Applications in adaptive cluster sampling of Gulf of Alaska rockfish

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Abstract—Adaptive cluster sampling (ACS) has been the subject of many publications about sampling aggregated populations. Choosing the criterion value that invokes ACS remains problematic. We address this problem using data from a June 1999 ACS survey for rockfish, specifically for Pacific ocean perch (*Sebastes alutus*), and for shortraker (*S. borealis*) and rougheye (*S. aleutianus*) rockfish combined. Our hypotheses were that ACS would outperform simple random sampling (SRS) for *S. alutus* and would be more applicable for *S. alutus* than for *S. borealis* and *S. aleutianus* combined because populations of *S. alutus* are thought to be more aggregated. Three alternatives for choosing a criterion value were investigated. We chose the strategy that yielded the lowest criterion value and simulated the higher criterion values with the data after the survey. Systematic random sampling was conducted across the whole area to determine the lowest criterion value, and then a new systematic random sample was taken with adaptive sampling around each tow that exceeded the fixed criterion value. ACS yielded gains in precision (SE) over SRS. Bootstrapping showed that the distribution of an ACS estimator is approximately normal, whereas the SRS sampling distribution is skewed and bimodal. Simulation showed that a higher criterion value results in substantially less adaptive sampling with little tradeoff in precision. When time-efficiency was examined, ACS quickly added more samples, but sampling edge units caused this efficiency to be lessened, and the gain in efficiency did not measurably affect our conclusions. ACS for *S. alutus* should be incorporated with a fixed criterion value equal to the top quartile of previously collected survey data. The second hypothesis was confirmed because ACS did not prove to be more effective for *S. borealis*-*S. aleutianus*. Overall, our ACS results were not as optimistic as those previously published in the literature, and indicate the need for further study of this sampling method.

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In nature, populations are sometimes distributed in a patchy, rare, or aggregated manner. Conventional sampling designs such as simple random sampling (SRS) do not take advantage of this spatial differentiation. Thompson (1990) introduced a sampling design called adaptive cluster sampling (ACS) to survey these types of distributions.

Adaptive cluster sampling, in theory, can be much more precise for a given amount of effort than conventional sampling designs (Thompson, 1990). In practice, however, this is not always the case. In some cases, the variance is greatly reduced, but bias is induced from stopping rules and criterion values that are sometimes changed mid-survey (Lo et al., 1997). In 1998, we conducted a survey on Gulf of Alaska rockfish in which ACS was efficient and successful, but the gains in precision, if any, were small compared to those of a SRS of the same size (Quinn et al., 1999; Hanselman et al., 2001).

Recently papers about ACS have included efficiency comparisons (Christman, 1997), restricted ACSs (Lo et al., 1997; Brown and Manly, 1998), bootstrap confidence intervals (Christman and Pontius, 2000), and bias estimates (Su and Quinn, 2003). However, little work has been done on determining the criterion value that, when exceeded, invokes additional sampling. In the following study, we examine the details for choosing this criterion value by using data from a 1999 field survey for Gulf of Alaska rockfish. We then simulate the outcome of the experiment with different criterion values after the survey. We also compare the efficiency of ACS to SRS.

In the basic adaptive cluster sampling (ACS) design, a simple random sample (SRS) of size n is taken; if y (the variable of interest) exceeds c (a criterion value), then neighborhood units are added (e.g. units above, below, left, and right in a cross pattern, Fig. 1) to the sample. These are called network units. If any network unit has $y>c$, then its neighborhood is added. Units that do not exceed the criterion are called edge units, and sampling does not continue around them. This process continues until no units are added or until the boundary of the area is reached (Thompson and Seber, 1996). Neighborhoods can be defined in any general way. The only condition is that if unit i is in the neighborhood of j, then unit j is in the neighborhood of i. The "unbiasedness" of the estimators relies on all neighborhood units of $y>c$ being sampled. If logistics cause the sampling to be curtailed before the sampling is complete, then biased estimators can

