_{t_{L-1}=0}^{Q-1} (t_{L-1}+\lambda) f_{L-1}(t_{L-1}) \\ &= E_{L-1}(T_\ell/\ell) - \frac{1}{L-1} \sum_{t_{L-2}=0}^{Q-1} f_{L-2}(t_{L-2}) \sum_{x_{L-1}=0}^{Q-1-t_{L-2}} (t_{L-2}+x_{L-1}) f_X(x_{L-1}) \\ &\quad + \frac{1}{L} \sum_{t_{L-1}=0}^{Q-1} (t_{L-1}+\lambda) f_{L-1}(t_{L-1}) \end{aligned}$$

$$= E_{L-1}(T_{\ell}/\ell) + D_L(Q-1),$$

where we use  $D_L(Q-1)$  to denote the second two terms above. We now have a recursion relation, which allows us to write

$$E_L(T_{\ell}/\ell) = E_1(T_1) + D_2(Q-1) + \dots + D_L(Q-1) = \lambda + \sum_{\ell=2}^L D_{\ell}(Q-1)$$

Note that we have used the convolution formulas,

$$f_{\ell}(t) = \sum_{t_{\ell-1}=0}^t f_{\ell-1}(t_{\ell-1}) f_{\chi}(t-t_{\ell-1}),$$

$$F_{\ell}(t) = \sum_{t_{\ell-1}=0}^t f_{\ell-1}(t_{\ell-1}) F_{\chi}(t-t_{\ell-1})$$

where  $F$  denotes the cumulative distribution function of  $f$ . For the case of the Poisson distribution we have

$$f_{\chi}(x) = \frac{e^{-\mu} \mu^x}{x!} \equiv p(x; \mu),$$

$$x f_{\chi}(x) = \mu p(x-1; \mu),$$

$$f_{\ell}(t_{\ell}) = p(t_{\ell}; \ell\mu).$$

These equations together with the convolution equation imply

$$D_{\ell}(Q-1) = \frac{\mu}{\ell} p(Q-1; (\ell-1)\mu).$$

So in the Poisson case

$$E_L(T_{\ell}/\ell) = \mu \left[ 1 + \sum_{\ell=2}^L \frac{1}{\ell} p(Q-1; (\ell-1)\mu) \right].$$

N.B. If  $X_i = \sum_j X_{ij}$  is a sum of independent Poisson variables with means  $E(X_{ij}) = p_j \mu$ ,  $\sum_j p_j = 1$ , then  $T_{\ell} = \sum_j T_{\ell j}$  and  $E(T_{\ell j}/\ell) = p_j E(T_{\ell}/\ell)$ .

## APPENDIX VI

### TESTING FOR A QUADRATIC RELATIONSHIP BETWEEN VARIANCE AND MEAN USING PAIRED OBSERVATIONS

Suppose that  $n$  paired observations  $(X_1, Y_1), (X_2, Y_2), \dots, (X_n, Y_n)$  are collected. These may be two replicate observations  $X_i$  and  $Y_i$  taken at each of  $n$  times indexed by  $i=1, 2, \dots, n$ . We wish to test the null hypothesis that  $\sigma_i^2 = \mu_i + c^2 \mu_i^2$  for each  $i$ . Assume that for every  $i$ ,  $X_i$  and  $Y_i$  are independent, identically distributed random variables with mean  $\mu_i$  and variance  $\mu_i + c^2 \mu_i^2$ . Let

$$Z_i = (X_i + Y_i)/2$$

$$S_i^2 = (X_i - Y_i)^2/2.$$

Then

$$EZ_i = (\mu_i + \mu_i)/2 = \mu_i$$

$$\begin{aligned} E(S_i^2) &= (1/2)[E(X_i^2) + E(Y_i^2) - 2E(X_i)E(Y_i)] = E(X_i^2) - E(X_i)E(Y_i) \\ &= \text{Var}(X_i) + [E(X_i)]^2 - [E(X_i)]^2 = \mu_i + c^2 \mu_i^2. \end{aligned}$$

Note that

$$\begin{aligned} E(Z_i^2) &= (1/4)[E(X_i^2) + E(Y_i^2) + 2E(X_i)E(Y_i)] \\ &= (1/2)[E(X_i^2) + E(X_i)E(Y_i)] \\ &= \text{Var}(X_i)/2 + [E(X_i)]^2 \\ &= \mu_i/2 + (1 + c^2/2)\mu_i^2. \end{aligned}$$

It follows routinely that

$$ES_i^2 = \mu_i + c^2 \mu_i^2 = E \left[ \frac{Z_i(1 + c^2 Z_i)}{1 + c^2/2} \right],$$

so under appropriate assumptions,

$$S_i^2 / \frac{Z_i(1 + c^2 Z_i)}{1 + c^2/2} \sim \chi_1^2.$$

If  $c^2$  is constant, or relatively constant, from sample to sample, we can sum over the  $n$  independent samples to obtain

$$\sum_{i=1}^n \frac{S_i^2(1 + c^2/2)}{Z_i(1 + c^2 Z_i)} \sim \chi_n^2$$

and

$$\sum_{i=1}^n \frac{S_i^2(1 + \hat{c}^2/2)}{Z_i(1 + \hat{c}^2 Z_i)} \sim \chi_{n-1}^2, \quad (\text{VI.1})$$

where  $\hat{c}^2$  is an estimate of  $c^2$ . Each sample variance  $S_i^2$  based on a paired sample  $(X_i, Y_i)$  is an unbiased estimator of the variance  $\mu_i + c^2 \mu_i^2$ , so

$$\begin{aligned} 2(\mu_i + c^2 \mu_i^2) &= 2E[(X_i - Y_i)^2/2] \\ &= E[(X_i - Y_i)^2] \\ &= E[X_i^2 + Y_i^2 - 2X_i Y_i] \end{aligned} \quad (\text{VI.2})$$

It is also true that (using the independence of  $X_i$  and  $Y_i$ )

$$\begin{aligned} E[X_i + Y_i + 2X_i Y_i c^2] &= E(X_i) + E(Y_i) + 2c^2 E(X_i)E(Y_i) \\ &= 2(\mu_i + c^2 \mu_i^2) \end{aligned} \quad (\text{VI.3})$$

It follows, with the help of these results, that

$$\begin{aligned} E[(X_i + Y_i)(X_i + Y_i - 1)] &= E[X_i^2 + Y_i^2 + 2X_i Y_i - (X_i + Y_i)] \\ &= E[X_i^2 + Y_i^2 - 2X_i Y_i] - E[X_i + Y_i + 2X_i Y_i c^2] \\ &\quad + E[4X_i Y_i + 2X_i Y_i c^2] \\ &= 2(\mu_i + c^2 \mu_i^2) - 2(\mu_i + c^2 \mu_i^2) + 2(c^2 + 2)E[X_i Y_i] \end{aligned} \quad (\text{VI.4})$$

Thus, if a number of independent pairs  $(X_i, Y_i)$  having a common  $c^2$ , although not necessarily a common  $\mu$ , are available,

then a consistent estimator of  $c^2$  is given by

$$\hat{c}^2 = \left[ \frac{\sum_{i=1}^n (X_i + Y_i)(X_i + Y_i - 1)}{2 \sum_{i=1}^n X_i Y_i} - 2 \right]^+ \quad (\text{VI.5})$$

where  $[ ]^+$  is the positive part function:  $[w]^+ = \max(w, 0)$ . (Consistency is an important property for an estimator to possess. Informally, it means that the estimator is certain to approach the true, unknown parameter as the number of observations increases. For a more formal definition, see Mood, Graybill, and Boes (1974, p. 295).)

Substituting the  $\hat{c}^2$  of formula (VI.5) into (VI.1) gives a relationship that can be used to test  $H_0: \sigma_i^2 = \mu_i + c^2 \mu_i^2$  for each  $i$ .

## APPENDIX VII

### A TIME SERIES APPROACH TO ESTIMATING ENTRAINMENT ABUNDANCE

A recent paper (Madenjian and Jude, 1983) has proposed that time series techniques can be incorporated directly into the estimation of entrainment at power plants. In assessing whether this approach is appropriate, it is important to distinguish between the actual entrainment during the year and the theoretical expected entrainment of the power plant. The theoretical expected entrainment can be viewed as the average entrainment that would be observed in many replications of the sampling year under identical plant and river conditions. These replications would differ in observed entrainment because of random variation in the underlying process. The actual entrainment during the year is a single replication from this conceptual system.

Madenjian and Jude have confused sampling error and process error. (Sampling error is the random variability inherent in sampling from the discharge channel at a given time; process error is the random variability inherent in the stochastic process of entrainment over the year.) Their replicate samples at a given instant provide a measure of the sampling error variance of  $\hat{N}_j - N_j$ , where  $N_j$  is the actual number entrained during period  $j$  and  $\hat{N}_j$  is their estimate of this number. (In the paper they fail to make this notational distinction, referring to  $N_j$  as the estimated number entrained.) The entrainment process ( $N_j$ ) over the year is indeed a stochastic process that generates a time series when time is blocked into periods; if the correct model for this process could be specified, then sampling error  $\hat{N}_j - N_j$  and process error  $N_j - \mu_j$  might be separately estimated, where  $\mu_j$  is the theoretical expected entrainment during period  $j$ . Since sampling errors on different sampling occasions are uncorrelated ( $\text{Cov}(\hat{N}_i - N_i, \hat{N}_j - N_j) = 0$ ), an estimated serial correlation is

$$r_{|i-j|} = \frac{\text{Cov}(N_i - \mu_i, N_j - \mu_j)}{\sqrt{[\text{Var}(\hat{N}_i - N_i) + \text{Var}(N_i - \mu_i)][\text{Var}(\hat{N}_j - N_j) + \text{Var}(N_j - \mu_j)]}}$$

The authors calculate  $r_{|i-j|}$  using Box and Jenkins techniques, and then incorrectly multiply it by sampling standard errors to "estimate" the numerator, which in their notation is  $\text{Cov}(N_i, N_j)$ , as

$$\text{Cov}(N_i, N_j) = r_{|i-j|} \sqrt{\text{Var}(\hat{N}_i - N_i) \text{Var}(\hat{N}_j - N_j)}$$

## VII.2

Madenjian and Jude seem unaware of the statistical subtleties in estimation inference when several components of estimation error are involved. If the estimation target is the actual entrainment during the specific year in question then process error is not an issue, since process error refers to what might have been entrained rather than what was entrained (process error is the difference between actual entrainment and the theoretically expected entrainment). Actual entrainment is estimated by a sampling process that yields internal estimates of sampling error variance and permits estimation of the sampling standard error of our estimate of the actual entrainment.

If the objective were, instead, to estimate the theoretical entrainment, then the estimate of the actual entrainment for the year would serve that purpose, but would include a second component of error

$$\begin{aligned} \text{Actual} - \text{Theoretical} &= (\text{Actual} - \text{Actual}) \\ &+ (\text{Actual} - \text{Theoretical}). \end{aligned}$$

Estimation of the second component is model-dependent, and several years of data would be required to validate a model. Statistical separation of sampling error variance and process error variance would require more sophisticated statistical methods than those used by the authors.

APPENDIX VIII

COMPUTER CODE (IN BASIC) FOR SIMULATION STUDY OF CHAPTER 6:  
ONE DAY (8 P.S.U.'S), UNIFORM VARIATION OF DENSITY

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```

10  !SIMULATES: SIMULATES VALUES FOR 1 DAY:8 3-HOUR PERIODS
20  !AND PRINTS OUT THE RESULTS.
30  !LAMBDA IS UNIFORM.
40  OPTION BASE 1
50  DIM A(16,4),C(16)
60  !A CONTAINS THE RELEVANT PARAMETERS AND STATISTICS:
70  !THE THE AVERAGE (OVER ALL SIMULATIONS) OF THE DAILY VALUES
80  !C CONTAINS THE DAILY VALUES (TO UPDATE A)
90  !A1:TRUE MEAN  A2:TRUE VAR  A3:TRUE CV
100 !A4-AB:1-SAMPLE STATISTICS
110 !A4:MEAN  A5:VAR(CSQ=1)  A6:CV
120 !A7-A12:2-SAMPLE STATISTICS
130 !A7:MEAN  A8:VAR(KNOWN CSQ)  A9:VAR(EST CSQ)
140 !          A10:CV(KNOWN CSQ)  A11:CV(EST CSQ)
150 !          A12:EST OF CSQ
160 DIM Vsample(4),Lambda(4),Csq(6)
170 DIM X(8),Y(8),Meanden(8,100)
180 DIM Lab1$(16)[15],Lab2$(16)[15] ! LABELS FOR NICE OUTPUT
190 Lab1$(1)=" TRUE DAILY  "
200 Lab2$(1)="  TOTAL"
210 Lab1$(2)=" TRUE STD DEV"
220 Lab1$(3)=" TRUE COEFF.  "
230 Lab2$(3)="OF VARIATION  "
240 Lab1$(4)=" DAILY TOTAL"
250 Lab2$(4)=" (one sample)"
260 Lab1$(5)=" STANDARD DEV."
270 Lab2$(5)=" (one sample)"
280 Lab1$(6)=" COEFF. OF VAR."
290 Lab2$(6)=" (one sample)"
300 Lab1$(7)=" DAILY TOTAL"
310 Lab2$(7)=" (two sample)"
320 Lab1$(8)=" STANDARD DEV."
330 Lab2$(8)=" (csq known)"
340 Lab1$(9)=" STANDARD DEV."
350 Lab2$(9)="(csq estimated)"
360 Lab1$(10)=" COEFF. OF VAR."
370 Lab2$(10)=" (csq known)"
380 Lab1$(11)=" COEFF. OF VAR."
390 Lab2$(11)="(csq estimated)"
400 Lab1$(12)=" ESTIMATE OF"
410 Lab2$(12)=" C-SQUARED"
420 Nsim=100!*****NUMBER OF SIMULATED DAYS*****
430 Nvar=12 !  NUMBER OF VARIABLES
440 Nperiod=8 !NUMBER OF SAMPLING PERIODS PER DAY
450 READ Vsample(*)
460 DATA 100,150,200,300
470 READ Lambda(*)
480 DATA .01,.05,.1,.25
490 READ Csq(*)
500 DATA 0,.05,.1,.33333,.5,1

```

```

510  FOR I1=1 TO 4!LOOP ON LAMBDA
520  ! GENERATE PERTURBED LAMBDA VALUES WITH UNIFORM VARIATION
530  FOR I=1 TO Nperiod
540  FOR J=1 TO Nsim
550  Meanden(I,J)=Lambda(I1)+Lambda(I1)*(RND-.5)
560  NEXT J
570  NEXT I
580  FOR Ic=1 TO 6 !LOOP ON VALUES OF CSQ
590  !INITIALIZE
600  FOR I=1 TO Nvar
610  FOR Iv=1 TO 4
620  A(I,Iv)=0
630  NEXT Iv
640  NEXT I
650  FOR Iv=1 TO 4! LOOP ON VALUES OF SAMPLE VOLUME
660  !*****START SIMULATION LOOP*****
670  FOR N=1 TO Nsim
680  ! INITIALIZE
690  FOR I=1 TO Nvar
700  C(I)=0
710  NEXT I
720  FOR I=1 TO Nperiod ! 8 3-HR SAMPLING PERIODS (1 DAY)
730  Mu=Vsample(Iv)*Meanden(I,N)
740  C(1)=C(1)+Mu
750  C(2)=C(2)+Mu+Csq(Ic)*Mu^2
760  IF Csq(Ic)=0 THEN
770  CALL Poisvar(Mu,X(I))
780  CALL Poisvar(Mu,Y(I))
790  ELSE
800  K=INT((1/Csq(Ic))+.5)
810  P=1/(Csq(Ic)*Mu+1)
820  CALL Negativbi(K,P,X(I))
830  CALL Negativbi(K,P,Y(I))
840  END IF
850  NEXT I
860  C(3)=1
870  IF C(1)>0 THEN C(3)=SQR(C(2))/C(1)
880  FOR I=1 TO Nperiod ! ONE SAMPLE STATISTICS
890  C(4)=C(4)+X(I)
900  C(5)=C(5)+X(I)*(1+X(I))/2
910  NEXT I
920  C(6)=1
930  IF C(4)>0 THEN
940  C(6)=SQR(C(5))/C(4)
950  END IF
960  !***** CALCULATE ESTIMATE OF CSQ *****
970  Top=0
980  Bot=0
990  FOR I=1 TO Nperiod
1000 Z=X(I)+Y(I)

```

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```

1010 IF Z>0 THEN
1020 Top=Top+Z*(Z-1)
1030 Bot=Bot+X(I)*Y(I)
1040 END IF
1050 NEXT I
1060 IF Bot>0 THEN
1070 Csqhat=.5*Top/Bot-2
1080 IF Csqhat<0 THEN Csqhat=0
1090 ELSE
1100 Csqhat=0
1110 END IF
1120 !*****TWO-SAMPLE STATISTICS*****
1130 FOR I=1 TO Nperiod
1140 Z=(X(I)+Y(I))/2
1150 C(7)=C(7)+Z
1160 C(8)=C(8)+2*Z*(1+Csq(Ic)*Z)/(2+Csq(Ic))
1170 C(9)=C(9)+2*Z*(1+Csqhat*Z)/(2+Csqhat)
1180 NEXT I
1190 C(10)=1
1200 C(11)=1
1210 IF C(7)>0 THEN
1220 C(10)=SQR(C(8))/C(7)
1230 C(11)=SQR(C(9))/C(7)
1240 END IF
1250 C(12)=Csqhat
1260 !*****UPDATE VARIABLES*****
1270 FOR I=1 TO Nvar
1280 A(I,Iv)=((N-1)*A(I,Iv)+C(I))/N
1290 NEXT I
1300 NEXT N
1310 !*****END SIMULATION LOOP*****
1320 NEXT Iv!END OF LOOP ON SAMPLE VOLUMES
1330 FOR I=1 TO 11
1340 FOR Iv=1 TO 4
1350 IF I=3 OR I=6 OR I=9 THEN
1360 A(I,Iv)=A(I,Iv)*100 ! PUT CV ON PERCENT SCALE
1370 END IF
1380 IF I=2 OR I=5 OR I=8 OR I=9 THEN
1390 A(I,Iv)=100000*SQR(A(I,Iv))/Vsample(Iv)
1400 END IF
1410 IF I=1 OR I=4 OR I=7 THEN
1420 A(I,Iv)=100000*A(I,Iv)/Vsample(Iv)
1430 END IF
1440 NEXT Iv
1450 NEXT I
1460 !*****PRINT OUT RESULTS*****
1470 PRINTER IS 701 ! TURN ON PRINTER
1480 PRINT USING 1490
1490 IMAGE /,6X,"SIMULATION OF SAMPLING FOR ONE DAY- EIGHT 3-HOUR PERI
ODS",/
1500 PRINT USING 1510