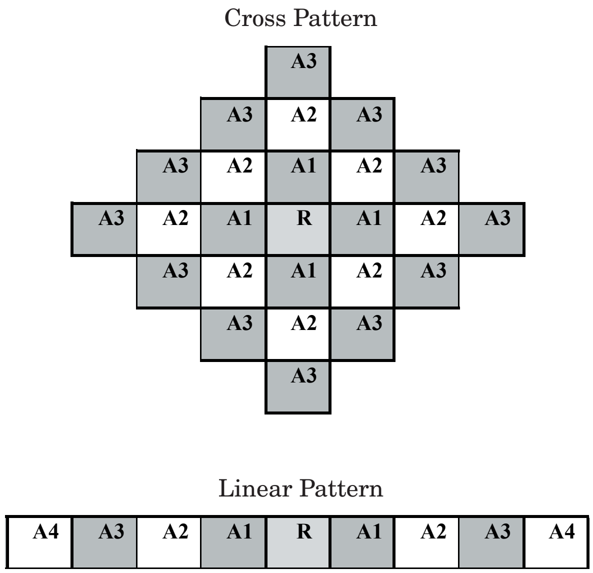

Figure 1

Diagram of a basic cluster sampling design, showing the maximum possible number of adaptive hauls for the cross (S=3) and the linear (S=4) patterns with the imposition of a stopping rule. The initial random tow is denoted as "R," and the adaptive tows as "A" and their respective level number.

result. For our study, all samples were called "tows" because our study was a trawl survey.

When little information is available to preset a fixed criterion value, order statistics are often used to choose a criterion value (Thompson and Seber, 1996). The basic idea is that an initial random sample is conducted. Next, the values of the random tows are ordered, and ACS is conducted around the top r stations. The variable r is decided by the experimenter and depends on the amount of resources available and the suspected aggregation of the population. The criterion value is then set at the value of the next highest tow (r+1). This was the design used in the 1998 adaptive cluster sampling survey for rockfish (Quinn et al., 1999, Hanselman et al., 2001). The use of order statistics has several limitations, however. First, initial random samples must be taken before the adaptive phase can begin. This procedure can be inefficient, because the experiment may have to move a large distance back to the previous tows that exceeded the criterion, by which time the aggregation may have moved or dispersed. In some cases, this procedure may result in a very small criterion value that leads to an overwhelming amount of adaptive sampling around some tows. Second, the process of achieving simple unbiased estimates of abundance is more complicated with order statistics because the criterion value is dependent on the sampling.

In our study, we address methods to avoid these limitations and illustrate these methods with a 1999 ACS survey for Gulf of Alaska rockfish. The primary target of the survey was Pacific ocean perch (*Sebastes alutus* [POP]). These fish have extremely uncertain biomass estimates in the Gulf of Alaska (Heifetz et al.[1]). The estimates are based in part on a standardized stratified random survey conducted by the National Marine Fisheries Service every three years (every two since 2000). This uncertainty is likely due to their highly clustered distribution (Lunsford, 1999) and has led to two independent surveys (1998, 1999) to test the benefits of ACS in sampling POP. Shortraker (*S. borealis*) and rougheye (*S. aleutianus*) rockfish combined (SR-RE) are also tested to compare the results of a population that is considered highly clustered (POP) versus one that is considered more uniformly distributed (SR-RE). SR-RE are combined because they co-occur in identical habitat and are managed as a complex.

Materials and methods

In June 1999, ACS was carried out between 140° and 144° west longitude near Yakutat in the Gulf of Alaska (Fig. 2). Approximately 75% of sampling was directed toward the POP depth stratum (180–300 m) and 25% directed toward SR-RE depths (300–450 m). A 182-ft. factory trawler, the *Unimak*, was chartered to conduct trawl samples. Fishing and field operations are described in Clausen et al.[2] Duration of all trawl hauls was 15 (POP) and 30 (SR-RE) minutes on the bottom. SR-RE tows were made parallel to the depth contours in a linear pattern (Fig. 1) because the slope that SR-RE inhabit is too steep for perpendicular tows. Travel time between all tows was recorded to examine time efficiency.