```

```

1510 IMAGE 10X," Channel volume=100,000 cubic meters per 3-hour perio
d.",/
1520 PRINT USING 1530
1530 IMAGE 15X,"Mean density subjected to uniform perturbations.",/
1540 PRINT USING 1550;1000*Lambda(I1),Csq(Ic)
1550 IMAGE 1X,"Mean Density of Organisms=",3D.D,"/ 1000 cubic meters",
4X,"Value
of c-squared=",D.2D,//
1560 PRINT USING 1570
1570 IMAGE 32X,"Volume of Sample in cubic meters"
1580 PRINT USING 1590
1590 IMAGE 18X,"-----"
-----"
1600 PRINT USING 1610;Vsample(1),Vsample(2),Vsample(3),Vsample(4)
1610 IMAGE 15X,4(5X,5D,6X)
1620 PRINT USING 1590
1630 FOR I=1 TO Nvar
1640 PRINT USING 1650;Lab1$(I),A(I,1),A(I,2),A(I,3),A(I,4)
1650 IMAGE 15A,4(10D.DD,3X)
1660 PRINT USING 1670;Lab2$(I)
1670 IMAGE 15A,/
1680 NEXT I
1690 PRINT USING 1700;Nsim
1700 IMAGE /,15X,"The value given is the average over",4D," simulation
s."
1710 PRINT CHR$(12) ! FORM FEED
1720 PRINTER IS 1 ! TURN OFF PRINTER
1730 NEXT Ic!END OF LOOP ON CSQ VALUES
1740 NEXT I!END OF LOOP ON LAMBDA
1750 END
1760 SUB Negativbi(K,P,X)
1770 ! GENERATES A RANDOM VARIABLE X FROM A NEGATIVE BINOMIAL
1780 ! DISTRIBUTION WITH PARAMETERS K AND P.
1790 RANDOMIZE
1800 U=RND
1810 X=0
1820 Sum=0
1830 Prob=P^K
1840 Sum=Sum+Prob
1850 IF Sum>U THEN GOTO Endit
1860 IF Sum<=U THEN
1870 Prob=((1-P)*(K+X)/(X+1))*Prob
1880 X=X+1
1890 GOTO 1840
1900 END IF
1910 Endit: SUBEND
1920 SUB Poisvar(L,X)
1930 ! GENERATES A RANDOM VARIABLE X FROM A POISSON
1940 ! DISTRIBUTION WITH PARAMETER L.
1950 RANDOMIZE
1960 U=RND
1970 X=0
1980 Sum=0
1990 Prob=EXP(-L)
2000 Sum=Sum+Prob

```

APPENDIX IX  
 INSTRUCTIONS FOR  
 IMPLEMENTING PROCEDURES GIVEN IN  
 EVALUATION OF  
 ENTRAINMENT ABUNDANCE  
 SAMPLING DESIGNS

The analysis described here must be performed separately on each life stage of each species of interest. Thus  $\hat{c}^2$  should, in principle, be indexed or subscripted by the life stage and species being analyzed, but this notation will be omitted for convenience. The same is true for all of the computations detailed in these instructions.

1. The first step is to estimate  $c^2$  by the estimator  $\hat{c}^2$ :

$$\hat{c}^2 = \left[ \frac{\sum_{i=1}^n (X'_{i1} + X'_{i2})(X'_{i1} + X'_{i2} - 1)}{2 \sum_{i=1}^n X'_{i1} X'_{i2}} - 2 \right]^+ \quad (1)$$

where  $[ ]^+$  is the positive part function:  $[z]^+ = \max(z, 0)$ . If the expression in square brackets in (1) is negative, then the estimator  $\hat{c}^2$  becomes 0. In formula (1),  $n$  is the number of p.s.u.'s in which replicate sampling has been performed, and  $X'_{i1}$  and  $X'_{i2}$  are the adjusted counts of the life stage and species under consideration in the two replicate samples taken in p.s.u.  $i$ ,  $i=1, \dots, n$ . These adjusted counts are obtained for each replicate sample as the product of the observed density (in organisms per cubic meter) in that sample and the average volume of the two replicate samples in the pair:

$$\begin{aligned} X'_{i1} &= (X_{i1}/v_{i1})(v_{i1} + v_{i2})/2 \\ X'_{i2} &= (X_{i2}/v_{i2})(v_{i1} + v_{i2})/2 \end{aligned} \quad (2)$$

where  $X_{i1}$  and  $X_{i2}$  are the actual organism counts in the two replicate samples and  $v_{i1}$  and  $v_{i2}$  are the corresponding volumes of the two samples in p.s.u.  $i$ . It is assumed that the two replicate samples in any p.s.u. have volumes that differ by no more than 10%; in other words, the ratio of the larger sample volume to the smaller is at most 1.10. Sample volumes may vary without restriction from one p.s.u. to another.

The number of p.s.u.'s in which  $X_{i1} + X_{i2}$  (or equivalently  $X'_{i1} + X'_{i2}$ ) exceeds 0 must be at least 24 in order for this procedure to be used without further concern about its accuracy. It is important to note that the entire estimation process described here depends on  $\hat{c}^2$ . Replicate observations in which  $X_{i1} = X_{i2} = 0$  contain no information bearing on the estimation of  $c^2$  for the life stage and species of ichthyoplankton being

considered; there must be sufficiently many replicate observations (at least 24 pairs) in which  $X_{i1} + X_{i2}$  exceeds 0, which means that  $X_{i1}$ ,  $X_{i2}$ , or both are greater than 0. Unless this condition is met, the procedures presented here may estimate  $c^2$  with insufficient accuracy. For example, in the worst possible case, a particular life stage and species might have  $X_{i1} = X_{i2} = 0$  for every p.s.u. in which replicate sampling occurs; there is no way to use these methods in such a case. If there are insufficient replicate observations for the accurate estimation of  $c^2$ , the life stage and species will require further consideration in order to arrive at a value of  $\hat{c}^2$ . Methods for doing this are being developed. Methods for using triplicate data, in which three samples are taken in a given p.s.u., in the formula for  $\hat{c}^2$  are also being developed.

2. The next step is to treat the seasonal total number of organisms (of a specified life stage and species) entrained, T. Three cases must be considered: continuous sampling, continuous sampling with a small number of missing p.s.u.'s, and noncontinuous sampling.

#### A. Continuous sampling

The estimate of T is

$$\hat{T} = \sum_{i=1}^N \hat{T}_i = \sum_{i=1}^N (V_i/v_i)X_i \quad (3)$$

where

$V_i$  = volume of water in discharge channel in p.s.u. i (that is, the volume of water flowing past the sampling location in p.s.u. i)

$v_i$  = volume of water sampled in p.s.u. i

$X_i$  = number of organisms collected in sample in p.s.u. i

$N$  = number of p.s.u.'s in the entrainment season

$\hat{T}_i = (V_i/v_i)X_i$  = estimated number of organisms entrained in p.s.u. i

If replicate sampling has been performed in p.s.u. i, then  $v_i$  and  $X_i$  are totals over the replicates, that is,

$v_i = v_{i1} + v_{i2}$  = volume of sample 1 + volume of sample 2 in p.s.u. i

$X_i = X_{i1} + X_{i2}$  = organism count of sample 1 + organism count of sample 2 in p.s.u. i

$N$  will be greater than  $n$  (of step 1, formula (1)) unless replicate sampling is performed in every p.s.u.

The estimated variance of  $\hat{T}$  is

$$\hat{\text{Var}}(\hat{T}) = \sum_{i=1}^N \hat{\text{Var}}(\hat{T}_i) \quad (4)$$

where

$$\hat{\text{Var}}(\hat{T}_i) = (V_i/v_i)^2 \hat{\text{Var}}(X_i) \quad (5)$$

$$\hat{\text{Var}}(X_i) = \frac{X_i + \hat{c}^2 X_i^2}{1 + \hat{c}^2} \quad (6)$$

The estimated coefficient of variation of  $\hat{T}$  is

$$\hat{cV}(\hat{T}) = [\hat{\text{Var}}(\hat{T})]^{1/2} / \hat{T} \quad (7)$$

Confidence intervals are of the form

$$\hat{T} \pm b[\hat{\text{Var}}(\hat{T})]^{1/2} \quad (8)$$

where the coefficient  $b$  that gives an approximate 90% confidence interval is given by

$N$	$b$
6	3.0
8	2.5
16 or more	2.0

The value of  $b$  that gives 95% confidence intervals is being calculated.

#### B. Continuous sampling with a small number of missing p.s.u.'s

If the p.s.u.'s are ordered chronologically, assume that there is a gap in sampling between p.s.u.'s  $m$  and  $m'$ . For example, if a single p.s.u. is missing, then sampling occurs at p.s.u.'s  $m$  and  $m' = m+2$  and there is no sampling at p.s.u.  $m+1$ . If two consecutive p.s.u.'s are missing, then  $m' = m+3$  and no sampling occurs at  $m+1$  and  $m+2$ . To adjust for the missing p.s.u.'s, the terms from  $i=m+1$  to  $i=m'-1$  in the sum in formula (3) for  $\hat{T}$  must be replaced. The replacement terms will be linear combinations of the counts  $X_i$  from nearby p.s.u.'s in which sampling does occur.

Three methods of replacing the terms for p.s.u.'s in which there is no sampling will now be described. The first of these methods is the simplest to calculate, but this is because it does not utilize knowledge of  $V_i$  in missing p.s.u.'s; it is based on the procedure of finding the estimate  $\hat{T}_i$  for each missing p.s.u. by linear interpolation between  $\hat{T}_m$  and  $\hat{T}_{m'}$ . The second method improves on this by interpolating linearly between  $V_i(X_m/v_m) = (V_i/v_m)X_m$  and  $V_i(X_{m'}/v_{m'}) = (V_i/v_{m'})X_{m'}$ , to find  $\hat{T}_i$  for each missing p.s.u. The third method is much more general, allowing

the estimate  $\hat{T}_i$  for each missing p.s.u. to be any linear combination of the observed  $X_i$ 's in the p.s.u.'s during which sampling occurs. For practical reasons, the second of these three methods seems the best choice.

Method 1. In the sum of formula (3) that gives  $\hat{T}$ , leave the term  $\hat{T}_i$  unchanged unless p.s.u.  $i$  is adjacent to one or more missing p.s.u.'s (that is, p.s.u.'s in which sampling does not occur); when this happens at p.s.u.  $i$ , which is sampled, replace  $\hat{T}_i$  in (3) by

- (a)  $\frac{1}{2}(m'+1-i)\hat{T}_i$  when p.s.u.  $i$  is followed by missing p.s.u.'s up to (but not including) p.s.u.  $m'$ , at which sampling occurs, and p.s.u.  $i$  is not preceded directly by missing p.s.u.'s;
- (b)  $\frac{1}{2}(i+1-m'')\hat{T}_i$  when p.s.u.  $i$  is preceded by missing p.s.u.'s back to (but not including) p.s.u.  $m''$ , at which sampling occurs, and p.s.u.  $i$  is not followed directly by missing p.s.u.'s;
- (c)  $\frac{1}{2}(m' - m'')\hat{T}_i$  when p.s.u.  $i$  is preceded by missing p.s.u.'s back to (but not including) p.s.u.  $m''$  and is followed by missing p.s.u.'s up to (but not including) p.s.u.  $m'$ , at both of which sampling occurs;

remove  $\hat{T}_i$  from (3) for all p.s.u.'s  $i$  in which sampling does not occur. For convenience of notation, let  $\Sigma_i^+$  denote summation over all p.s.u.'s in which sampling occurs. We can express  $\hat{T}$  as

$$\hat{T} = \Sigma_i^+ a_i \hat{T}_i \quad (9)$$

where

$$a_i = \begin{cases} 1 & \text{when p.s.u.'s } i-1, i, \text{ and } i+1 \text{ are sampled} \\ \frac{1}{2}(m'+1-i) & \text{when p.s.u.'s } i-1, i, \text{ and } m' \text{ are sampled but} \\ & i+1, i+2, \dots, m'-1 \text{ are not} \\ \frac{1}{2}(i+1-m'') & \text{when p.s.u.'s } m'', i, \text{ and } i+1 \text{ are sampled but} \\ & m''+1, m''+2, \dots, i-1 \text{ are not} \\ \frac{1}{2}(m' - m'') & \text{when p.s.u.'s } m'', i, \text{ and } m' \text{ are sampled but} \\ & m''+1, \dots, i-1 \text{ and } i+1, \dots, m'-1 \text{ are not} \end{cases}$$

The formula for  $\hat{\text{Var}}(\hat{T})$  is, using the constants  $a_i$  just defined,

$$\hat{\text{Var}}(\hat{T}) = \Sigma_i^+ a_i^2 \hat{\text{Var}}(\hat{T}_i) \quad (10)$$

Method 2. In the sum of formula (3) that gives  $\hat{T}$ , leave the term  $\hat{T}_i$  unchanged if p.s.u.  $i$  and the adjacent p.s.u.'s  $i-1$  and  $i+1$  are all sampled. Omit the term  $\hat{T}_i$  from the sum if sampling does not occur in p.s.u.  $i$ . If p.s.u.  $i$  is adjacent to one or more missing p.s.u.'s, replace  $\hat{T}_i$  in (3) by  $a_i \hat{T}_i$ , giving

$$\hat{T} = \sum_i^+ a_i \hat{T}_i \quad (11)$$

where

$$a_i = \begin{cases} 1 & \text{when p.s.u.'s } i-1, i, \text{ and } i+1 \text{ are sampled} \\ \sum_{j=i}^{m'-1} \left( \frac{m'-j}{m'-i} \right) (V_j/V_i) & \text{when p.s.u.'s } i-1, i, \text{ and } m' \text{ are sampled} \\ & \text{but } i+1, \dots, m'-1 \text{ are not} \\ \sum_{j=m''+1}^i \left( \frac{j-m''}{i-m''} \right) (V_j/V_i) & \text{when p.s.u.'s } m'', i, \text{ and } i+1 \text{ are sampled} \\ & \text{but } m''+1, \dots, i-1 \text{ are not} \\ \sum_{j=m''+1}^i \left( \frac{j-m''}{i-m''} \right) (V_j/V_i) + \sum_{j=i+1}^{m'-1} \left( \frac{m'-j}{m'-i} \right) (V_j/V_i) & \text{when p.s.u.'s } m'', i, \text{ and } m' \text{ are sampled} \\ & \text{but } m''+1, \dots, i-1 \text{ and } i+1, \dots, m'-1 \\ & \text{are not} \end{cases}$$

The formula for  $\hat{\text{Var}}(\hat{T})$  is (10), but using the constants  $a_i$  defined for Method 2 just below (11).

Method 3. Let  $\sum_i^+$  denote summation over all p.s.u.'s in which sampling occurs and  $\sum_j^-$  summation over all p.s.u.'s in which sampling does not occur. For any p.s.u.  $j$  in which sampling does not occur, we can estimate the number of organisms entrained during the p.s.u. by a linear combination of the estimated entrainment  $\hat{T}_i$  in sampled p.s.u.'s:

$$\hat{T}_j = \sum_i^+ b_{ij} \hat{T}_i \quad (12)$$

The constants  $b_{ij}$  may be chosen to give the analysis of Method 1 or 2 above, or they may be chosen to give some other analysis deemed suitable. Then

$$\begin{aligned} \hat{T} &= \sum_{i=1}^N \hat{T}_i = \sum_i^+ \hat{T}_i + \sum_j^- \hat{T}_j \\ &= \sum_i^+ \hat{T}_i + \sum_j^- \sum_i^+ b_{ij} \hat{T}_i = \sum_i^+ [1 + \sum_j^- b_{ij}] \hat{T}_i \\ &= \sum_i^+ a_i \hat{T}_i \end{aligned} \quad (13)$$

where

$$a_i = 1 + \sum_j^- b_{ij} \quad (14)$$

The formula for  $\widehat{\text{Var}}(\widehat{T})$  is (10), but with the constants  $a_i$  defined in (14) rather than those defined for Method 1.

With the appropriate formulas for  $\widehat{T}$  and  $\widehat{\text{Var}}(\widehat{T})$  for whichever of the three methods is used, the estimated coefficient of variation and confidence intervals are computed from formulas (7) and (8).

### C. Noncontinuous sampling (of a systematic nature)

Let  $L$  be the number of days in the entrainment season and let  $h$  be the number of these on which sampling is performed. A sampling day is defined to be a 24-hour period in which samples are drawn from the discharge channel; this period is sampled as a sequence of  $N'$  p.s.u.'s ( $N' = 8$  for 3-hour p.s.u.'s,  $N' = 24$  for 1-hour p.s.u.'s, etc.). The remaining  $L - h$  unsampled days may form gaps of several different lengths.

Let  $d_j$  be the day of the season on which the  $j^{\text{th}}$  sampling day occurred. For example, if sampling occurred on days 1, 8, 12, ... then  $d_1=1$ ,  $d_2=8$ ,  $d_3=12$ , and so on. For each day  $d_j$ , the estimated entrainment on that day is given by formula (3) with  $N$  replaced by  $N'$ , the number of p.s.u.'s in the day, where the volumes  $V_i$  and  $v_i$  and the counts  $X_i$  are from the p.s.u.'s in that day. The estimated variance of this daily entrainment estimate  $\widehat{T}_j$ , which is denoted by  $\widehat{\text{Var}}(\widehat{T}_j)$ , is given by formula (4) with  $N$  again replaced by  $N'$ , using the volumes and counts from the p.s.u.'s in day  $d_j$ .

The season has now been divided into a sequence of days. For each day on which sampling occurred,  $\widehat{T}_j$  and  $\widehat{\text{Var}}(\widehat{T}_j)$  are known. All remaining days are days on which sampling did not occur. The analysis of the case of continuous sampling with missing p.s.u.'s is directly applicable to noncontinuous sampling by making one adjustment: days 1, 2, ...,  $L$  must be used instead of p.s.u.'s 1, 2, ...,  $N$  of the entrainment season. With this done and with  $\widehat{T}_j$  and  $\widehat{\text{Var}}(\widehat{T}_j)$  known for each sampled day, we can use interpolation of the sampled days to estimate entrainment on the days when sampling did not occur; this is exactly what was done for p.s.u.'s in the case of continuous sampling with missing p.s.u.'s.