Initially, a set of systematic random tows was conducted from west to east across the entire study area to determine the criterion value. Samples were chosen systematically by longitude and distributed randomly by depth within each longitudinal strip. This procedure was a necessary proxy for simple random sampling because of poorly known bathymetry in the area. The use of simple random latitudes and longitudes often results in the selection of sites that are well out of the sampling depth interval. After random sampling was completed, we compiled and examined the data to set the criterion value. Criterion values were chosen based on a hierarchy of three alternatives described below. Next, we conducted a new set of random tows from east to west across the area, in which any tows exceeding the criterion value were adaptively sampled. A distance of 0.19 km (0.1 nmi) was used between all adaptive tows and the initial random tow to avoid depletion effects on the catches.

[1] Heifetz, J., D. L. Courtney, D. M. Clausen, J. T. Fujioka, and J. N. Ianelli. 2001. Slope rockfish. *In* Stock assessment and fishery evaluation for the groundfish resources of the Gulf of Alaska, 72 p. North Pacific Fishery Management Council, 605 W. 4th Ave, Suite 306, Anchorage, AK 99501.

[2] Clausen, D. M., D. H. Hanselman, C. Lunsford, T. Quinn II, and J. Heifetz. 1999. *Unimak* enterprise cruise 98-01 rockfish adaptive sampling experiment in the central Gulf of Alaska 1998, 49 p. Auke Bay Lab, NMFS, NOAA, 11305 Glacier Hwy, Auke Bay, Alaska, 99801.