Methods 1, 2, and 3 for continuous sampling with missing p.s.u.'s therefore carry over to the situation of noncontinuous sampling, except that the analysis here will be based on 24-hour sampling days rather than on shorter p.s.u.'s. The formulas already given for these three methods are directly applicable to noncontinuous sampling.

An alternative way to express Method 1, which is equivalent to the approach just described for noncontinuous sampling, is the following:

Define the weights

$$w_j = \begin{cases} (1 + d_2 - d_1)/2 & \text{for } j=1 \\ (d_{j+1} - d_{j-1})/2 & \text{for } j=2, 3, \dots, h-1 \\ (1 + d_h - d_{h-1})/2 & \text{for } j=h \end{cases} \quad (15)$$

The estimate of seasonal entrainment is then

$$\hat{T} = \sum_{j=1}^h w_j \hat{T}_j \quad (16)$$

and its estimated variance is

$$\hat{\text{Var}}(\hat{T}) = \sum_{j=1}^h w_j^2 \hat{\text{Var}}(\hat{T}_j) \quad (17)$$

This method is based on the use of linear interpolation of  $\hat{T}_j$ 's for days on which sampling occurred in order to obtain a  $\hat{T}_j$  for each day on which there was no sampling. It can therefore be improved in ways analogous to Methods 2 and 3 discussed for continuous sampling with a small number of missing p. s. u. 's.

3. The next step is to treat the entrainment reduction due to outage and the potential entrainment under full flow conditions. For any p. s. u.  $i$ , let  $V_i$  denote the realized discharge volume and let  $V_i + V_i'$  denote the potential discharge volume under full flow conditions. Similarly, let  $T_i$  denote the realized entrainment in p. s. u.  $i$  and  $T_i + T_i'$  the potential entrainment when the discharge volume is  $V_i + V_i'$ . The seasonal total reduction in entrainment,  $T'$ , is estimated by

$$\hat{T}' = \sum_{i=1}^N \hat{T}'_i \quad (18)$$

where

$$\hat{T}'_i = (V_i'/V_i) \hat{T}_i \quad (19)$$

For any p. s. u. in which sampling occurs, the estimated variance of  $\hat{T}'_i$  is obtained from the formula

$$\hat{\text{Var}}(\hat{T}'_i) = \hat{T}'_i{}^2 \left[ \frac{1}{\hat{T}_i} + \frac{1}{\hat{T}_i} + \frac{1/X_i + \hat{c}^2}{1 + \hat{c}^2} \right] \quad (20)$$

Note that if  $X_i = 0$ , then  $\hat{T}_i = \hat{T}'_i = 0$  and  $\hat{\text{Var}}(\hat{T}'_i) = 0$ .

Break the sum of formula (18) into sampled and unsampled p. s. u. 's, if there are any of the latter, along the lines of formula (13):

$$\hat{T}' = \sum_{i=1}^N \hat{T}'_i = \sum_i^+ \hat{T}'_i + \sum_j^- \hat{T}'_j = \sum_i^+ a_i' \hat{T}'_i \quad (21)$$

The estimated variance of  $\hat{T}'$  is then given by

$$\widehat{\text{Var}}(\hat{T}') = \sum_i^+ a_i'^2 \widehat{\text{Var}}(\hat{T}'_i) \quad (22)$$

When there are no missing p.s.u.'s, this reduces to

$$\widehat{\text{Var}}(\hat{T}') = \sum_{i=1}^N \widehat{\text{Var}}(\hat{T}'_i) \quad (23)$$

When missing p.s.u.'s are present, formula (21) can be evaluated by substituting specific expressions into the sum over missing p.s.u.'s  $\sum_j^- \hat{T}'_j$ . To do this, first find  $\hat{T}'_j$  for each p.s.u.  $j$  contained in a sampling gap, that is, for each p.s.u.  $j$  in which no sampling data are obtained. Let  $m$  and  $m'$  denote the (sampled) boundaries of the sampling gap containing p.s.u.  $j$ , so sampling occurs in p.s.u.  $m$  and then in p.s.u.  $m'$  but not in any of the p.s.u.'s between these, including  $j$ . For Method 1 (with continuous sampling and a small number of missing p.s.u.'s),

$$\hat{T}'_j = \left(\frac{m'-j}{m'-m}\right)\hat{T}'_m + \left(\frac{j-m}{m'-m}\right)\hat{T}'_{m'} \quad (24)$$

and for Method 2

$$\hat{T}'_j = \left(\frac{m'-j}{m'-m}\right)(v_j/v_m)\hat{T}'_m + \left(\frac{j-m}{m'-m}\right)(v_j/v_{m'})\hat{T}'_{m'} \quad (25)$$

Then derive  $\hat{T}'_j$  by using (19):  $\hat{T}'_j = (v'_j/v_j)\hat{T}'_j$ . For Method 2, combining (19) and (25) this way gives

$$\hat{T}'_j = \left(\frac{m'-j}{m'-m}\right)(v'_j/v_m)\hat{T}'_m + \left(\frac{j-m}{m'-m}\right)(v'_j/v_{m'})\hat{T}'_{m'} \quad (25')$$

Substituting these expressions for each missing p.s.u.  $j$  into (21) yields explicit terms  $a_i'$  in the right-hand side of this formula. The estimated variance of the resulting  $\hat{T}'$  is then found from formula (22).

The estimated coefficient of variation of  $\hat{T}'$  is

$$\widehat{\text{CV}}(\hat{T}') = [\widehat{\text{Var}}(\hat{T}')]^{1/2}/\hat{T}' \quad (26)$$

The potential total entrainment over the entire season,  $T+T'$ , is estimated by using  $\hat{T}$  from formula (3) and  $\hat{T}'$  from equation (18):

$$\widehat{T+T'} = \hat{T} + \hat{T}' = \sum_{i=1}^N (\hat{T}_i + \hat{T}'_i) = \sum_{i=1}^N \widehat{T_i + T'_i} \quad (27)$$

For each sampled p.s.u.  $i$ ,

$$\widehat{\text{Var}}(\widehat{T_i + T'_i}) = \widehat{\text{Var}}(\hat{T}_i) + \widehat{\text{Var}}(\hat{T}'_i) + 2(v'_i/v_i)\widehat{\text{Var}}(\hat{T}_i) \quad (28)$$

These estimated variances for individual p.s.u.'s can be combined to give the estimated variance of potential total entrainment over the entire season. When there are no missing p.s.u.'s, summing (28) over  $i=1, \dots, N$  gives

$$\widehat{\text{Var}}(\widehat{T+T'}) = \widehat{\text{Var}}(\hat{T}) + \widehat{\text{Var}}(\hat{T}') + 2 \sum_{i=1}^N (v'_i/v_i)\widehat{\text{Var}}(\hat{T}_i) \quad (29)$$

When missing p. s. u. 's are present, write

$$\begin{aligned}\widehat{T+T'} &= \sum_{i=1}^N \widehat{T}_i + \sum_{i=1}^N \widehat{T}'_i \\ &= \sum_i^+ \widehat{T}_i + \sum_j^- \widehat{T}_j + \sum_i^+ \widehat{T}'_i + \sum_j^- \widehat{T}'_j\end{aligned}\quad (30)$$

Express  $\widehat{T}_j$  for each missing p. s. u.  $j$  as a linear combination of values  $\widehat{T}_i$  for sampled p. s. u. 's  $i$ ; for example, formulas (24) and (25) do this for Methods 1 and 2. Similarly, express  $\widehat{T}'_j$  for each missing p. s. u.  $j$  by applying formula (19) as described in the discussion of evaluating formula (21) when there are missing p. s. u. 's. Adding the terms for actual and potential entrainment over both the sampled and the missing p. s. u. 's in formula (30) produces

$$\widehat{T+T'} = \sum_i^+ a_i \widehat{T}_i + \sum_i^+ a'_i \widehat{T}'_i \quad (31)$$

It is important that only the  $\widehat{T}_i$  and  $\widehat{T}_j$  terms from the right-hand side of (30) must be used in constructing the sum  $\sum_i^+ a_i \widehat{T}_i$  in (31), and that only the  $\widehat{T}'_i$  and  $\widehat{T}'_j$  terms from the right-hand side of (30) must be used in constructing the sum  $\sum_i^+ a'_i \widehat{T}'_i$  in (30). This segregation of terms is necessary because of the difference between  $\widehat{\text{Var}}(\widehat{T}_i)$  and  $\widehat{\text{Var}}(\widehat{T}'_i)$ . The variance contribution corresponding to p. s. u.  $i$  is estimated by

$$\begin{aligned}\widehat{\text{Var}}(a_i \widehat{T}_i + a'_i \widehat{T}'_i) &= a_i^2 \widehat{\text{Var}}(\widehat{T}_i) + a_i'^2 \widehat{\text{Var}}(\widehat{T}'_i) + \\ &\quad 2a_i a'_i (V'_i/V_i) \widehat{\text{Var}}(\widehat{T}_i)\end{aligned}\quad (32)$$

Summing these over all p. s. u. 's  $i$  in which sampling occurs, we have the estimated variance over the season

$$\widehat{\text{Var}}(\widehat{T+T'}) = \sum_i^+ \widehat{\text{Var}}(a_i \widehat{T}_i + a'_i \widehat{T}'_i) \quad (33)$$

The coefficient of variation of  $T+T'$  is estimated by

$$\widehat{\text{CV}}(\widehat{T+T'}) = [\widehat{\text{Var}}(\widehat{T+T'})]^{1/2} / (\widehat{T+T'}) \quad (34)$$

4. If mortality rather than entrainment is to be estimated, a mortality factor  $F_c$  must be included in the calculations. This mortality factor is affected by plant operating conditions, such as the number of pumps operating, water temperature, and so on. Consequently,  $F_c$  changes from one p. s. u. to another, and  $F_c$  during a given p. s. u. is affected by whether the plant is operating at full flow or at reduced flow. Accordingly, the mortality factor  $F_c$  must be indexed by the p. s. u.  $i$  and also by the flow condition or volume. Let  $F_{c,i}$  denote mortality in p. s. u.  $i$

at actual flow conditions and  $F_{c,i}^+$  denote mortality in p.s.u.  $i$  at full flow conditions. These will be equal when the plant is operating at full flow during p.s.u.  $i$ . Assume that  $F_{c,i}$  and  $F_{c,i}^+$  are known constants for each p.s.u.  $i$ . The formulas here for entrainment become estimates of mortality when  $F_{c,i}$  and  $F_{c,i}^+$  are inserted into the estimation process in appropriate fashion. It would be more realistic to treat  $F_{c,i}$  and  $F_{c,i}^+$  as unknown parameters, which must be estimated, and to factor the variance and bias of this estimation process into the mortality analysis (Vaughan and Kumar, 1981). Because of the complexity of this task, it will not be addressed here;  $F_{c,i}$  and  $F_{c,i}^+$  will be assumed to be known constants.

To illustrate this, let  $M$  denote the realized mortality during the actual entrainment season, and let  $M+M'$  denote the potential mortality during the season when the discharge volume is  $V_i + V_i'$  (rather than the realized discharge volume  $V_i$ ) in p.s.u.  $i$ ,  $i=1, \dots, N$ . Then  $M'$  is the difference between realized mortality and the potential mortality during the season at discharge volume  $V_i + V_i'$  in p.s.u.  $i$ . For continuous sampling (with no missing p.s.u.'s),

$$\hat{M} = \sum_{i=1}^N F_{c,i} \hat{T}_i \quad (35)$$

$$\widehat{M+M'} = \sum_{i=1}^N F_{c,i}^+ (\hat{T}_i + \hat{T}_i') \quad (36)$$

$$\begin{aligned} \hat{M}' &= \sum_{i=1}^N (F_{c,i}^+ \hat{T}_i + F_{c,i}^+ \hat{T}_i') - \sum_{i=1}^N F_{c,i} \hat{T}_i \\ &= \sum_{i=1}^N [(F_{c,i}^+ - F_{c,i}) \hat{T}_i + F_{c,i}^+ \hat{T}_i'] \end{aligned} \quad (37)$$

All three of these estimates have estimated variances that follow directly from the methods developed here, using (32), (20), and (5).

$$\widehat{\text{Var}}(\hat{M}) = \sum_{i=1}^N F_{c,i}^2 \widehat{\text{Var}}(\hat{T}_i) \quad (38)$$

$$\widehat{\text{Var}}(\widehat{M+M'}) = \sum_{i=1}^N F_{c,i}^+{}^2 \left[ (1 + 2V_i'/V_i) \widehat{\text{Var}}(\hat{T}_i) + \widehat{\text{Var}}(\hat{T}_i') \right] \quad (39)$$

$$\begin{aligned} \widehat{\text{Var}}(\hat{M}') &= \sum_{i=1}^N \left[ (F_{c,i}^+ - F_{c,i})^2 \widehat{\text{Var}}(\hat{T}_i) + F_{c,i}^+{}^2 \widehat{\text{Var}}(\hat{T}_i') + \right. \\ &\quad \left. 2(F_{c,i}^+ - F_{c,i}) F_{c,i}^+ (V_i'/V_i) \widehat{\text{Var}}(\hat{T}_i) \right] \end{aligned} \quad (40)$$

The adjustments for continuous sampling with a few missing p.s.u.'s or for noncontinuous sampling are straightforward. For example,

$$\hat{M} = \sum_i^+ F_{c,i} \hat{T}_i + \sum_j^- F_{c,j} \hat{T}_j \quad (41)$$

$$\widehat{M+M'} = \sum_i^+ F_{c,i}^+ \hat{T}_i + \sum_j^- F_{c,j}^+ \hat{T}_j + \sum_i^+ F_{c,i}^+ \hat{T}_i' + \sum_j^- F_{c,j}^+ \hat{T}_j' \quad (42)$$

Equations (41) for  $\hat{M}$ , (42) for  $\widehat{M+M'}$ , and the corresponding equation for  $\hat{M}'$  differ from equation (30) for  $\widehat{T+T'}$  only in having constants multiplied by the  $\hat{T}_i$ ,  $\hat{T}_j$ ,  $\hat{T}'_i$ , and  $\hat{T}'_j$  terms. Carrying these constants through the analysis of equations (30) to (34) results in estimated variances and coefficients of variation of the mortality seasonal totals  $\hat{M}$ ,  $\widehat{M+M'}$ , and  $\hat{M}'$  when there are missing p.s.u.'s or sampling is noncontinuous. The constants used in these seasonal mortality estimators to multiply the  $\hat{T}_i$ ,  $\hat{T}_j$ ,  $\hat{T}'_i$ , and  $\hat{T}'_j$  terms are different for each of the three mortality quantities ( $M$ ,  $M+M'$ , and  $M'$ ). All three of these sets of constants differ from the constants in (30) to (33), since they must now reflect the presence of the mortality factors  $F_{c,i}$  and  $F_{c,i}^+$  throughout.

5. We now extend the work just done in Sections 3 and 4 of this appendix. Our goal is to obtain an estimate of each of the six quantities in the table

	Actual	Reduction	Potential	
Entrainment	T	T'	T+T'	(43)
Mortality	M	M'	M+M'	

The estimated variances of the estimates of these quantities are also of major interest. "Actual" refers to actual flow conditions, "Reduction" refers to the reduction in entrainment or mortality due to the plant's operation at reduced flow rather than full flow, and "Potential" refers to the level that entrainment or mortality would have assumed if the plant had operated at full flow throughout the entrainment season.

Several complicating factors will be dealt with in the analysis of this section. These stem from the fact that each plant consists of two units that operate independently of each other. At Bowline Point, each unit has a separate cooling water system. At Indian Point, each unit has a separate intake structure, but there is a common discharge channel. The two units at Roseton share a common intake and discharge pipe. (Units 2 and 3 at Indian Point are considered here, but Unit 1, which has not operated for commercial production since October 1974, is not. This simplifies the presentation, allowing us to restrict attention to the two-unit case; however, handling three or more units in a plant would not be a problem, as we now note.) The treatment here of two units can be generalized in straightforward fashion to apply to a plant consisting of three or more units.

The complications related to the two-unit structure of each plant are:

1. Mortality rates may differ between the two units. Let  $F_{cij}$  and  $F_{cij}^+$  denote the mortality rate  $F_c$  in p.s.u.  $i$  at unit  $j$  under actual flow conditions and full flow conditions, respectively. It is possible that  $F_{cil} \neq F_{ci2}$ ,  $F_{cil}^+ \neq F_{ci2}^+$ , or both in any p.s.u.  $i$ .

2. The volumes of cooling water flowing through the two units during a specified p.s.u. may differ. Let  $V_{ij}$  denote the volume of cooling water that actually flows through unit  $j$  in p.s.u.  $i$ , and let  $V_i$  denote the volume of cooling water passing through the whole plant in p.s.u.  $i$ , so

$$V_i = V_{i1} + V_{i2} \quad (44)$$

Then it is possible that  $V_{i1}$  and  $V_{i2}$  are unequal. In particular, if unit  $j$  is not in operation in p.s.u.  $i$ , then  $V_{ij} = 0$ . In this case, the entire plant's cooling water flow for p.s.u.  $i$  passes through the other unit  $j'$  ( $j' \neq j$ ), that is,  $V_i = V_{ij'}$ .

3. Estimates both for the whole plant and for each unit within the plant are desired.

To address these issues within a unified framework, we consider the nature of the cooling water flow through a plant's two units and the discharge channel. The two units may have either a common intake structure or two separate intake structures. Similarly, they may have either a common discharge channel or two separate discharge channels (at the location of the entrainment sampling gear, which is the position of importance in any discussion of sampling). All water flowing through the plant during a specified p.s.u. can be classified as belonging to either the portion of the plant flow from which the entrainment sample is drawn or a portion of plant flow excluded from the volume from which this sample is drawn. Any volume of water that might have flowed through the plant but did not, because of reduced flow rather than full flow operation in p.s.u.  $i$ , is excluded from the volume out of which the entrainment sample is drawn. For example, recall that Bowline Point has two separate cooling systems; if a sample is drawn in p.s.u.  $i$  from the unit 1 discharge channel, the sample comes from the actual unit 1 discharge volume  $V_{i1}$ , while the actual unit 2 discharge volume  $V_{i2}$  and unrealized discharge volumes  $V'_{i1}$  and  $V'_{i2}$  are unsampled. (The unrealized discharge volume at unit  $j$  in p.s.u.  $i$ , denoted by  $V'_{ij}$ , is the difference between the full flow volume that unit  $j$  could have in p.s.u.  $i$  and the actual flow  $V_{ij}$ .) Roseton, on the other hand, has a common intake and discharge pipe, so both actual discharge volumes  $V_{i1}$  and  $V_{i2}$  are sampled; of course, the unrealized discharge volumes  $V'_{i1}$  and  $V'_{i2}$  are unsampled.