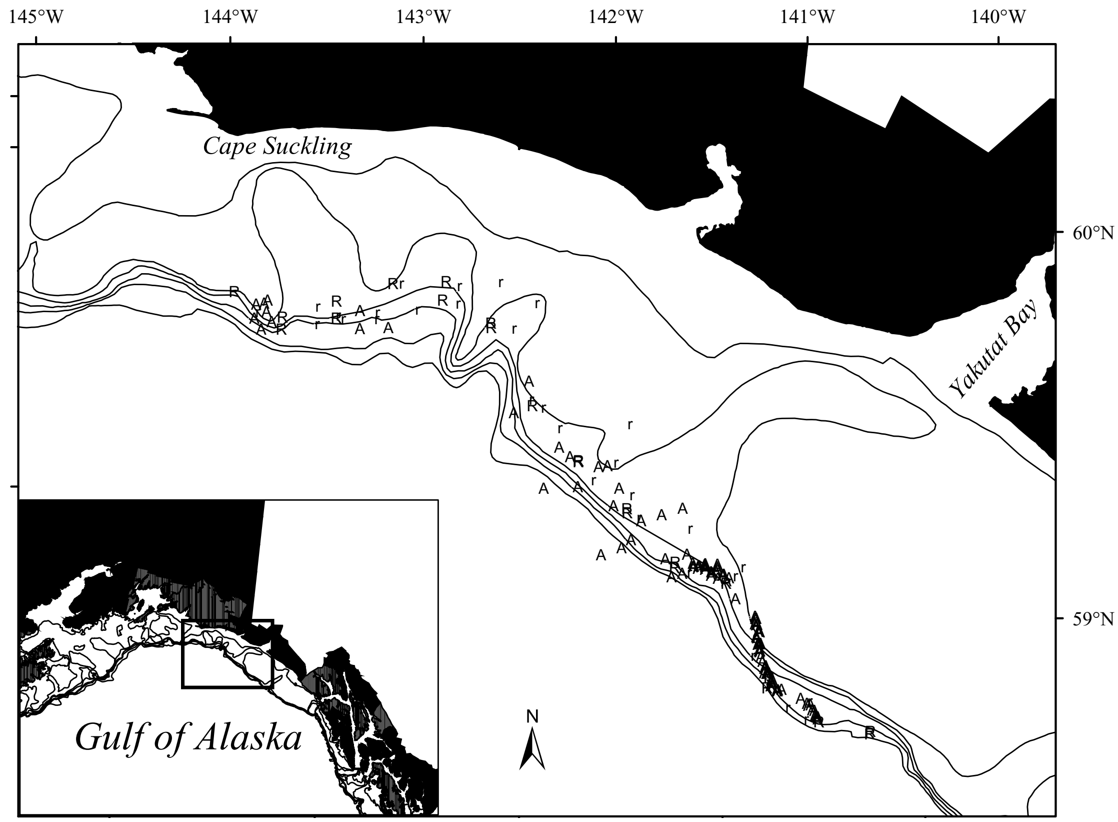

Figure 2

Map of sampling area in the Gulf of Alaska on the *Unimak* 99-01 adaptive sampling cruise. "R" symbols are the initial random tows for the criterion phase, "r" symbols are random stations in the survey phase, "A" symbols are adaptive cluster samples.

Three methods were formulated for determining a fixed criterion value c of POP catch-per-unit-of-effort (CPUE). (1) We combined and calibrated past survey and fishing data to provide the anticipated distribution of CPUE in the 1999 survey. Then we calculated the 80th percentile of that distribution as the criterion value. Our rationale was that this value would correspond to that obtained from order statistics. (Three networks were sampled in 1998; therefore the criterion value was set to the 4th highest of the ordered 15 initial tows, which corresponded approximately to the 80th percentile.) (2) We used the mean CPUE of past survey and fishery data because when we compared the 80th percentile criterion against the 1998 ACS survey's data, the sampling would have resulted in primarily edge units. (3) After a representative random sample was taken across the entire area in 1999, we would use the initial mean CPUE for the criterion value for the return trip. The rationale for using mean CPUE above is that in an aggregated population, the majority of the tows would be less than the mean. The actual values of the criterion chosen under each alternative are described in the results.

We chose the SR-RE criterion to be the mean CPUE of initial tows. We assumed this was a reasonable criterion value because if the population of SR-RE were somewhat uniform, a lower value would result in too much ACS, but mean CPUE would still be low enough to allow higher criterion values to be examined. Although we concentrated on evaluating criterion alternatives for POP, we present the SR-RE data to illustrate that different levels of aggregation could affect how much can be gained with ACS in terms of precision and efficiency.

A major problem in applying adaptive sampling is that sampling may continue indefinitely because of a low criterion value. To limit the amount of adaptive sampling, an arbitrary stopping rule of S levels was imposed. For those strata where the cross pattern of adaptive sampling was used (POP), the stopping rule was $S = 3$ levels, allowing for a maximum of 24 adaptive tows around each high-CPUE random tow (Fig. 1). For the strata with the linear pattern of adaptive sampling (SR-RE), the stopping rule was $S = 4$ levels, for a maximum of eight adaptive tows around each high-CPUE random tow. This stopping rule differs from that of the previous year in which we used a stopping rule of six because we believed that the possible 30-km difference between the ends of the networks was too large for efficient sampling (Clausen[2]). In addition, no adaptive sampling extended beyond a stratum boundary. The result of adaptive sampling around each high-CPUE tow was a network of tows that extended over and, in some cases, delineated the geographic boundaries of a rockfish aggregation.

Statistical analysis of the results was based on adaptive cluster sampling (Thompson and Seber, 1996). First, we estimated the abundance (kg/km) for the targeted rockfish species from the n initial random tows using the standard simple random sampling (SRS) estimator. Then, two adaptive estimators of abundance, a Hansen-Hurwitz estimator (HH) and a Horvitz-Thompson estimator (HT), were calculated. We computed standard error (SE) as a measure of precision. The unbiased HH estimator for the ACS mean is

$$\hat{\mu}_{HH} = \frac{1}{n}\sum_{i=1}^{n} w_i = \frac{1}{n}\sum_{i=1}^{n}\frac{y_i^*}{x_i}, \tag{1}$$

where w_i and y_i^* = the mean and total (respectively) of the x_i observations in the network that intersects sample unit i.