The key point of this discussion is that there is a crucial distinction, for entrainment and mortality estimation purposes, between the (actual) discharge flow from which the entrainment sample is drawn in p.s.u.  $i$  and the discharge flow, either realized or unrealized, that is excluded from the sampling process in p.s.u.  $i$ . The sampled flow is analyzed like the  $\hat{T}_i$  terms in  $\hat{T}$  and  $\hat{Var}(\hat{T})$  in equations (3) to (6); the unsampled flow is analyzed like the  $\hat{T}'_i$  terms in  $\hat{T}'$  and  $\hat{Var}(\hat{T}')$  in equations (18)

to (20) and (23); combining these to get  $T+T'$  follows the pattern of equations (27) to (29). Starting from this set of principles, we can incorporate the necessary adjustments for mortality estimation and for missing p.s.u.'s.

We now introduce some needed notation.

For each p.s.u.  $i$ , define

$U_i$  = volume of discharge flow in p.s.u.  $i$  from which entrainment sample is drawn

$U_{ij}$  = volume of discharge flow in p.s.u.  $i$  at unit  $j$  from which entrainment sample is drawn

$U'_i$  = volume of discharge flow in p.s.u.  $i$  excluded from entrainment sampling process (both realized and unrealized flow)

$U'_{ij}$  = volume of discharge flow in p.s.u.  $i$  at unit  $j$  excluded from entrainment sampling process (both realized and unrealized flow)

$V_i$  = volume of actual discharge flow in p.s.u.  $i$

$V_{ij}$  = volume of actual discharge flow in p.s.u.  $i$  at unit  $j$

$V'_i$  = volume of unrealized discharge flow in p.s.u.  $i$

$V'_{ij}$  = volume of unrealized discharge flow in p.s.u.  $i$  at unit  $j$

It follows immediately from these definitions that

$$\begin{aligned}
 U_i &= U_{i1} + U_{i2} \\
 U'_i &= U'_{i1} + U'_{i2} \\
 V_i &= V_{i1} + V_{i2} \\
 V'_i &= V'_{i1} + V'_{i2} \\
 U_{ij} + U'_{ij} &= V_{ij} + V'_{ij} \\
 U_i + U'_i &= V_i + V'_i
 \end{aligned} \tag{45}$$

We further define

$\hat{T}_i^s = (U_i/v_i)X_i$  = estimated entrainment in the sampled portion of the actual discharge flow in p.s.u.  $i$

$\hat{T}_i^u = ((V_i - U_i)/v_i)X_i$  = estimated entrainment in the unsampled portion of the actual discharge flow in p.s.u.  $i$

$\hat{T}_i = \hat{T}_i^s + \hat{T}_i^u = (V_i/v_i)X_i$  = estimated entrainment in the actual discharge flow in p.s.u.  $i$

$\hat{T}_{ij}^s = (U_{ij}/v_i)X_i$  = estimated entrainment in the sampled portion of the actual discharge flow in p.s.u.  $i$  at unit  $j$

$\hat{T}_{ij}^u = ([V_{ij} - U_{ij}] / v_i) X_i =$  estimated entrainment in the unsampled portion of the actual discharge flow in p.s.u. i at unit j

$\hat{T}_{ij} = \hat{T}_{ij}^s + \hat{T}_{ij}^u = (V_{ij} / v_i) X_i =$  estimated entrainment in the actual discharge flow in p.s.u. i at unit j

The sample volume  $v_i$  and the organism count  $X_i$  are totals over the replicates in p.s.u. i, as specified below equation (3).

To distinguish between entrainment at the whole plant and entrainment at an individual unit over the entire season, we use the notation

$\hat{T}$  = estimated seasonal entrainment at whole plant

$\hat{T}(\text{unit } j)$  = estimated seasonal entrainment at unit j

$\hat{T}'$  = estimated seasonal reduction in entrainment at whole plant

$\hat{T}'(\text{unit } j)$  = estimated seasonal reduction in entrainment at unit j

and similarly for  $\widehat{T + T'}$  and the corresponding mortality terms.

Continuous sampling: entrainment estimation, whole plant

For the entire plant, including both units,

$$\hat{T} = \sum_{i=1}^N \hat{T}_i = \sum_{i=1}^N (\hat{T}_i^s + \hat{T}_i^u) \quad (46)$$

To evaluate this, observe that

$$\widehat{\text{Var}}(\hat{T}_i^s) = (U_i / v_i)^2 \widehat{\text{Var}}(X_i) \quad (47)$$

$$\begin{aligned} \widehat{\text{Var}}(\hat{T}_i^u) &= \hat{T}_i^u \left( \frac{1}{\hat{T}_i^u} + \frac{1}{\hat{T}_i^s} + \frac{1/X_i + \hat{c}^2}{1 + \hat{c}^2} \right) \\ &= ([V_i - U_i] / v_i)^2 \widehat{\text{Var}}(X_i) + ([V_i - U_i] / v_i) (V_i / U_i) X_i \end{aligned} \quad (48)$$

where  $\widehat{\text{Var}}(X_i)$  is given by equation (6). Both of the estimated variances in (47) and (48) are 0 if  $X_i = 0$ . Then the estimated variance of  $\hat{T}$  is

$$\widehat{\text{Var}}(\hat{T}) = \sum_{i=1}^N \widehat{\text{Var}}(\hat{T}_i) \quad (49)$$

where

$$\begin{aligned} \widehat{\text{Var}}(\hat{T}_i) &= \widehat{\text{Var}}(\hat{T}_i^s) + \widehat{\text{Var}}(\hat{T}_i^u) + 2([V_i - U_i] / U_i) \widehat{\text{Var}}(\hat{T}_i^s) \\ &= (V_i / v_i)^2 \widehat{\text{Var}}(X_i) + ([V_i - U_i] / v_i) (V_i / U_i) X_i \end{aligned} \quad (50)$$

The reduction in entrainment for the whole plant,  $T'$ , is estimated by

$$\hat{T}' = \sum_{i=1}^N \hat{T}'_i \quad \text{where} \quad \hat{T}'_i = (V'_i/V_i)\hat{T}_i = (v'_i/v_i)X_i \quad (51)$$

and the variance of the estimate  $\hat{T}'_i$  is estimated by

$$\hat{\text{Var}}(\hat{T}') = \sum_{i=1}^N \hat{\text{Var}}(\hat{T}'_i) \quad (52)$$

where  $\hat{\text{Var}}(\hat{T}'_i)$  is given by equation (20) with  $\hat{T}_i$  replaced by  $\hat{T}_i^S$ .

Finally, we consider potential entrainment,  $T+T'$ :

$$\begin{aligned} \widehat{T+T'} &= \hat{T} + \hat{T}' = \sum_{i=1}^N (\hat{T}_i + \hat{T}'_i) = \sum_{i=1}^N [(U_i/v_i)X_i + (U'_i/v_i)X_i] \\ &= \sum_{i=1}^N [\hat{T}_i^S + (\hat{T}_i^u + \hat{T}'_i)] \end{aligned} \quad (53)$$

Using the fact that

$$\hat{\text{Var}}(\hat{T}_i^u + \hat{T}'_i) = (U'_i/v_i)([U_i+U'_i]/U_i)X_i + (U'_i/v_i)^2\hat{\text{Var}}(X_i) \quad (54)$$

which follows from equation (20) with  $\hat{T}_i$  and  $\hat{T}'_i$  replaced by  $\hat{T}_i^S$  and  $\hat{T}_i^u + \hat{T}'_i$ , respectively, we see from (28) that

$$\begin{aligned} \hat{\text{Var}}(\widehat{T_i+T'_i}) &= \hat{\text{Var}}(\hat{T}_i + \hat{T}'_i) = [1 + 2(U'_i/U_i)]\hat{\text{Var}}(\hat{T}_i^S) + \\ &\quad \hat{\text{Var}}(\hat{T}_i^u + \hat{T}'_i) \end{aligned} \quad (55)$$

and thus

$$\hat{\text{Var}}(\widehat{T+T'}) = \sum_{i=1}^N \hat{\text{Var}}(\widehat{T_i+T'_i}) \quad (56)$$

An alternative form of equation (56) is given by

$$\hat{\text{Var}}(\widehat{T+T'}) = \sum_{i=1}^N \left\{ ([U_i+U'_i]/v_i)^2\hat{\text{Var}}(X_i) + (U'_i/v_i)([U_i+U'_i]/U_i)X_i \right\} \quad (57)$$

#### Continuous sampling: entrainment estimation, one unit

We now present the formulas providing the results analogous to those developed in equations (46) to (57) but for only one of the plant's two units.

$$\hat{T}(\text{unit } j) = \sum_{i=1}^N \hat{T}_{ij} \quad (58)$$

$$\hat{\text{Var}}(\hat{T}_{ij}^S) = (U_{ij}/v_i)^2\hat{\text{Var}}(X_i) \quad (59)$$

$$\begin{aligned}\widehat{\text{Var}}(\widehat{T}_{ij}^u) &= \widehat{T}_{ij}^u \left( \frac{1}{\widehat{T}_{ij}^u} + \frac{1}{\widehat{T}_i^s} + \frac{1/X_i + \widehat{c}^2}{1 + \widehat{c}^2} \right) \\ &= ([v_{ij} - U_{ij}]/v_i)^2 \widehat{\text{Var}}(X_i) + \left( \frac{v_{ij} - U_{ij}}{v_i} \right) \left( \frac{v_{ij} + U_i - U_{ij}}{U_i} \right) X_i\end{aligned}\quad (60)$$

$$\widehat{\text{Var}}(\widehat{T}_{ij}) = (v_{ij}/v_i)^2 \widehat{\text{Var}}(X_i) + \left( \frac{v_{ij} - U_{ij}}{v_i} \right) \left( \frac{v_{ij} + U_i - U_{ij}}{U_i} \right) X_i \quad (61)$$

$$\widehat{\text{Var}}(\widehat{T}(\text{unit } j)) = \sum_{i=1}^N \widehat{\text{Var}}(\widehat{T}_{ij}) \quad (62)$$

$$\begin{aligned}\widehat{T}'(\text{unit } j) &= \sum_{i=1}^N \widehat{T}'_{ij} \quad \text{where} \quad \widehat{T}'_{ij} = (v'_{ij}/v_i) \widehat{T}_i \\ &= (v'_{ij}/v_i) X_i\end{aligned}\quad (63)$$

$$\widehat{\text{Var}}(\widehat{T}'(\text{unit } j)) = \sum_{i=1}^N \widehat{\text{Var}}(\widehat{T}'_{ij}) \quad (64)$$

where  $\widehat{\text{Var}}(\widehat{T}'_{ij})$  is given by (20) with  $\widehat{T}_i$  replaced by  $\widehat{T}'_{ij}$  and  $\widehat{T}_i$  by  $\widehat{T}_i^s$ .

$$\begin{aligned}\widehat{T + T}'(\text{unit } j) &= \widehat{T}(\text{unit } j) + \widehat{T}'(\text{unit } j) = \sum_{i=1}^N (\widehat{T}_{ij} + \widehat{T}'_{ij}) \\ &= \sum_{i=1}^N [(U_{ij}/v_i) X_i + (U'_{ij}/v_i) X_i] \\ &= \sum_{i=1}^N [\widehat{T}_{ij}^s + (\widehat{T}_{ij}^u + \widehat{T}'_{ij})]\end{aligned}\quad (65)$$

From (20) with  $\widehat{T}_i$  replaced throughout by  $\widehat{T}_{ij}^u + \widehat{T}'_{ij} = (U'_{ij}/v_i) X_i$ ,

$$\widehat{\text{Var}}(\widehat{T}_{ij}^u + \widehat{T}'_{ij}) = (U'_{ij}/v_i)^2 \widehat{\text{Var}}(X_i) + (U'_{ij}/v_i) ([v_i + U'_{ij}]/v_i) X_i \quad (66)$$

This leads to

$$\begin{aligned}\widehat{\text{Var}}(\widehat{T + T}'(\text{unit } j)) &= \sum_{i=1}^N \{ ([U_{ij} + U'_{ij}]/v_i)^2 \widehat{\text{Var}}(X_i) + \\ &\quad (U'_{ij}/v_i) ([v_i + U'_{ij}]/v_i) X_i \}\end{aligned}\quad (67)$$

Continuous sampling: mortality estimation, whole plant

$$\widehat{M} = \sum_{i=1}^N \widehat{M}_i \quad (68)$$

where

$$\begin{aligned}\widehat{M}_i &= F_{ci1} (\widehat{T}_{i1}^s + \widehat{T}_{i1}^u) + F_{ci2} (\widehat{T}_{i2}^s + \widehat{T}_{i2}^u) \\ &= ([F_{ci1} U_{i1} + F_{ci2} U_{i2}]/v_i) X_i + ([F_{ci1} (v_{i1} - U_{i1}) + F_{ci2} (v_{i2} - U_{i2})]/v_i) X_i \\ &= G_i^s \widehat{T}_i^s + G_i^u \widehat{T}_i^u\end{aligned}\quad (69)$$

with

$$G_i^S = [F_{ci1}U_{i1} + F_{ci2}U_{i2}]/U_i \quad (70)$$

$$G_i^u = [F_{ci1}(V_{i1}-U_{i1}) + F_{ci2}(V_{i2}-U_{i2})]/[V_i - U_i] \quad (71)$$

$$\widehat{\text{Var}}(\widehat{M}_i) = G_i^{S2}\widehat{\text{Var}}(\widehat{T}_i^S) + G_i^{u2}\widehat{\text{Var}}(\widehat{T}_i^u) + 2G_i^S G_i^u ([V_i - U_i]/U_i)\widehat{\text{Var}}(\widehat{T}_i^S) \quad (72)$$

$$\widehat{\text{Var}}(\widehat{M}) = \sum_{i=1}^N \widehat{\text{Var}}(\widehat{M}_i) \quad (73)$$

For potential mortality:

$$\widehat{M} + \widehat{M}' = \sum_{i=1}^N \widehat{M}_i + \widehat{M}'_i \quad (74)$$

where

$$\begin{aligned} \widehat{M}_i + \widehat{M}'_i &= F_{ci1}^+ (\widehat{T}_{i1}^S + \widehat{T}_{i1}^u + \widehat{T}'_{i1}) + F_{ci2}^+ (\widehat{T}_{i2}^S + \widehat{T}_{i2}^u + \widehat{T}'_{i2}) \\ &= ([F_{ci1}^+ U_{i1} + F_{ci2}^+ U_{i2}]/v_i) X_i + \\ &\quad ([F_{ci1}^+ (v_{i1} + v'_{i1} - U_{i1}) + F_{ci2}^+ (v_{i2} + v'_{i2} - U_{i2})]/v_i) X_i \\ &= H_i^{S\widehat{T}_i^S} + H_i^u (\widehat{T}_i^u + \widehat{T}'_i) \end{aligned} \quad (75)$$

with

$$H_i^S = [F_{ci1}^+ U_{i1} + F_{ci2}^+ U_{i2}]/U_i \quad (76)$$

$$H_i^u = [F_{ci1}^+ U'_{i1} + F_{ci2}^+ U'_{i2}]/U'_i \quad (77)$$

$$\widehat{\text{Var}}(\widehat{M}_i + \widehat{M}'_i) = H_i^{S2}\widehat{\text{Var}}(\widehat{T}_i^S) + H_i^{u2}\widehat{\text{Var}}(\widehat{T}_i^u + \widehat{T}'_i) + 2H_i^S H_i^u (U'_i/U_i)\widehat{\text{Var}}(\widehat{T}_i^S) \quad (78)$$

$$\widehat{\text{Var}}(\widehat{M} + \widehat{M}') = \sum_{i=1}^N \widehat{\text{Var}}(\widehat{M}_i + \widehat{M}'_i) \quad (79)$$

For reduction of mortality:

$$\widehat{M}' = \widehat{M} + \widehat{M}' - \widehat{M} = \sum_{i=1}^N [I_i^S \widehat{T}_i^S + I_i^u (\widehat{T}_i^u + \widehat{T}'_i)] \quad (80)$$

where

$$I_i^S = H_i^S - G_i^S \quad (81)$$

$$I_i^u = H_i^u - G_i^u (V_i - U_i)/U'_i \quad (82)$$

$$\widehat{\text{Var}}(\widehat{M}') = \sum_{i=1}^N [I_i^{S2}\widehat{\text{Var}}(\widehat{T}_i^S) + I_i^{u2}\widehat{\text{Var}}(\widehat{T}_i^u + \widehat{T}'_i) + 2I_i^S I_i^u (U'_i/U_i)\widehat{\text{Var}}(\widehat{T}_i^S)] \quad (83)$$

In the above formulas, the squares of the coefficients  $G_i^S$ ,  $H_i^u$ ,  $I_i^S$ , etc. are written without parentheses or brackets: these squares appear as  $G_i^{S2}$ ,  $H_i^{u2}$ ,  $I_i^{S2}$ , etc. rather than as  $(G_i^S)^2$ ,  $(H_i^u)^2$ ,  $(I_i^S)^2$ , etc.

Continuous sampling: mortality estimation, one unit

In the analysis of continuous sampling mortality estimation for the whole plant, formulas (68) to (83), omit every term in which the unit not of interest appears explicitly. For example, if unit 2 is of interest, then terms involving unit 1 should be omitted, which causes (69), (70), and (71) to become

$$\begin{aligned}\hat{M}_i &= F_{ci2}(\hat{T}_{i2}^s + \hat{T}_{i2}^u) \\ &= (F_{ci2}U_{i2}/v_i)X_i + (F_{ci2}[V_{i2}-U_{i2}]/v_i)X_i \\ &= G_i^s \hat{T}_i^s + G_i^u \hat{T}_i^u\end{aligned}\quad (84)$$

with

$$G_i^s = F_{ci2}U_{i2}/U_i \quad \text{and} \quad G_i^u = F_{ci2}(V_{i2}-U_{i2})/(V_i - U_i) \quad (85)$$

When these values of the coefficients (the G's, H's, and I's) are used, we obtain the mortality estimates for the single unit of interest.