The HH estimator essentially replaces tows around which adaptive sampling occurred with the mean of the network of adaptive tows that exceeded the criterion CPUE.

The unbiased HT estimator for the ACS mean is

$$\hat{\mu}_{HT} = \frac{1}{N}\sum_{k=1}^{\kappa}\frac{y_k^*}{\alpha_k}, \tag{2}$$

where y_k^* = the sum of the y-values for the kth network;
κ = the number of distinct networks in a sample;
α_k = the probability that network k is included in the sample; and
N = the total number of sampling units.

If there are x_k units in the kth network, then

$$\alpha_k = 1 - \binom{N - x_k}{n} \Big/ \binom{N}{n}, \tag{3}$$

where N = the total number of sampling units;
n = the initial random sample; and
x_k = the number of units in the network.

The HT estimator is based on the probability of sampling a network given the initial tows sampled and involves the number of distinct networks sampled (in contrast to the HH estimator which is based only on the initial tows). The HT estimator often outperforms other estimators as seen in simulation studies (Su and Quinn, 2003). Both estimators use the network samples and initial random samples, but not the edge units. This sample size is referred to as ν' (convention established by Thompson (1990) and used in Thompson and Seber (1996)). To include edge units into the estimates Thompson and Seber (1996) and Salehi (1999) used the Rao-Blackwell theorem, which is a complex method that could theoretically result in more precise estimates. However, it had little effect for the 1998 survey data (<1% improvement, Hanselman, 2000); therefore these calculations were not used in our study.

When a stopping rule is used, the theoretical basis for the adaptive sampling design changes. It may result in incomplete networks that overlap and are not fixed in relation to a specified criterion—changing with the pattern of the population. In contrast, the nonstopping-rule scheme has disjoint networks that form a unique partition of the population for a specified criterion. This partitioning is the theoretical basis for the unbiasedness of $\hat{\mu}_{HH}$ and $\hat{\mu}_{HT}$. Thus with a stopping rule, some bias may be introduced.

Recent simulation studies (Su and Quinn, 2003) have estimated the bias induced by using a stopping rule on each estimator with order statistics, but not with a fixed criterion. Because the use of a fixed criterion is design unbiased, its estimate should be less biased by the stopping rule than a sample with order statistics. Therefore, we can use the Su-Quinn simulation results to approximate the maximum bias induced by the stopping rule. With a stopping rule of three and the HH estimator, the maximum positive bias is 17% for a highly aggregated simulated population. With a stopping rule of three and the HT estimator, the maximum bias is approximately 12%. Considering our design, we accepted the tradeoff of relatively small bias for gains in precision and logistical efficiency.

Additionally, nonparametric bootstrap methods were adapted from Christman and Pontius (2000) and we used the HH version of the estimates to examine bias from our survey. Five thousand resamples were performed by using n for the SRS bootstrap, and the sample size from the original criterion value of 220 kg/km (ν') was used for the ACS bootstrap. Bootstrap distributions of the data were examined for SRS and ACS designs to examine the capability of each design to clearly demonstrate a central tendency.

We evaluated two hypotheses: 1) Adaptive sampling would be more effective in providing precise estimates of POP biomass than would a simple random survey design; and 2) Assessment of POP abundance would benefit more from an adaptive sampling design than would SR-RE because POP are believed to be more clustered in their distribution than SR-RE. SRS estimates were obtained from the initial random tows, and variance estimates were calculated for the initial sample size (n) and for the equivalent sample size that included the adaptive tows but not the edge units (ν'). This procedure makes the theoretical comparison fair because each estimate is based on the same number of samples. Total sample size including edge units (ν) was not used in the theoretical precision comparison but was considered when efficiency issues were examined later. These hypotheses were assessed by comparing the standard errors (SEs) of ACS to those of SRS. Substantial reductions in SE with ACS for POP would support the first hypothesis, whereas no reductions of SE using ACS for SR-RE would support the second hypothesis. This comparison is qualitative because relevant significance tests are unavailable and the two methods are different in terms of efficiency.

To evaluate different alternatives and criterion values, each network was reconstructed as if the higher criterion values had been used in the field. We also examined the tradeoff between amounts of additional sampling compared with the gains in precision. A comparison was made of the SRS results by using sample sizes constructed with the number of possible samples with the time-per-sample

Table 1

Data used to determine criterion values c for the 1999 adaptive cluster sampling (ACS) survey. Data from a 1998 ACS survey from a different area is divided by the National Marine Fisheries Service triennial survey data and fishery data from the same area to obtain gear efficiency values. The mean of these gear efficiencies are then multiplied against triennial and fishery data from the new area to yield gear-calibrated CPUEs for the new area. Only numbers in bold were used in calculations. n = the number of observations of that data set; 80% = the 80th percentile catch of that data set.

Data source	Year	Mean CPUE (kg/km)	80%	n
ACS results from different area and year	1998	**284.94**	**223.92**	57
(divided by)			÷	
CPUEs of corresponding previous area from triennial and fishery data	Triennial 1993	**38.36**	**7.89**	50
	1996	**46.64**	**27.33**	51
	1993–96	**42.54**	**18.79**	101
	Fishery 1996–98	**30.64**	**14.03**	434
(equals)			=	
Gear efficiency of the *Unimak*	1993	7.44	28.18	
	1996	6.12	8.14	
	1993–96	6.71	11.84	
	1996–98	9.32	15.85	
	Mean	**7.63**	**17.39**	
(multiplied by)			×	
Prior CPUE data from area for 1999 ACS survey	Triennial 1993	**40.32**	**46.74**	29
	1996	**26.50**	**33.50**	25
	1993–96	**33.92**	**38.85**	54
	Fishery 1996–98	**19.61**	**30.47**	190
(equals)			=	
Calibrated CPUE data for 1999 ACS survey	Triennial 1993	**307.52**	**812.67**	29
	1996	**202.06**	**582.52**	25
	1993–96	**258.69**	**675.63**	54
	Fishery 1996–98	**149.57**	**529.90**	137
Criterion value c	Mean	**219.71**	**641.69**	

data we collected. In this comparison we used three new sample sizes: 1) v_t, the number of samples that could have been taken in the same amount of time as that for a SRS if sampling time for edge units was negligible; 2) v_e, in which the edge units had taken the same amount of time as non-edge units; and 3) v_d, in which the average distance between each tow type was used as effort instead of time (with edge units included).

Results

Formulation of criterion alternatives

A total of 164 tows were conducted for the ACS experiment. Nearly all tows were made successfully; only a few exceptions were deemed untrawlable and moved to the nearest trawlable bottom. We determined the POP criterion value for alternatives 1 and 2 (see below) before the survey by looking at the 1998 ACS results from a different geographic area, as well as prior survey and fishery data in our study area. We obtained the criterion value by calculating a gear efficiency coefficient for the 1998 survey by using NMFS survey data (1993, 1996) and fishery data (1996–98) from the observer program for the same area. This gear coefficient was then multiplied by the same data for the new area to establish the expected catches. The data used and the calculations are shown in Table 1. To implement alternative 3, we conducted 13 initial POP and 10 initial SR-RE random tows across the entire area. Catches from these initial tows gave us the following results for each criterion alternative:

Alternative 1 For alternative 1, the mean of the 80th percentile of the data from Table 1 is 641.69 kg/km. We rounded this downward to c = 540 kg/km (1000 kg/nmi) for ease of operation in the field (the design was originally in kg/nmi units).

Alternative 2 The mean calibrated CPUE for the area from Table 1 yielded a criterion value c of 220 kg/km (rounded).

Alternative 3 In this alternative, the mean CPUEs from the initial sample in 1999 yielded criterion values of c = 250 kg/km for POP and c = 418 kg/km for SR-RE.