Noncontinuous sampling: entrainment estimation

Here we treat both sampling cases of concern: continuous sampling with a small number of missing p.s.u.'s, and noncontinuous sampling of a systematic nature. The two situations of whole plant estimation and single unit estimation are, similarly, variations on the theme that will now be discussed.

When there are some p.s.u.'s in which sampling does not occur, estimates of entrainment in the missing p.s.u.'s must be constructed from the data pertaining to the sampled p.s.u.'s. For example, equation (3) can no longer be used to determine  $\hat{T}$ ; it must be replaced by (9), (11), or (13). Extending this idea to the analyses of entrainment and mortality for the whole plant and for a single unit, analogous changes must be made in order to handle missing p.s.u.'s. For instance, equation (46) for continuous sampling, whole plant entrainment estimation must be replaced by an equation of the form

$$\hat{T} = \sum_i^+ a_i \hat{T}_i = \sum_i^+ [a_i^s \hat{T}_i^s + a_i^u \hat{T}_i^u] \quad (86)$$

Correspondingly, equations (51) and (53) for  $\hat{T}'$  and  $T+T'$  must be replaced by equations of the form

$$\hat{T}' = \sum_i^+ a_i \hat{T}_i' \quad (87)$$

$$\widehat{T+T'} = \sum_i^+ [a_i^s \hat{T}_i^s + a_i^u (\hat{T}_i^u + \hat{T}_i')] \quad (88)$$

The method of formulas (31) to (33) can then be used to obtain the variance of any of these estimators. No conceptual problems arise with

this approach: we select a subset of the N p.s.u.'s consisting of those p.s.u.'s during which entrainment sampling occurred, then decide on weights to combine with these data in order to obtain estimates of entrainment over N p.s.u.'s from the smaller number of p.s.u.'s that were sampled, and finally compute variances along the lines of formulas (32) and (33).

#### Noncontinuous sampling: mortality estimation

As with entrainment estimation under noncontinuous sampling, we treat simultaneously the case of a few random missing p.s.u.'s and systematic noncontinuous sampling, and also the cases of whole plant and single unit estimation. The general method is very similar to that just presented for entrainment estimation; the only difference is that the mortality factors  $F_{ci1}^-$ ,  $F_{ci2}^+$ , and so on must be included in the calculations, which affects the values taken on by the weights  $a_i$ . For example, with missing p.s.u.'s (68) becomes

$$\hat{M} = \sum_i^+ \hat{M}_i + \sum_j^- \hat{M}_j = \sum_i^+ a_i \hat{M}_i = \sum_i^+ [a_i^s \hat{T}_i^s + a_i^u \hat{T}_i^u] \quad (89)$$

In the same way,

$$\begin{aligned} \widehat{M+M'} &= \sum_{i=1}^N [F_{ci1}^+ (\hat{T}_{i1}^s + \hat{T}_{i1}^u + \hat{T}'_{i1}) + F_{ci2}^+ (\hat{T}_{i2}^s + \hat{T}_{i2}^u + \hat{T}'_{i2})] \\ &= \sum_i^+ [a_i^s \hat{T}_i^s + a_i^u (\hat{T}_i^u + \hat{T}'_i)] \end{aligned} \quad (90)$$

The mortality factors are reflected in the values of the weights  $a_i^s$  and  $a_i^u$ . Once these weights have been determined, we proceed exactly as in the estimation of entrainment.

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**ADDENDUM**  
**to**  
**EVALUATION OF ENTRAINMENT ABUNDANCE SAMPLING DESIGNS**

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## INTRODUCTION

This Addendum reviews selected aspects of entrainment abundance sampling performed at Indian Point, Roseton, and Bowline Point Generating Stations since 1983. The emphasis is on the information obtained from these sampling programs about the pattern of variation in density of entrained ichthyoplankton in the discharge channel. For convenience, the term "discharge channel" will often be used to mean either a discharge channel or a discharge pipe, whichever is present at the generating station. The sampling programs in specified years at these generating stations are discussed, with emphasis on the results as they pertain to sampling issues. The concluding section considers what has been learned, what information is still needed, and what directions would be fruitful for future work.

Recent progress on the problem of estimating confidence intervals for entrainment abundance sampling programs is examined here. In the report to which this Addendum is attached, the complexity of the statistical estimation problem is described in detail. The presence of greater variability in ichthyoplankton counts than is consistent with a Poisson distribution is shown, and methodology for handling the variation is presented. This additional variation leads to the introduction of the parameter  $c^2$  in the quadratic variance model, which is given by the variance formula

$$\sigma^2 = \mu + c^2\mu^2 . \quad (1)$$

The value of the parameter  $c^2$  reflects the level of extra-Poisson variability present. This variance formula appeared in Sections 4.2.2 and 4.5 of the report preceding this Addendum. Except for this Addendum, the report has been available in draft form since July, 1986; it has been cited by several of the reports discussed in this Addendum as Casella *et al*, 1986.

Estimating the level of variability in organism counts requires the performance of replicate sampling, in which two (or more) sampling intakes draw simultaneous samples from each cooling water discharge under identical conditions from the discharge channel. Data from these replicate samples can be used to compute the estimate  $\hat{c}^2$  of the parameter  $c^2$ . The phrase "identical conditions" implies that each set of replicate samples must be drawn at locations within the discharge flow where exactly the same process is occurring during the sampling period. That is, the distribution of possible organism densities is identical at all

sampling locations. If the depths within the discharge flow, the distances to the channel wall, or other differences between the sampling locations result in nonidentical conditions, the simultaneous samples will not be true replicates.

The estimator  $\hat{c}^2$  is a method of moments estimator based on the variance formula (1), and is therefore applicable to any distribution satisfying this relationship. The negative binomial distribution is the most tractable of these distributions, and a rationale for using it as the alternative model to the Poisson is detailed in Section 4.2.1. It is shown there that a gamma mixture of Poissons has the negative binomial distribution. On the basis of this argument, the alternative to the Poisson is often referred to in this Addendum as the negative binomial model. However, there may also be other distributions suitable for modeling the extra-Poisson variability in ichthyoplankton counts present when (1) holds with  $c^2 > 0$ . For example, one possibility is a lognormal mixture rather than a gamma mixture of Poissons.

The extra-Poisson variation results in wider confidence intervals than the standard Poisson model. In order to determine how wide these confidence intervals must be, the degree of extra-Poisson variation, quantified by the parameter  $c^2$ , must be estimated from the data for each taxon/life stage of interest. This increases the difficulty of the sampling problem, because only nonzero replicate samples contribute information to the estimation of  $c^2$ . Consequently, sampling must be performed in a way that results in enough nonzero replicates to provide accurate estimation of  $c^2$  for each taxon/life stage of interest. Unless sufficiently many nonzero replicate samples are available for these taxa/life stages, another approach applicable to these undersampled taxa/life stages must be taken.

The pattern of variability in the discharge channel is a major focus of this work. It is important to note that there are two distinct types of departure from the Poisson model of completely random distribution of organisms within the discharge flow are possible. These are:

I. Stratification within the discharge channel. There may be relatively homogeneous zones within the discharge channel, that is, zones in which simultaneously drawn observations are likely to be more similar than simultaneously drawn observations from the entire channel.

II. Clumping of organisms. Within each stratum, or within the entire discharge channel if there is no stratification, organisms may form clumps or clusters rather than exhibiting a random Poisson distribution.

These two types of possible departure from the null hypothesis of Poisson (i.e., randomly) distributed organisms lead to four combinations:

1) No stratification and no clumping. These conditions satisfy the null hypothesis of Poisson distributed counts, so the parameter  $c^2$  equals 0. Any two simultaneous samples in the discharge channel will be true replicates, with a common mean  $\mu_1 = \mu_2$ .

2) Clumping but no stratification. The distribution of clumps of organisms across the discharge channel is random, so simultaneous samples will have a common mean  $\mu_1 = \mu_2$ ; however, clumping produces greater variability than the Poisson distribution, so  $c^2 > 0$ .

3) Stratification but no clumping. Because the discharge channel contains strata, or relatively homogeneous zones that differ from one another, two sampling situations must be distinguished.

– If simultaneous sampling takes place within a single stratum, as occurs when two (or more) samples are drawn from locations that are very close to one another in the discharge channel, the situation appears to reduce to 1) above, no stratification and no clumping. Drawing two (or more) samples from a single stratum makes it impossible to detect the presence of other strata.

– If simultaneous samples are drawn from different strata, they can no longer be assumed to have a common mean, so  $\mu_1 \neq \mu_2$ ; this difference in means across strata produces greater-than-Poisson variability between observed counts from different strata, so  $c^2 > 0$ .

4) Both stratification and clumping. As in 3), two sampling situations must be distinguished.

– If simultaneous sampling takes place within a single stratum, the situation appears to reduce to 2) above, clumping but no stratification. As was just noted in 3), drawing the two (or more) samples from a single stratum makes it impossible to detect the presence of other strata.

– If simultaneous samples are drawn from different strata, they do not have a

common mean, so  $\mu_1 \neq \mu_2$ ; both the clumping within strata and the difference in means across strata produce greater-than-Poisson variability between observed counts from different strata, so  $c^2 > 0$ .

With the proper experimental set-up, the patterns outlined above for  $\mu$ 's and  $c^2$  can be detected through careful statistical analysis. For example, if the magnitude of variability among sets of randomly located replicate samples yields  $\hat{c}^2 \approx 0$ , then we might conclude that  $c^2 \approx 0$  and that there is no stratification. From the above categorizations, this would support the fact of the organisms being randomly distributed in the discharge channel. If, instead, the statistical evidence supports  $c^2 > 0$ , then different situations may exist. For instance, stratification is a serious type of departure from randomness because it can lead to biased entrainment estimates; if a carefully chosen experiment is performed under the right conditions and is analyzed under the right assumptions, stratification can be detected from differences among mean densities from fixed sampling locations. Furthermore, situations other than those outlined above can occur, affecting  $\hat{c}^2$  and tests of  $\mu_1 = \mu_2$ . For example, an inflated value of  $c^2$  could result from inherent errors in the measurement process (such as extrusion, mutilation, non-detection, or misidentification).

In summary, the presence or absence of stratification, the presence or absence of clumping, and the sampling pattern of replication within or across strata (when strata are present) must be considered in assessing the variability in the discharge channel. If statistical analyses support the claim that  $c^2=0$ , then stronger conclusions can be drawn than if the claim  $c^2 > 0$  is supported. In this latter case determination of the exact cause of the "extra-Poisson" variability is quite important. Such determinations can only be made with carefully designed experiments.

The observations collected in entrainment abundance studies are dependent on the site and conditions. Observations taken at different locations along the discharge channel or under different flow conditions can vary greatly. The same is true for the organism counts of different taxa/life stages. Similarly, data collected at different generating stations can lead to very different conclusions.

Entrainment abundance sampling involves several major difficulties inherent in the process of field collection and laboratory analysis. One is that the locations of the sampling

intakes in the discharge channel must be fixed; engineering constraints limit the possible positions of the sampling intakes, and prevent moving the sampling intakes in order to randomize their locations during the entrainment season. The high velocity of the discharge flow is another physical problem, made more complicated by its variability over time, location in the channel, and conditions. Bias in the test of randomness may be unavoidable. The classification model in Section 4.5 of this report raises this issue. The discussion later in this Addendum of Appendix F of the 1984 Bowline Point report and Young (1988) also bear on the problem of bias. Sorting, identification, and extrusion of the ichthyoplankton in the samples collected in the field can introduce errors whose ramifications are substantial. A final sampling issue is the possibility of splitting some or all samples into subsamples, and counting ichthyoplankton in only a subset of these according to a carefully specified rule. Splitting is possible, and may under some conditions be an effective technique for improving the accuracy of estimation attainable for a given total cost; the reduced cost per sample achieved through splitting should allow increased replication. For instance, if dividing a sample into halves and examining one of them provides almost as much information as the whole sample, then counts from two half-samples taken from two separate samples may provide much more information than one count of a whole sample at little or no extra expense. Furthermore, it may also be true that counting and sorting precision in splits is subject to relatively smaller measurement error than for full samples. This aspect of splitting is analogous to the improved quality of data seen by the Census Bureau in sample surveys as compared to the Census. More work must be done to establish the conditions that are favorable for sample splitting.

This introductory section closes with discussions of three important methodological issues that arise in the study of entrainment abundance.

#### *A Justification for the Logarithmic Transformation of ANOVA Data*

The rationale for the use of the logarithmic transformation for ANOVA purposes is instructive, so it will now be presented briefly. A fundamental assumption of ANOVA is that all observations have equal variances; performing an ANOVA on data that violate this assumption may well lead to erroneous conclusions (for a thorough discussion, see Scheffe, 1959, Sec. 10.2 and 10.4). It has therefore become common to transform data in which the

variances of observations are known to be unequal, in an attempt to equalize them.

The variance stabilizing transformation of an observed random variable (in this case, an observed count  $x$ ) is defined as the transformation  $z = f(x)$  whose standard deviation is equal, at least approximately, to a specified constant  $\sigma_z$ . As pointed out by Scheffe (1959, Sec. 10.7), this transformation is obtained by solving the equation

$$f(x) = \sigma_z \int \frac{dx}{\phi(x)} \quad (2)$$

where the function  $\phi$  expresses the standard deviation  $\sigma_x$  of an observed count  $x$  as a function of the mean  $\mu$ . For example, this transformation becomes  $f(x) = \sqrt{x}$  for data from a Poisson distribution; in the case where per cent error is constant, it becomes the logarithmic transformation,  $f(x) = \ln(x)$ , which results from substituting  $\sigma_x = \phi(\mu) = c\mu$  into (2). In the more complex situation of interest here, equation (1) gives

$$\sigma_x = \phi(\mu) = \sqrt{\mu + c^2\mu^2}, \quad (3)$$

so setting the arbitrary constant  $\sigma_z$  equal to 1 yields

$$f(x) = \int \frac{dx}{\sqrt{x + c^2x^2}}. \quad (4)$$

Looking up this integral leads immediately to

$$f(x) = \frac{1}{c} \ln \left\{ \frac{1+2c^2x}{2c} + \sqrt{x+c^2x^2} \right\}. \quad (5)$$

If  $\mu$  is sufficiently large, then  $x$  is also large with probability close to 1, so that  $\sqrt{x+c^2x^2} \approx cx$ , and therefore

$$f(x) \approx \frac{1}{c} \ln \left\{ 2c \left( x + \frac{1}{4c^2} \right) \right\} = \frac{1}{c} \ln(2c) + \frac{1}{c} \ln \left( x + \frac{1}{4c^2} \right). \quad (6)$$

This shows that the transformation (5) is roughly equivalent to the logarithm of (count + constant) when  $\mu$  is large. This derivation applies to both natural logarithms (usually denoted by  $\ln$ , as above, or  $\log_e$ ) and logarithms taken with base 10 (usually denoted by

$\log_{10}$ ), since the identity

$$\ln x = \log_{10}x \cdot \ln 10 = 2.30259 \log_{10}x \quad (7)$$

holds for any positive number  $x$ .

This demonstration is a justification for using the logarithmic transformation with ANOVA data. It is applicable when  $\mu$  is large and the quadratic variance formula (1), or equivalently (3), holds. Under different conditions, such as a variance formula other than (1), the logarithmic transformation would not be a good approximation to the variance stabilizing transformation. This makes it clear that using the  $\log_{10}(x+1)$  transformation for ANOVA data requires attention to the conditions under which it is applied. It is *not* an automatic device that always solves the problem of variance heterogeneity.

A final note of caution about variance stabilizing transformations is in order. Scheffe (1959, Sec. 10.7) mentioned the belief "that if a transformation equalizes the variances it is also likely to reduce nonnormality and nonadditivity." He forcefully demonstrated that this belief can be incorrect by presenting an example, involving observations drawn from the Poisson distribution, in which additivity and equality of variance are irreconcilable, that is, equality of variance can be obtained only by sacrificing additivity.

#### *Sampling Gear and Abundance Estimation*

The method of sample collection, i.e., net or pump, may have an effect on the estimation of entrainment abundance in a flowing media. This is because the volume of discharge water that passes through the cross-section of the sampling device depends on many factors. One of these is the velocity of the discharge flow, which differs from one location to another in the discharge channel, being slower near the walls than near the center. Another is the ratio of the sample volume to the discharge volume, which is represented as  $v/V$  or  $v(t)/V(t)$ , where  $t$  denotes the time at which a primary sampling unit occurs. This ratio may also depend on the sampling gear: net sampling results will be identical to pump sampling results only if the pump mechanism is able to withdraw water from the sampling pipe by matching the discharge flow exactly in local velocity, never drawing either faster or

slower than the flow immediately surrounding the pipe.

As discussed earlier in this report (pp. 9.2-9.3), the sample volumes should be regulated to make the ratio  $v(t)/V(t)$  as nearly constant across time as possible for each plant. When replicate sampling takes place, the same restriction applies equally to each individual sampling unit. At each sampling station, the velocity of water through the sampling orifice should be equal to the local velocity of the adjacent water. The consequences of departing from this pattern of equal  $v/V$  ratios are of unknown magnitude. They could be investigated by simulation modeling, based on different assumptions about the nature of the discharge flow and the behavior of sampling gear of various kinds.

This set of issues needs further consideration; its resolution is beyond the scope of this Addendum. Current results on estimation of entrainment abundance could be modified to reflect the results of such an investigation.

#### ***Random Variability, Uniform Variability, and Replicate Sampling***

The need for replicate sampling is based on a simple but far-reaching observation: without replicate sampling, there is no way to model the variability in the discharge channel. Without replication, the assumption of Poisson variability or some similar highly structured situation is unavoidable. Such an assumption may or may not be justified, but one has no way of knowing how consistent (or inconsistent) the assumption is with the data, unless some replicate sampling has been performed, allowing the analyst to test the fit of the data to the assumption.

It is a commonly stated misconception that Poisson variability in replicate sample counts inevitably results from *random* variability in the channel. It is important to note that Poisson variability results from a very specific type of random variability, *uniform* variability, which occurs when every location in the cross-section of the channel has the same (i.e., uniform) probability of containing an organism. When this is true, ichthyoplankton are said to be randomly dispersed within the channel cross section. Other types of random variability can lead to non-Poisson distributions, such as the negative binomial or lognormal, which may occur when ichthyoplankton are clumped. Without replicate sampling, the exact type of

variability cannot be determined. For example, it may be possible to have different levels of uniform variability in different strata. This would lead to Poisson distributions within a stratum, but the discharge channel as a whole would not exhibit Poisson variability. This is a possibility that can only be detected with replicate sampling. An adequate description of the variability in the channel is needed in order to place accurate confidence limits on the entrainment estimates.

### **REVIEW OF SELECTED POST-1983 STUDIES**

Several recent studies of entrainment abundance sampling will now be discussed. These are limited to those studies selected by Consolidated Edison because they involved replicate sampling. Some of these studies are taken from the annual reports of Indian Point, Bowline Point, and Roseton Generating Stations. Others are articles relating to replicate sampling.

#### ***Indian Point Generating Station Entrainment Abundance Program 1985 Annual Report, Appendices G and I***

Appendix G is a small but well conceived study of the reliability of the sample splitting procedure. It was conducted by counting all eight splits of each sample instead of only one split, and then examining the variability of ichthyoplankton and amphipod counts among the eight splits. The study was repeated here on two sets of three samples, but under somewhat different protocols; the nonstandard precautions taken in the second set may make these data unrepresentative of standard splits. Under both protocols, tests of splitting replicability revealed higher variability among splits than expected from random chance. The observed degree of variability among splits was described as comparable to the subsampling variability found in several earlier studies considered by Van Guelpen *et al* (1982). This source of error in estimates of organism density appears to be relatively unimportant, because (1) high variability among splits seems most likely to occur with high counts, which lead to low percent errors, and (2) the sampling error resulting from the high variability in the densities is much greater. This study merits repetition and elaboration. What is missing in the design is duplicate counts of the same sample, by different people, to provide a measure of this

source of variance. It is important to note that splitting is a form of subsampling, *not* replicate sampling: the analysis of two (or more) splits obtained from a single sample is quite different from the analysis of two (or more) replicate samples.

Appendix I treats the estimation of the quadratic variance parameter  $c^2$ , which is needed for the calculation of entrainment confidence limits. These limits are required for 16 taxa/life stages, of which only four occurred in at least 40 pairs of nonzero replicate samples, the number of pairs recommended for accurate estimation of  $c^2$ . The Appendix considers the estimation of generalized  $c^2$  values, applicable even when there are fewer than 40 pairs of replicate samples for a given taxon/life stage. It treats this shortage of replicate observations by proposing that different taxa may have a common value of  $c^2$  for each life stage. Generalized values of  $\hat{c}^2$ , pooled over taxa for each life stage, are developed for eggs, post yolk-sac larvae, and juveniles; these values are consistent with data for individual taxa with both large ( $\geq 40$ ) and small ( $< 40$ ) numbers of nonzero replicate pairs.

Results of the study of split replicability indicated that at higher densities the splits exhibit greater than equi-multinomial (Poisson) variation, even under the precautions taken with the second set of samples. For a test of given size  $\alpha$ , where the size is defined as the probability  $P(\text{reject } H_0: \text{Poisson distribution} \mid H_0 \text{ is true})$ , the power of the test is an increasing function of the density of organisms. The magnitude of the excess in variance over the Poisson model can be measured by  $\hat{c}^2$ . This requires use of the generalized formula for  $\hat{c}^2$ , for arbitrary sample size  $n$  (in the case here,  $n = 8$ ) rather than simply  $n=2$ , given by

$$\hat{c}^2 = (s^2 - \bar{x}) / (\bar{x}^2 - s^2/n) . \quad (8)$$

This formula is a more general version of the formula for  $\hat{c}^2$  given in Chapter 4 of the report to which this Addendum is attached. In terms of Fisher's  $D^2$  statistic (see, for example, Haas and Heller, 1986)

$$D^2 = \sum_{i=1}^n (x_i - \bar{x})^2 / \bar{x} , \quad (9)$$

the formula for  $\hat{c}^2$  in (8) can be written as

$$\hat{c}^2 = [D^2 - (n - 1)] / [(n - 1)\bar{x} - D^2/n] . \quad (10)$$

Under the beta-multinomial model, the sampling distribution of this estimator is closely approximated by assuming that

$$D^2 (1 + \bar{x}c^2) / (1 + c^2/n) \sim \chi_{n-1}^2 , \quad (11)$$

that is, the expression in (11) has a chi-squared distribution with  $n-1$  degrees of freedom, where  $c^2$  is now a parameter of the distribution. The validity of this approximation in the beta-multinomial case has been illustrated by enumeration of the exact sampling distribution of the expression in (11). As a consequence, we may replace  $n-1$  in (10) by the 5% and 95% percentiles of the chi-squared distribution to obtain 90% confidence limits for  $c^2$ .

Note also that  $\hat{c}^2$  is unbounded when there is only one nonzero count among the  $n$  counts, and  $\hat{c}^2$  is negative, and should be truncated to zero, when  $s^2 < \bar{x}$  (an occurrence that indicates low sample variability).

***Indian Point Generating Station Entrainment Abundance Program 1986 Annual Report, pages 91-110***

This report addresses many important issues raised in an earlier draft of Casella *et al* (1986). A reasonable method of gathering data is described, and the appropriate analyses are performed to estimate  $c^2$ . The estimate  $\hat{c}^2$  is then used to construct confidence limits on the number of a given species entrained. (Using the assumption that the variance of the number of a species observed in a sample satisfies the relationship (1), we see that the quantity  $c^2$  is a variance parameter.)

For a detailed description of sampling procedures at Indian Point, see Sections 1.2.2 and 1.3 and Figure 1.3 in Chapter 1.

There were eight taxa/life stages with enough replicate data to yield good  $c^2$  estimates, based on guidelines set down by Casella *et al* (1986). In all, there were 688 replicate samples in the channel, which is a huge increase over the amount of replicate sampling done in the past.

For eight *key species*, defined in the report (Table 4.4-1, page 96), the estimates of the parameter  $c^2$  were very consistent. It was argued convincingly, based on the small variability among these estimates, that considering  $c^2$  to be constant among these species was very reasonable. For the other, non-key, species,  $\hat{c}^2$  was more variable, so  $c^2$  could not be considered constant. This higher variability could also be a function of a lower sample size, as was noted in the report.

Furthermore, for most of the key species, the estimated value of  $c^2$  was quite close to zero. Since a  $c^2$  value of zero indicates that the Poisson model is appropriate, the report discusses the viability of the Poisson model, which was rejected by Casella *et al* (1986) in favor of the more variable negative binomial model. Although the 1986 Indian Point report concludes that the Poisson model may be viable, it notes at least two other explanations. First, the sample size is much larger than before, so variability in the estimates of  $c^2$  is reduced. Taking this smaller variance of  $\hat{c}^2$  into account, smaller values of  $\hat{c}^2$  would tend to favor the Poisson model. Second, the two sampling intakes, which provide the replication, are close to each other (one meter apart at the same depth). This may not provide an adequate representation of the variability in the entire channel cross-section, making replicate samples appear less variable. The key issue in this discussion is that two different kinds of variation are involved: variation in the whole discharge channel vs. the "local" variation in a small section (which could be a stratum) of the channel. The 1986 Indian Point report correctly concludes that this study alone is insufficient evidence to prefer the Poisson model over the negative binomial model, and recommends continuing with the more conservative negative binomial model.

As noted above, this report is a careful and thorough treatment of the issues raised by Casella *et al* (1986). The estimates of entrainment, including confidence limits based on the quadratic variance model, are presented (Tables 4.4-2 to 4.4-4), showing the estimated number killed with and without mitigation. The quadratic variance model is retained, as it should be. There is sufficient evidence to support it, and the quadratic variance model provides a more conservative picture than the more optimistic Poisson model. Although the estimates of  $c^2$  are small, there is still enough variation to warrant continued estimation and replicate sampling.

The number of replicate samples, 688, seems to be adequate, although there were a few

taxa/life stages that had insufficient sample size to estimate the  $c^2$  parameter. The feasibility of increasing the sample size to provide better estimates for these taxa/life stages should be considered. Keeping the sample size fixed, but changing the times at which replicate samples are taken, is *not* feasible: this cannot be done effectively for the many taxa/life stages of interest, especially when (as is the case here) these times must be selected far in advance of the actual sampling. However, the sample of 688 replicate observations did provide very good information on a large number of species.

The other major point of consideration is the proximity of the two sample intakes. As stated in the report, even though the sample size was large, the proximity of the intakes makes it difficult to evaluate the variability in the channel. Previously, the sample size chosen was insufficient to provide a complete picture, although some observations are possible. For example, for those taxa/life stages that had  $\hat{c}^2 \approx 0$ , this is evidence that there was no clumping within strata, and that sorting and identification errors are not much of a problem. Then further studies could be aimed at testing for the presence of stratification and at calculating  $\hat{c}^2$  from more widely separated samples. What is now needed are data of a sufficient number (as in this 1986 Indian Point report) taken at replicate intakes that are far enough apart to provide some assurance that the variability in the channel is being adequately described.

The question of whether  $c^2$  is greater than 0 in the quadratic variance model (1) is an important one, as it can have a great effect on the size of the confidence limits (with those based on the Poisson model being smaller). Thus, from both an economic and an environmental point of view, the decision about the model is crucial. For further discussion of the importance of an adequate description of channel variability, see the treatment of the 1987 Bowline Point sampling program later in this Addendum.

*Indian Point Generating Station Entrainment Abundance Program 1987 Annual Report, pages 71-102*

This report is very similar to the 1986 report, and therefore will not be described in detail. The intakes are in the same position as 1986, one meter apart at the same depth. A few differences will be noted, however.

Unit 3 was not operational during the entire sampling period, so data apply to conditions present when only Unit 2 is in operation.

The analyses described in the 1986 report were done again in this report, along with some additional ones. In particular, t-tests were performed to see if the mean density of ten taxa/life stages of ichthyoplankters differed between the two sample intakes. These tests were done on a logarithmic transformation of the data, and essentially showed no difference in the intake densities. (There was one significant difference, but this can be attributed to chance because of the large number of t-tests performed. This is noted in the report.) These t-tests provide an opportunity to confirm the null hypothesis of equal mean densities at the two intakes, if it is true. If the null hypothesis were rejected, the two intakes clearly would not provide true replicates, that is, pairs of observations of exactly the same random process. In other words, rejection of the null hypothesis tells us that the two intakes are not close enough together to constitute a true replicate measuring process. These t-tests, like the ANOVAs to be discussed shortly, are based on the assumption that all log-transformed observations have equal variance.

A common rationale for log-transforming the data has to do with additivity of effects. That is, if the experimenters are concerned about understanding a treatment effect when underlying counts vary over 2-3 orders of magnitude, the situation may be remedied with a logarithmic transformation. The experimenters often feel that, in cases such as these, the log transform is reasonable, resulting in an analysis that is easier to interpret. As discussed earlier in this Addendum (see the subsection *A Justification for the Logarithmic Transformation of ANOVA Data*), though, concern over additivity alone is not sufficient justification for the logarithmic transformation. The form of the variance function is a crucial component in the decision to transform the data, and information on the form is attainable through the estimation of  $c^2$ . Although the estimation of  $c^2$  has not been a major focus of the entrainment sampling program, many studies (Indian Point, for example) have obtained data suitable for this estimation. Moreover, this estimation can be performed for different years, and the consistency of  $\hat{c}^2$ , and hence  $c^2$ , can be evaluated. Armed with this additional information, a more fully informed decision about logarithmic transformations can be made.

The ANOVA of  $\log_{10}(x+1)$ -transformed data is based on the implicit assumption that the quadratic term in (1) dominates the linear term. This is true when either the density  $\mu$  is

high or  $c^2$  is large, but not when  $\mu$  is low and  $c^2 = 0$ , e.g., not for the Poisson distribution with small mean  $\mu$ . When the logarithmic transformation is applied to counts made during relative peak abundance, with virtually no zero counts present, the large  $\mu$  criterion is satisfied, and this reasoning seems relevant as a potential justification of analyzing log-transformed counts. This method is then likely to be justified on additivity grounds as well, since effects are more likely to result in percentage changes in counts rather than additive changes.

*Indian Point Generating Station Entrainment Survival Study 1988 Annual Report, pages 4-21 to 4-33*

The purposes of the 1988 study at the Indian Point Generating Station were (1) to estimate the survival of ichthyoplankton entrained in the cooling water flow, (2) to determine whether live and dead ichthyoplankton are distributed at random within the discharge canal at sampling station D2, and (3) to assess whether the thermal and mechanical components of entrainment stress appear to be independent. The second objective addresses the issue of potential bias of past survival and abundance estimates, which were based on samples drawn from mid-depth in the discharge channel. If live organisms are able to move vertically in the water column, then fixed location sampling might be biased by the movement of live organisms into or out of the sampled stratum. If directed vertical movement of live organisms were to occur, then samples taken near the surface might have a different density and different proportion of live ichthyoplankton than samples taken near the bottom. Tests of the null hypotheses of equal density and equal fractions alive at surface and bottom, conditions that would occur under random distribution within the channel, were included in the program objectives. Due to the nature of survival sampling, it was not anticipated that sufficient paired surface and bottom samples would be available to examine the magnitude of  $\hat{c}^2$ .

Numbers of live and dead ichthyoplankton for surface and bottom samples were compared by chi-squared analysis of contingency tables. Results of these analyses showed significant differences in survival between surface and bottom samples for two of five species/life stage combinations, post yolk-sac larvae for striped bass and white perch. For striped bass, survival was slightly better from bottom samples than from surface samples,

70.5% vs 66.1%. For white perch, survival was only 6.2% for bottom samples, but was 26.2% for surface samples. The other species/life stage combinations did not show a tendency for live or dead organisms to be concentrated in bottom or surface samples. ANOVA was performed on the densities of these samples after application of the  $\log_{10}(x+1)$  transformation; it indicated significant differences between bottom and surface for striped bass yolk-sac larvae and white perch post yolk-sac larvae.

To obtain further information on stratification, nets were used to sample ichthyoplankton at three depths, surface, middepth, and bottom. The resulting densities were transformed by  $\log_{10}(x+1)$ , in order to compensate for their heterogeneity of variance, and ANOVA was then used to test the transformed densities for differences between means. No difference between mean densities on the basis of depth was found.

The objectives of this study, which were reported at the beginning of this section, were quite different from those of the 1986 and 1987 Indian Point studies of entrainment abundance. A large number of replicate samples were present in the 1986 and 1987 data; that is not the case in 1988, so the information in the 1988 report bears on only a limited number of the issues addressed in this Addendum. One such issue is whether there is stratification of ichthyoplankton in the discharge flow. Because the number of replicate samples in 1988 was not large, the statistical analyses performed in the 1988 report do not include the entrainment estimates and confidence limits based on the quadratic variance model that were present in the reports of the previous two years. The 1988 Indian Point report thus does not utilize the statistical estimation procedures detailed in Casella *et al* (1986).

The major issue addressed in the sections reviewed here is the vertical distribution of ichthyoplankton, i.e., whether stratification is present in the channel, not the estimation of entrainment abundance. The ANOVAs in these sections, like the t-tests in 1987, provide an opportunity to disprove the null hypothesis of equal mean densities at the two intakes, if it is not true. If the null hypothesis is rejected, the two intakes clearly do not provide true replicates, that is, pairs of observations of exactly the same random process. In other words, rejection of the null hypothesis tells us that the two intakes are not close enough together to constitute a true replicate measuring process, and that stratification is present. The one-way and two-way ANOVAs were intended primarily to examine differences in means across

different locations in the discharge, but do not provide any information about the variability in the channel, e.g., the possibility of clumping. ANOVAs were performed for six species/life stage combinations; significant differences in mean density across different locations were found only for striped bass yolk-sac larvae and white perch post yolk-sac larvae. The finding of higher survival of white perch post yolk-sac larvae at the surface raises the possibility of stratification for this taxon. A suggestion is made (p. 4-32) that vertical distribution is dependent on flow in the discharge channel. This is presented as the reason for discrepancies seen in vertical distribution for 1973, when only Unit 1 was operating, and subsequent years (1974-present), when Unit 1 was not operating but Unit 2 and/or Unit 3 was.

Since these ANOVAs were analyses on means, no conclusions could be reached about variances. Moreover, this shows that a conclusion in the report is incorrect, going beyond the scope of the data analysis: it is stated (pg. 4-32) that "These results, together with analyses conducted on data collected since Unit 2 began operation (1974), provide a fairly consistent pattern supporting the contention that fish eggs and larvae tend to be randomly distributed within the discharge channel at Indian Point. The only exceptions to this pattern across the 22 species/life stage evaluations made across the years were for striped bass post yolk-sac larvae in 1975 and for striped bass yolk-sac larvae and white perch post yolk-sac larvae in flume samples from the current study." Although the majority of species and life stages examined showed no evidence of stratification, none of the statistical analyses done in this report can be used to justify a claim of random distribution in the discharge channel, as the conclusions were not based on an appropriate analysis of replicate sampling data.

The report describes the sampling intakes as being at the top and the bottom of the channel. Since these sampling intakes are much further apart than the one meter separation in the 1986 and 1987 studies, valuable information could be gained from these data. If an analysis similar to those done in 1986 and 1987 were performed for those taxon/life stage combinations where a sufficient number of replicates exist, and values of  $c^2$  were estimated, then we could have a clearer picture of the variability in the channel. Furthermore, we could assess the validity of the confidence limits based on the quadratic variance model, which reflects extra-Poisson variation, and the Poisson model.

Using the 1988 data from the separated intakes,  $\hat{c}^2$  could be calculated for various taxa/life stages. If  $\hat{c}^2$  turned out to be much larger than 0, we would be led to reject the null

hypothesis  $H_0: c^2 = 0$ . This hypothesis is satisfied by the Poisson distribution, which also supposes no clumping and no stratification. The departure from the null hypothesis could be due to either clumping or stratification, or a combination of the two; however, with only two replicates, there is no way to tell from the data which of these is the underlying situation. (Details of this test are given in Casella *et al* (1986), Appendix VI.) A significant t-test would indicate that stratification is present, but would not indicate what portion of the departure from the null hypothesis is due to this stratification; the rest of the departure would be due to the presence of some degree of clumping.

*Roseton and Danskammer Point Generating Stations Entrainment Abundance Studies 1986 Annual Report, pages 5-77 to 5-103, 4-1 to 4-8*

The specific objective of the 1986 entrainment abundance program at the Roseton Generating Station was to evaluate the effectiveness of outages in reducing entrainment at this station. Continuous sampling was done on 62 predetermined dates from May through July, with samples retained as one-hour collections. Replicate samples were collected during two randomly determined days per week, resulting in 48 replicated one-hour sampling periods per week, throughout the 13-week sampling period.