Table 2

Summary of density estimates ($\hat{\mu}$) and standard errors (SE) for the 1999 adaptive cluster sampling experiment for the *Sebastes alutus* and the *S. borealis-S. aleutianus* complex. c is the criterion value, r is the number of adaptive networks, n is the initial sample size, ν' is the adaptive sampling size (excluding edge units). SRS = simple random sampling estimator, HH = Hansen-Hurwitz adaptive estimator, and HT = Horvitz-Thompson adaptive estimator. Alt. = criterion alternative.

	Sebastes alutus				*Sebastes borealis* and *S. aleutianus*	
	Alt. 2	Alt. 3	Alt. 1	—	Alt. 3	—
c (kg/km)	>220	>250	>540	>1080	>418	>540
r	6	6	5	3	5	3
n	25	25	25	25	9	9
ν'	74	73	55	48	30	14
$\hat{\mu}_{SRS}$	904	904	904	904	447	447
SE_n	496	496	496	496	115	115
SE_{ν}'	288	290	334	358	63	92
$\hat{\mu}_{HH}$	498	501	566	526	511	486
SE	166	167	192	197	128	141
$\hat{\mu}_{HT}$	471	472	567	527	511	486
SE	167	167	192	197	128	141

The second phase of the experiment began with random tows in an east to west direction. Complete location and CPUE data for both species are located in Appendix I. In order to analyze all alternatives, the lowest alternative was used in the field for adaptive sampling during the second phase, which resulted in the 220 kg/km criterion value for POP from alternative 2. For SR-RE, the criterion value was the mean CPUE of 418 kg/km from alternative 3. The remaining alternatives were simulated following the completion of the survey.

POP results

After the initial tows, 25 random tows were selected for the return trip across the area. All 25 were completed, of which six became networks of more than one unit. A total of 106 tows were completed in the POP stratum. At one of the tows that exceeded the criterion value, the captain deemed that further adaptive sampling was not feasible because of the presence of coral. Of the six networks, two overlapped, resulting in five distinct networks. In these networks, 81 adaptive samples were taken, of which 49 exceeded the criterion and 32 did not and were therefore edge units and not included in the sample estimates.

We compared the results of the original adaptive sample (alternative 2) with the simulated results of higher criterion values (Table 2). The precision of simple random sample estimates with both n (number of random samples) and ν' (number of random samples plus the number of adaptive network samples, not edge units) was contrasted with that of the adaptive estimators described above. As the criterion value increased, n remained the same, whereas ν' and r (the number of networks) decreased. At the 220 kg/km criterion value (alt. 2), there were substantial reductions in SE over the SRS estimators by using ACS estimators for both the n and ν' sample sizes. The 250 kg/km criterion value (alt. 3) resulted in a nearly identical sample to that of the 220 kg/km (alt. 2) criterion value and the loss of only one network sample. Hence, the estimates were nearly identical. The HT mean estimates were slightly lower than the HH estimates for the two lowest criterion values (alts. 2 and 3) because two networks overlapped. These networks became separate at the next higher criterion value, which aligned the estimators. The next highest criterion value of 540 kg/km (alt. 1) showed that even though the sample size was reduced by 19 tows from the original criterion value, the ACS estimators performed nearly as well, yielding just slightly larger SEs. When the criterion was arbitrarily doubled to 1080 kg/km, the sample size was further reduced by seven, and had similar SEs to the 540 kg/km criterion value.

The SRS and ACS bootstraps for POP resulted in very different distributions. Five thousand replications showed that the SRS distribution was bimodal and right skewed (Fig. 3). The SRS mean fell on the second mode, which is more than twice the ACS mean. This bimodal distribution is driven by the presence of the very large random catch (tow no. 60). If that haul is present in a bootstrap replicate, then the SRS estimate tends to be high, leading to the second mode in the bootstrap distribution. The ACS bootstrap distribution was symmetric and closely resembled a normal distribution (Fig. 3). The average estimates of bias showed that the bias of HH was +4% and the bias of HT was –1%. The standard error had an estimated bias of +3% for HH and HT.