Primary sampling at Roseton was at a fixed location midpoint in the discharge pipe, 1.8 meters from the bottom. (See Figure 1.4 in Chapter 1.) This has been the sampling location for collections since 1980. Replicate samples were collected from a point next to the seal well wall, approximately 2.8 meters from the bottom. The replicate sampler was approximately 4.3 m in front of the primary sampler. Sampling was done with a pump-net-barrel system. Sample volumes were standardized and replicate sampling was performed, with flow and volume used as criteria for acceptance or rejection of the samples for the 24-hour collection period. (For further details on sampling procedures at Roseton, see Sections 1.2.3 and 1.3 in Chapter 1.)

Sample volumes for primary and replicate collections were very similar, as were the percent composition of key taxa/life stages. Comparison of hourly samples for the two collection sites throughout the season were presented by a variety of graphs, charts, and tables; this information showed similar patterns with little difference, in general, between

primary and replicate samples. The report states that "[data] for certain taxa and life stages, suggest a small but consistent difference..." Differences in ichthyoplankton density between primary and replicate samples were treated statistically by a t-test using the difference of the natural logarithms of density as the response variable. The null hypothesis implicit in this procedure is  $H_0: \ln D = 0$ , where  $D$  is defined by

$$D = \frac{\text{primary sample organism density} + 1}{\text{replicate sample organism density} + 1} \quad (12)$$

Using this procedure, 4 out of 20 possible sets of density differences were found to be statistically significant at the  $\alpha = 0.05$  level.

The location of the replicate sampling intake against the seal well wall would seem to be a "worst case" location. The report acknowledges this possibility, noting that the selected sampling locations "provided the most severe test of homogeneous mixing..." In spite of this, the resulting counts and density estimates of ichthyoplankton were very similar to those collected at the primary sampling site. The observation of similar sample statistics, however, does not necessarily imply equality of the distribution of organisms in the water from which the samples were drawn. The earlier subsection in this Addendum, entitled *Sampling Gear and Abundance Estimation*, is relevant to this situation.

The transformation of the data described by equation (12) above makes the validity of the assumptions underlying the t-test questionable. Little is known about the error structure of the data, but a multiplicative error structure is implicitly assumed by the model used in the report. The experimenters examined the transformed data and felt that the multiplicative error structure is justified, but we feel that statistical treatment of the data might be supplemented by the use of distribution-free (nonparametric) test statistics, rather than by attempting to normalize the data through transformation.

The paired samples from this study could be used to estimate the parameter  $c^2$  as proposed in Casella *et al* (1986). Although this was not included in the scope of work for the Roseton 1986 Annual Report, it would be straightforward to perform this analysis from the data compiled for that report.

*Bowline Point Generating Station Entrainment Abundance and Unit Outage Evaluation 1984 Annual Report, Appendix F*

This appendix is a discussion of sampling variation, emphasizing the Poisson model and the nature of alternative models of variability. Variation in the number of organisms recorded as present in a sample results from variation in the number of organisms actually present per cubic meter of water, the main quantity of interest. It also results from variation in many incidental factors, including the sampling duration, the pumping rate of the sampler, the fraction of organisms detected during sorting, and the fraction of organisms correctly identified. Each of these (and other) incidental factors is a random variable with its own distribution; perhaps several of these variables are dependent, requiring the use of a joint distribution.

The situation in which the number of organisms present per cubic meter is Poisson distributed is considered. The Appendix contains the claim that the recorded catch,  $C$ , then has a Poisson distribution if and only if the fractions of organisms detected during sorting ( $f_3$ ) and correctly identified ( $f_4$ ) and two other similar fractions "are all constant and equal to unity." Actually, the distribution of  $C$  can be shown to be Poisson under the less restrictive condition that the number of organisms present per cubic meter is Poisson distributed and all of the fractions  $f_1$  to  $f_4$  are constants between 0 and 1. This result follows from the distributional theorem:

*Theorem:* If the conditional distribution of  $Y$  given  $X=x$  is binomial( $x, p$ ), and  $X$  has a Poisson( $\lambda$ ) distribution, then the (marginal) distribution of  $Y$  is Poisson( $\lambda p$ ). Stated equivalently, but less formally, if  $Y|X \sim \text{binomial}(X, p)$  and  $X \sim \text{Poisson}(\lambda)$ , then  $Y \sim \text{Poisson}(\lambda p)$ .

Applying this result repeatedly to the equation  $C \sim B(B(B(B(P(t \cdot q \cdot \bar{d}), f_1), f_2), f_3), f_4)$  in the Appendix gives the distribution of  $C$  as Poisson( $t \cdot q \cdot \bar{d} \cdot f_1 \cdot f_2 \cdot f_3 \cdot f_4$ ).

In practice, the fractions  $f_1$  to  $f_4$  will be random variables, and  $f_4$  may, through misclassification, be greater than 1, making the distribution of the recorded organism count more complex than the Poisson. (Section 4.5 of this report discusses the connection between misclassification errors and the quadratic variance model.) Because of these extra sources of

variability, the variation in observed counts exceeds the variation in the number of organisms entering the sampler.

Replicate sampling data from 1983 and 1984 were analyzed to test the null hypothesis of a Poisson distribution. The test was based on the variance:mean ratios of replicate pairs; under the null hypothesis, the variance and the mean should be equal. Data from 1984 did not depart as much as 1983 data from the null hypothesis, perhaps because of additional controls imposed on the sampling process in 1984. The test used is the test recommended in Casella *et al* (1986, equation (4.3)).

This analysis highlights the fact that the variability in observed organism counts equals the variability in the number of organisms entering the sampler plus the additional variability due to experimental error, composed of factors like the ones mentioned above. It is therefore important to make the variation due to these additional sources of error as close to 0 as possible, in order to avoid or limit the bias toward rejecting the null hypothesis of the Poisson distribution. This important point is not addressed further in this Bowline Point report. However, it deserves more attention.

*Bowline Point Generating Station Entrainment Abundance and Unit Outage Evaluation 1986 Annual Report, pages 3-9 to 3-11, 4-16 to 4-22*

The results of the 1986 entrainment sampling at Bowline Point Generating Station are presented in terms of estimated daily and season total numbers of key taxa/life stages, with 90% confidence limits, and estimated percent reductions attributable to outages (without confidence limits). The outages reduced cooling water flow by 34%, while the estimated percent reduction in numbers ranged from 4.5% to 48.9% for the key taxa/life stages as a result of the timing of the outages relative to the development of life stages.

This report utilized the estimation approach developed in Casella *et al* (1986), involving the calculation of  $\hat{c}^2$  and its use to estimate the variances of the entrainment estimates and to obtain confidence limits. Details appear on pages 3-9 to 3-11 and in Appendix B-1.

*Bowline Point Generating Station Entrainment Abundance and Unit Outage Evaluation 1987 Annual Report, pages 4-11 to 4-28*

The 1987 Bowline Point report describes two major categories of bias affecting entrainment estimates. The first is differences between the true organism density in a sampled unit of water and the expected value of the organism density observed in a sample drawn from that unit, where each unit corresponds to a given time interval and a given portion of the discharge channel cross-section. Some components of this category involve nonrandom sampling error, e.g., gear avoidance. Others involve nonsampling error, which is defined as error not related to the selection of a sample, that would be present even in a census, e.g., organism misidentification. The second category of bias is location effects, a type of nonrandom sampling error, resulting from systematic patterns in organism density within the discharge channel. The report cites a recent critique (Versar, 1987) that concluded the distribution of organisms within the Bowline Point discharge pipe could and should be evaluated, using simultaneous sampling at multiple points within the discharge pipe. The 1987 studies at Bowline Point investigated the existence of location effects.

The importance of an adequate description of channel variability is highlighted by the 1987 Bowline Point program, which included a study comparing four different sampling stations or locations. Simultaneous replicate sampling was conducted in an extensive study of distribution patterns in the Unit 2 discharge pipe during June 2 through August 7, 1987. Four locations within a cross-section of the discharge pipe were simultaneously sampled in eight 3-hour collection periods per day, using two automated systems (AUTOSAM) and two manually operated systems (MANOSAM). (Differences in device type were not expected to influence the results of entrainment sampling.) These collection systems were switched among the different sampling locations on a daily basis in a four-day-per-week cycle. Three of the stations were near mid-depth of the discharge pipe, located in center, north, and south positions, while the fourth was located at the top of the discharge pipe at the entrance to the standpipe. (See Figure 1.2 in Chapter 1. For further details on sampling procedures at Bowline Point, see Sections 1.2.1 and 1.3 in Chapter 1.)

Entrainment abundance of only striped bass and bay anchovy post yolk-sac larvae were considered adequate to test for station and gear effects. Data from only their respective

periods of high abundance (four weeks in June for striped bass and six weeks in late June to early August for bay anchovy) were included in the analysis, in order to avoid the effect of a large number of zero catches. Log transformed data, coded by the eight three-hour daily collection periods and by week as well as by location and gear, were analyzed by SAS PROC GLM using a linear model including all four main effects and their first-order and second-order interactions, except for week  $\times$  location  $\times$  gear. Sample counts were first converted to densities (per 1000 m<sup>3</sup>) and then incremented by 1 before the log<sub>10</sub> transformation was performed to "minimize the effects of variance heterogeneity". This comment was the only reference in the report concerning a relationship between the variance and the mean. Implications of the transformation with respect to multiplicativity of factor effects on density were not discussed, and no test of the ANOVA's implied assumptions was reported.

The ANOVA can provide information about the pattern of stratification in the channel, but not about whether clumping is present. The considerations cited above in the subsection on the logarithmic transformation of ANOVA data were not discussed; however, they are quite relevant. The ANOVA of the log-transformed counts is based on the implicit assumption that the quadratic term in (1) dominates the linear term. This is true when either the density is high or  $c^2$  is large, but not when the density is low and  $c^2 = 0$ , e.g., not for the Poisson distribution with small mean. Restriction of the periods used for the analysis to those of high abundance increases the probability that this assumption is satisfied.

The untransformed data are potentially useful for exploring the relationship of variance to mean. The two MANOSAM devices, in particular, provide numerous paired counts over a range of densities for both species.

Interpretation of the analysis of the log-transformed data was complicated by the presence of a significant location  $\times$  gear interaction for both species. The possibility was mentioned that confounding due to some missing combinations in the four-factor factorial design might produce anomalous results; however, only 6 to 7% of more than 500 cells were missing in each case, so this is not a very promising source of anomalies. Three-dimensional graphical displays of the 4  $\times$  4 location  $\times$  gear means failed to reveal any consistent pattern for the two species; the significant location  $\times$  gear interaction does not appear to be safely interpretable. A comparison of bay anchovy length frequency distributions indicated a deficiency of smaller post yolk-sac larvae in AUTOSAM 1, at the top of the discharge pipe

location. There was an indication that excluding the data from this location and this sampling system would eliminate the difficulties, resulting in consistent homogeneity (i.e., random distribution) at mid-depth for both species.

#### *Two Related Articles*

The following two articles contain information that bears upon the question of the randomness of spatial distribution in the discharge channel. Consequently, we were asked to review them. Although a comprehensive search for material on this subject was not undertaken, the literature that was examined did not reveal any other work on this topic.

*Young, J.R. 1988. "Testing for Unbiased Sampling of Ichthyoplankton in a Power Plant Discharge Channel". Unpublished Manuscript.*

This study considered the issues of bias in entrainment sampling and the randomness of dispersion of ichthyoplankton in the discharge flow. Empirical data from the Indian Point 1986 sampling program and a computer simulation of the entrainment sampling process were used to examine the possibility of bias in tests of randomness (i.e., no stratification and no clumping of organisms) due to processing errors. Observing that "the tedious and repetitive nature of sample sorting make[s] it a likely source of additional variation in sample counts," the paper notes that a sample judged to be acceptable, or not defective, can contain a substantial amount of detection and classification error. A sample is judged to be defective if, based on a second independent examination of its contents, either (i) less than 90% of the organisms were found or (ii) any taxon count had an error rate of 10% or more; thus, a nondefective sample could have observed "counts" that are rather different from its true, unobserved organism counts.

Indian Point replicate data from 1986 on each abundant taxon/life stage were used to test the hypotheses: underlying mean densities are equal across locations in the discharge cross-section, and replicate counts are consistent with a Poisson distribution. A paired t-test was performed on the differences between log-transformed counts,  $\log_e(\text{count} + 1)$ , at the two sampling locations. These locations were within 1 meter of each other, as the sampling

program's original intention had been to measure sampling variation at a single location. Analysis of the data supported the view that there was no difference in mean density between the locations sampled.

In addition, Fisher's  $D^2$  was computed for each sample pair, given by  $D^2 = (n-1)s^2/\bar{x}$ , where  $\bar{x}$  and  $s^2$  are the mean and variance of the replicate pair and  $n=2$ . Under the null hypothesis that each pair consisted of two independent Poisson distributed observations with a common mean, the  $D^2$  for each pair has a chi-squared distribution with 1 degree of freedom. Using the average value of  $D^2$  over all nonzero replicates as a test statistic, the Poisson null hypothesis was decisively rejected. (This test statistic is equivalent to the statistic  $S_p$  defined in Section 4.1 of this report.) It is then demonstrated that the samples' true organism counts could be Poisson distributed, while errors arising during sorting and classification could explain the departure of the observed data from the null hypothesis. This is exactly the point advanced previously in Section 4.5 of this report.

The computer simulation and an analytical description of the sampling process examined the effect of sample processing errors on the expected value of  $D^2$ . Pairs of iid (independent, identically distributed) Poisson sample counts  $X$  were generated. Each count  $X$  was reduced to a smaller detected count  $Y$ , reflecting sorting error, the failure to detect all organisms in the sample;  $Y$  followed a binomial distribution with parameters  $X$  and  $R$ , a random detection probability having a beta distribution. Each detected count  $Y$  was then transformed further to an identified count  $Z$ , reflecting counting and identification error;  $Z$  followed a binomial distribution with parameters  $Y/q$  and  $q$ . The identified count  $Z$  could be either higher or lower than  $Y$ , but the expected counting and identification error was 0. The simulation and analytical results showed that sorting and identification errors can seriously bias  $D^2$ . This bias becomes greater as the mean density of organisms increases.

This article supports the position proposed in Section 4.5 of this report, that the true (but unknown) organism counts in the samples could follow a Poisson distribution, while errors arising during sorting and classification could result in observed sample counts that do not follow a Poisson distribution. From a statistical viewpoint, the analysis of the observed data must follow the quadratic variance model of equation (1), which is detailed in Chapters 4 and 5 of this report; regardless of whether the true (unobserved) organism counts are Poisson distributed, the observed data exhibit extra-Poisson variability and must be analyzed

accordingly.

*Jude, Mansfield, DeBoe, and Tesar. 1986. "Spatial Distribution of Entrained Fish Larvae in a Power Plant Discharge Canal". Can.J.Fish.Aquat.Sci. 43:1070-1074.*

An attempt to address the randomness and representativeness of larval fish entrainment sampling was undertaken in a brief study at the Monroe Power Plant on western Lake Erie. The expressed motives and objectives were the same as those of the Hudson River studies, to test for horizontal and vertical stratification of fish larvae in the discharge canal and to test the Poisson assumption in order to validate methodology for estimating entrainment loss. Simultaneous samples were taken at three equally spaced locations across the discharge canal using three 0.5-m-diameter plankton nets with attached flowmeters. Each station was alternately sampled for 10-minute periods at three depths of 1, 3, or 5 m in the 6 m deep canal, ordered among the stations in a 3x3 Latin square design. Sampling was conducted over a two-hour period around midnight on 7-8 June, 1982, when maximum catch was expected, and again on 12-13 July, when juvenile fish would be present in larger numbers. On each date, the Latin square was repeated four times during the two-hour period to provide replication in time.

Access to these stations was provided by a bridge spanning a bend in the discharge canal just before it widens into a channel into Lake Erie. The bend, unfortunately, created a twofold difference in water velocity across the canal, resulting in slow current near the inside wall, potentially below detection of the net's flowmeter at this station. Estimated density (catch/measured volume) was twice as large at this station as at the middle and outer stations, and the authors speculated that the propellor of this flowmeter was often not turning in the slow current so the sampled volume was grossly underestimated. Among all 72 samples, the estimated volumes filtered per sample ranged from 2.4 to 80 cubic meters, even though all 12 circulating water pumps were pumping continuously at the same rate throughout the two sampling periods. Subsequent measurements of water velocity at these stations and depths revealed large differences also between the middle and outer stations, as well as station x depth interaction in velocity. Reliability of the estimated volume filtered is thus a critical and questionable issue in the interpretation of the results of this sampling study.

The Latin square design carries with it implicit assumptions concerning lack of interactions among the three controlled factors (location, depth, and time) and hence does not lend itself to testing the Poisson model. Such a test, though attempted here (in an unspecified manner), is uninterpretable due to these other potential causes of model misspecification. (The Poisson goodness-of-fit test was said to be performed on density data; if so, this would be analogous to performing a chi-squared test on percentage data rather than on count data.) Data analysis proceeded on the basis of the Poisson model, employing ANOVA on the inappropriately square-root-transformed density data (rather than count data); with grossly varying sample volumes, the only appropriate Poisson-based analysis would employ likelihood ratio testing methods like those provided by the statistical package GLIM. In view of the defects in design, measurement, and data analysis, the reported results of this study are to be considered of dubious value. The merit of the published report lies in pointing out some of these defects so that others may avoid them in their sampling studies.

## CONCLUSIONS

This concluding section has two distinct parts. The first is a summary of what has been observed from these recent entrainment abundance sampling studies. The second is a discussion of what further knowledge is needed for a fuller understanding of the relationship between observed ichthyoplankton counts and entrainment abundance estimates.

### *Observations*

The quadratic variance formula (1) provides a better description of empirical entrainment data than the Poisson model. As noted earlier in this Addendum,  $c^2$  reflects the quadratic term in equation (1); it provides a measure of the extra variation present in the entrainment abundance data above the level consistent with the Poisson model. This extra variation is potentially due to some combination of stratification, clumping, and inherent sampling process error (such as extrusion, mutilation, non-detection, misidentification, and counting error). However, there are other possibilities: for instance, the entrainment sampling may have the structure of a gamma mixture over time of Poisson random observations, which also leads to the extra-Poisson variation of equation (1).

An important point concerns the relationship between the parameter  $c^2$ , its estimator  $\hat{c}^2$ , equation (1), and the negative binomial distribution. The estimator  $\hat{c}^2$  is a method of moments estimator of  $c^2$  in the quadratic variance formula (1). It is therefore not tied to the negative binomial distribution, but only to the quadratic variance formula (1), which is satisfied by the negative binomial but also by other distributions. The negative binomial distributional model serves as a convenient, flexible, and mathematically tractable device for simulating distributions having the quadratic variance function of (1). It also has intuitive appeal as a gamma mixture of Poisson distributions (see Appendix II). Our attention to the negative binomial model appears to have led to an emphasis on this distribution, rather than on the more general equation (1), in the recent generating station entrainment reports described above. The estimator  $\hat{c}^2$  may be tied to some restricted class of Poisson mixture models, in which  $c^2$  is the squared coefficient of variation of the mixing distribution of the Poisson parameter  $\lambda$ :

$$\begin{aligned}\text{Var}(X) &= E\{\text{Var}(X|\lambda)\} + \text{Var}\{E(X|\lambda)\} = E(\lambda) + \text{Var}(\lambda) = \mu_\lambda + \sigma_\lambda^2 \\ &= \mu_\lambda + (\sigma_\lambda^2/\mu_\lambda^2)\mu_\lambda^2 = \mu_X + (\sigma_\lambda^2/\mu_\lambda^2)\mu_X^2\end{aligned}\quad (13)$$

where the conditional distribution of  $X$  given  $\lambda$  is  $\text{Poisson}(\lambda)$ , and  $\lambda$  is a random variable with mean  $\mu_\lambda$  and variance  $\sigma_\lambda^2$ , so  $E(X|\lambda) = \text{Var}(X|\lambda) = \lambda$  and  $\mu_X = E(X) = E(E(X|\lambda)) = \mu_\lambda$ . Therefore equation (1) holds, with  $c^2 = \sigma_\lambda^2/\mu_\lambda^2$ .

For example, in the case of a lognormal mixture of Poissons,

$$c^2 = -1 + \exp\{\text{Var}(\ln \lambda)\} \quad (14)$$

From recent replicate sampling, we have gained some knowledge of the range of values assumed by the parameter  $c^2$ , based on the realized values of the estimator  $\hat{c}^2$ . Useful information has come from the Indian Point 1986 and 1987 sampling programs, during which simultaneous samples were taken at intakes one meter apart at the same depth, and potentially from the Bowline Point 1987 sampling program, during which pairs of MANOSAM samples were taken simultaneously at two mid-depth stations. From the data collected during these sampling programs, we have at least a preliminary idea what range of values  $c^2$  can assume. This is likely to change as further replicate sampling reveals more about which values are possible, and which values are likely, for  $c^2$ .

For taxa/life stages with small  $c^2$ , stratification and clumping within strata are not apparent. For taxa/life stages with larger  $c^2$ , more work is necessary to develop methods for determining the relative contributions of stratification, clumping, inherent sampling process error, and other features of the entrainment process. Experiments can be designed to estimate these different phenomena, any or all of which could be responsible for making  $c^2$  greater than zero. Note that inherent sampling process error leads to bias in testing the validity of the Poisson model via the null hypothesis that  $c^2$  equals 0.

We have gained some experience with the estimator  $\hat{c}^2$ , which has been calculated using empirical data as mentioned earlier, in sections of this Addendum discussing specific plant sampling programs. We also know a good deal about the behavior of  $\hat{c}^2$  under a wide range of (hypothetical) conditions from the extensive computer simulations in Chapter 6 of this report. Our knowledge about  $\hat{c}^2$  translates immediately into corresponding knowledge about the entrainment abundance confidence intervals based on  $\hat{c}^2$ , which provide more realistic entrainment estimates than the intervals based on the overly optimistic model of Poisson variation in the discharge channel.

We have some indications of the role of stratification in the distribution of ichthyoplankton in the discharge channel. The 1987 Bowline Point report concluded that its data analyses "provide no evidence of a non-random distribution of entrained organisms in the areas of the discharge pipe normally used for entrainment abundance" (p. 4-26). The only anomalous sampling location, the top of the discharge pipe at the entrance to the standpipe, produced significantly higher results than the other locations for striped bass post yolk-sac larvae but significantly lower results for bay anchovy post yolk-sac larvae. This location is not normally used in entrainment abundance studies. (Unexpectedly, there was clear evidence of a sampling gear effect, due largely, perhaps primarily, to AUTOSAM 1.) The Indian Point 1988 report described an analysis of seven species/life stage combinations sampled at three depth stations, surface, mid-depth, and bottom. There were no significant differences in density among the depth stations for any of the seven combinations. Based on the results of 19 out of 22 species/life stage evaluations over the years, the report concluded that there is a fairly consistent pattern supporting the contention that fish eggs and larvae in the discharge channel at Indian Point show a fairly consistent pattern of no stratification (p. 4-32). (This conclusion was worded in terms of fish eggs and larvae being randomly distributed; for a critique of this, see the subsection here on the Indian Point 1988 report.) In

the remaining 3 evaluations, surface densities exceeded bottom densities.

The study of sample splitting described in Appendix G of the 1985 Indian Point report is a promising initial effort on assessing the reliability of splitting. If the difficulties in achieving thorough uniform mixing of the sample before splitting can be resolved, the potential gains from sample splitting procedures are great, as suggested in Chapter 7 of the present report.

In summary, the statistical issues involved in the modeling process necessary for accurate estimation of entrainment abundance are much more complex and difficult to address through data collection and analysis than had originally been thought. Hypotheses about the distribution of organisms within the discharge channel are statements about the organisms at the point where they enter the sampling device. However, to test these hypotheses, we observe those organisms that have entered the device, passed through the sampling pump (if applicable), and been retained in the net, detected in the sample, and identified (correctly or not) as being a particular taxon/life stage. The estimated  $c^2$  values reflect not only the variation in the distribution of organisms, but also the variability added by the remainder of the sampling process.

### *Recommendations*

We now consider what remains to be learned about the number of ichthyoplankton in the discharge channel and the estimation of this quantity. The main issues to be treated are: (i) our understanding of the variability in the discharge channel and the roles of its potential sources, clumping and stratification; (ii) our understanding of the variability in the observed entrainment count data and the roles of its potential sources, which include inherent sampling process error as well as the variability of the true (unknown) organism counts; (iii) our careful consideration of flow velocity; (iv) our understanding of the behavior and properties of the estimator  $\hat{c}^2$ , especially the effects of clumping, stratification, and inherent sampling process error on  $\hat{c}^2$ ; (v) our knowledge of  $c^2$  for various taxa/life stages; and (vi) the prospects for sampling designs that address key questions connected with the pattern of entrainment in the discharge and the corresponding estimation techniques.

(i) The first major need is to gain a better picture of the variability in entrainment abundance across the discharge channel. The two components of the "state of nature" in the discharge are the presence or absence of stratification in the discharge flow, and the presence or absence of clustering of organisms (within each stratum if stratification is present). The pattern of variability reflects both of these; if neither stratification nor clumping is present, the distribution of organism counts will be Poisson.

Stratification represents "large scale" variation across the channel, in which moving from one sector of the channel cross-section to another (e.g., moving 2 meters up and 1 meter to the left) results in a different density of organisms, while replicate samples within a single sector (e.g., starting at the channel's geometric midpoint and moving 0.3 meters up and 0.2 meters to the right) constitute independent Poisson observations with a common density of organisms. Clustering of organisms is the opposite, representing "small scale" or "local" variation in the channel, in which even very close replicate samples in the channel display extra-Poisson variation; clustering does not involve any "large scale" variation or systematic changes across the channel. In many sampling studies, the "replicate" intakes have been located relatively far apart in the discharge channel. If extra-Poisson variability is observed under these conditions, it could be due to either stratification or clumping.

If stratification is present, it is likely to lead to bias in entrainment estimates. Unfortunately, the physical constraints on the sampling locations make it impossible to eliminate the bias by randomizing the location of the sampling station during the entrainment season. It therefore becomes critical to determine whether stratification occurs in the discharge channel, and, if so, in what pattern and degree and under what conditions of flow, organism density, etc. Different taxa/life stages must be investigated separately, although the possibility of combining data for similar taxa should receive attention. This process must be undertaken separately for each generating station; it cannot be safely assumed that they will display similar patterns. If stratification is present in the discharge flow, the ability to adjust suitably for its effects is very important in avoiding biased entrainment estimates. More must be learned about the patterns of stratification present in the generating station discharge channels, in order to develop adjustment techniques.

If clumping or clustering is present in the distribution of organisms, bias will not be introduced, but the variance of estimation (including confidence limits) will be greater than

under random Poisson distribution of organisms. Replicate sampling provides information about whether nonrandomness or clumping is present, and, if so, to what degree. Thus, if enough nonzero replicate samples are available for a given taxon/life stage, an accurate confidence interval for entrainment can be found. Problems in estimation arise only when replicate sampling data are insufficient. Testing for the presence of clumping requires eliminating the effects of possible stratification; this can be done by taking replicate samples very close together, to ensure that no stratification-based differences affect the observed organism counts. Testing for the presence of clumping is discussed in Section 4.5 of the present report, in the article by Young, and in the Bowline Point 1984 Appendix F.

It is vitally important to be able to separate the phenomena of stratification and clumping. To test the null hypothesis of a Poisson distribution of organisms (no stratification and no clumping) against the alternative that there is some departure from the Poisson (either some stratification or some clumping or both), data consisting of sets of two simultaneous observations are sufficient. To test for stratification and clumping separately, however, sets of three (or more) simultaneous observations would be required. The additional observation is needed because of the need to measure both the variation over the whole channel, i.e., stratification, and the variation at the "local" scale, i.e., clumping. To accomplish both objectives at once, a sampling pattern combining the approaches taken at Indian Point in 1987 (close intakes) and 1988 (separated intakes) would be effective. With three simultaneous intakes, two should be close enough together to be true replicates and the third farther away. It is important to know how close together two intakes must be to produce true replicates; at Indian Point, based on the 1987 replicate study in which  $\hat{c}^2 \approx 0$  for most taxon/life stage combinations (see p. 89, Table 4.3-2), it appears that a 1m distance may well be close enough to provide true replicates at a single location. For Bowline Point, it is possible that data gathered in the 1987 studies may be similarly informative, but further analysis of these data is needed. The two close intakes would allow the estimation of the degree of clumping, while the intake farther away would lead to an estimate of the degree of stratification. The layout and analysis for this sampling experiment, which should be implemented at each of the power stations, remain to be developed.

A set of simultaneous observations drawn from the discharge channel under identical conditions will appear to be true replicates only if the sampling gears at all sampling locations perform equally well. In practice, equality of sampling gears has been difficult to achieve.

Methods to evaluate this equality of sampling gears are also needed. Such methods should be able to determine whether two or more apparently identical gears are actually performing equally well.

Exploratory analysis of the relationship between variance and mean should be undertaken on existing data sets. For example, the mean square for sampling station within days at fixed segments of the diel period, which is the estimated error variance, may be plotted against the corresponding mean densities to look for the anticipated quadratic relationship (1). Since density levels change over the season, such interactions should be partitioned into short periods of relatively stable density. The counts should not be transformed in such explorations, since (1) refers to the original counts (for constant sample volume).

(ii) In addition to the variability in the true number of organisms in the discharge channel, we must investigate the variability in the observed entrainment data. The true number of organisms is known rarely, if ever; the observed organism counts reflect both the true (unknown) counts and the level of inherent sampling process error. This error includes extrusion, mutilation, non-detection, misidentification, and counting error. The overall rate of such problems may be small enough to be negligible, or large enough to be quite serious, or somewhere between these extremes. Knowing the consequences of different levels of the various kinds of sampling process error is necessary in order to judge the seriousness of their effects on  $\hat{c}^2$  and the entrainment estimates.

(iii) Another need is a greater understanding of the role of the velocity profile in the discharge channel under various flow conditions. All work on estimation of entrainment abundance at generating stations, in this report and elsewhere, where pumping is used to collect samples, is predicated on the assumption of a constant zone of withdrawal for the sampling pipe. If the validity of this assumption can be documented, entrainment abundance estimation becomes a more manageable task. Conversely, failure to satisfy this assumption would greatly complicate further analysis. None of the work under review has addressed this area of concern. A study to evaluate velocities in the sampler orifice and adjacent water under various flow conditions should be undertaken.

(iv) Much remains to be learned about estimating the quadratic variance parameter  $c^2$

and using the estimate in the estimation of entrainment abundance. The behavior and properties of the estimator  $\hat{c}^2$  should be investigated further. In particular, the effects of clumping, stratification, and inherent sampling process error on the behavior of  $\hat{c}^2$  and on the estimation of entrainment abundance are important to determine.

A related issue worth investigating is our understanding of the estimation of  $c^2$  and the possible development of improvements in the estimation process. The sampling behavior of the estimator  $\hat{c}^2$  should be explored more fully, to determine whether current procedures should be revised; for example, perhaps the requirement of 40 nonzero replicate samples should be made less stringent. It was never claimed that  $\hat{c}^2$  is the best possible estimator of  $c^2$ , and attempts to find a better estimator of  $c^2$  through different approaches should be considered.

(v) More detailed knowledge of the values assumed by  $c^2$  for the taxa/life stages of interest would be useful in applying the model of extra-Poisson variation to the task of obtaining confidence intervals for entrainment abundance. It would be especially helpful to know whether information about  $c^2$  can be pooled across taxa and across years. Pooling across generating stations might also be possible, and pooling across different life stages is a remote possibility. It would be very advantageous to establish that information from different taxa and years could be used to estimate a common value of  $c^2$ . If it turns out that one or more of these types of pooling is valid, this would decrease the amount of future sampling effort needed to obtain confidence intervals of a given degree of accuracy.

The variability in observed organism counts has two components, the variability in the number of organisms entering the sampler and the additional variability due to experimental error (also referred to above as inherent sampling process error). The latter was discussed in the Addendum sections on Appendix F of the 1984 Bowline Point report and on Young (1988). Its connection to the quadratic variance model was detailed in Section 4.5 of this report. More complete knowledge of the effect of the variability due to experimental error is necessary in order to judge whether current quality control practices for entrainment data are adequate. The cost of more stringent quality control should be compared to the savings realized from the more precise estimation possible with more accurate entrainment data.

(vi) A final recommendation is for a renewed interest in choosing entrainment

abundance sampling designs that make it possible to answer the critical questions about the variation of organism counts in the discharge flow and about the entrainment abundance estimation process. As usual, the term "sampling design" is used here to include the field collection, laboratory analysis, and statistical analysis of entrainment sampling data.

The reports from the last few years reveal great year-to-year and plant-to-plant differences in the sampling plans adopted at the generating stations and in the subsequent statistical analyses of the resulting entrainment data. Some of these sampling designs have been extremely informative about variation in the discharge channel, while others have contained little or no information bearing on this issue. Moreover, the estimation of entrainment abundance, a major goal of this entire effort, has received variable emphasis through the years. In some years it is a primary aim, while in other years the emphasis has been on addressing other issues, with very little being learned about channel variation and abundance estimation. From the perspective of these issues, it often seems that the three plants operate separately, with little coordination of sampling designs.

The annual reports from the generating stations must address a variety of entrainment-related issues, of which the topic of this Addendum is only one among many. Although there has been an effort to perform effective and informative entrainment abundance sampling over the years, this effort has not been uniform throughout plants or years. It would have benefited greatly from the presence of two features, both of which should be incorporated without delay:

— More thorough data exploration should be included to supplement the standard analyses. Innovative data analyses suggested directly by the entrainment process and by the data themselves can be extremely informative. The quadratic variance model of equation (1) and the  $\hat{c}^2$  approach to entrainment abundance estimation are one such analysis; other analyses in the same spirit would improve our knowledge further. This is especially important because the standard analyses often depend on assumptions that are known not to be met, which makes them only approximately valid.

— Integration of the work done in previous years at the three plants is vital. The changes in the three sampling programs over the years must reflect the past experience and findings of the programs, but there is no explicit evidence of this. The links between past and present sampling studies are not reported. This leaves the impression that the plants' annual reports do not use and build further upon the good work done in previous years. To counter this

impression, there is an immediate need for an overall review of the sampling programs at the three plants. It should explain exactly what was done at each plant in each year and (to the degree possible) why this choice was made, with special emphasis on the influence of results from studies done in previous years. Of course, as future annual reports are issued, they should include this information, which allows the reader to place each year's work properly in the perspective of the overall sampling program.

We find that the entire body of work – the reports for all plants and all years – leaves several basic issues largely or wholly unresolved. There have been approximately ten years for collection of data, and the total knowledge concerning variability in the discharge channel that has been accumulated over this time is small compared to what could have been learned with a more comprehensive approach. (For example, with a more coordinated sampling effort in the discharge channel, we would know, by now, how to model the variability.) An overall statistical plan, encompassing sampling and analysis, building a body of knowledge through the next few years, and coordinating the work at all three plants, is essential.

#### *Summary of Recommendations*

To highlight the points we consider important to concentrate on in the future, the following list reviews our recommendations. All points are described in detail in the above conclusions. Although we are not familiar with the entire range of questions relating to the impact of power plants on Hudson River ichthyoplankton, the following needs are most important based on what we have seen:

- Better description of discharge channel variability, including quantification of
  - stratification
  - clumping
  - replicability of sampling gear
  - comparisons between different sample collection methods, e.g., net vs pump
- Better description of observed count data variability, including
  - discharge channel variability factors, especially stratification and clumping
  - measurement process errors (extrusion, mutilation, misidentification)

- Separation of the effects of stratification and clumping
  - their effects lead to different conclusions
  - also tied to determination of placement of sampling gear
- Further exploration of the relationship between mean and variance
  - data analytic explorations needed
- Better description of velocity profile
- Better knowledge of estimator  $\hat{c}^2$ 
  - behavior and properties of  $\hat{c}^2$
  - effects of stratification, clumping, measurement process errors on  $\hat{c}^2$
  - possible improvements over  $\hat{c}^2$  in estimation of  $c^2$
- Better knowledge of  $c^2$  values for taxa/life stages of interest
  - examine consistency across species
  - examine consistency across years
- More exploration of effect of components of variance
  - effect of variability of number of organisms in sampler
  - effect of various components of experimental or measurement error
- Execution of sampling designs providing answers to key questions involving
  - variation of organism counts in discharge flow
  - entrainment abundance estimation process

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