The results from this POP study and the previous 1998 study were both greatly affected by one or two very large catches, as we expected for a highly clustered population. Of interest is what happened when the largest catch was changed to a nominal catch that still exceeded the criterion value. Appendix II shows the results of changing haul no. 60 from 12,000 kg/km to 540 kg/km. In the comparison at ν', SRS outperforms ACS in terms of SE. However, it also

Table 3

Comparisons of time per travel (TPT) and time per sample (TPS) of adaptive sampling against simple random sampling for Pacific ocean perch (*S. alutus*) and for shortraker (*Sebastes borealis*) and rougheye (*S. aleutianus*) rockfish combined, on a 1999 adaptive sampling cruise. TPT is the travel time between tows in hours; TPS is the travel time plus haul time in hours. "Distance between" is the average travel distance (km) between two adaptive stations and between two random stations. "Adjusted distance" is the distance if the random sample size was increased to 106.

	S. alutus		*S. borealis* and *S. aleutianus*	
	Random	Adaptive	Random	Adaptive
Time (h)	10.4	11.4	4.4	12.0
No. of hauls	23	72	9	24
TPT	0.45	0.16	0.49	0.50
TPS	0.95	0.66	1.49	1.50
Distance between	20.2	3.22		
Adjusted distance	4.73	3.22		

shows that the mean of ACS is stable because it changes little by removing a high catch, whereas the SRS mean is reduced by half.

SR-RE results

At every third POP random tow, a tow was made in the SR-RE depth stratum. A total of 35 tows were made in the SR-RE stratum. Nine random tows yielded five distinct networks with 21 network tows and five edge units. The stopping rule was invoked for three of the five networks.

At the mean CPUE criterion (418 kg/km, alt. 3), the adaptive estimators performed approximately the same in terms of SE compared to the SRS estimator using n (Table 2). With ν', the SRS estimator yielded a lower SE than both adaptive estimators. When the criterion value increased to an arbitrarily higher value (540 kg/km), the adaptive estimators performed worse than SRS estimates for both n and ν'.

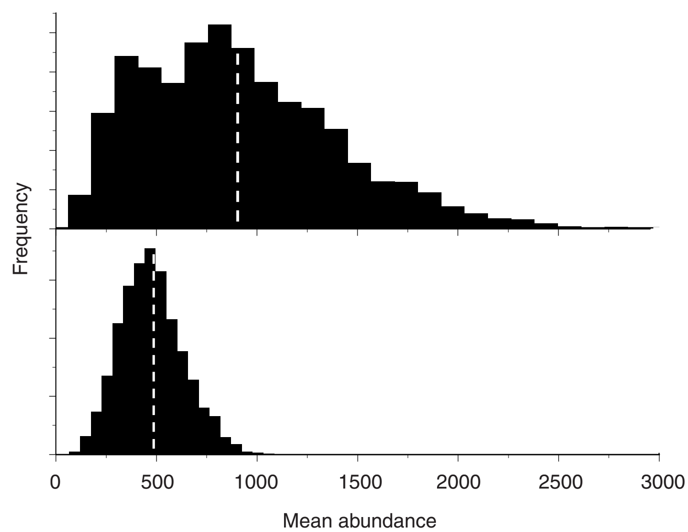

Figure 3

Bootstrap distributions for the 1999 adaptive sampling survey (25,000 replicates). Dotted line is the sampling estimate of mean abundance (kg/km) from the survey. Top graph is the distribution of mean abundance estimates for simple random sampling. Bottom graph is the distribution of mean abundance estimates for adaptive cluster sampling (obtained with the Hansen-Hurwitz estimator).

Time efficiency

We recorded and compared travel time between adaptive tows and simple random tows for 149 of the tows (Table 3). Not all the tows were used because of mechanical failure or because the factory capacity was reached. In the survey, 38 hours out of 10 days were spent in transit between sampling tows, which for a short survey was a substantial amount of the available time. For POP, substantial gains in travel-time efficiency were achieved with ACS. Average travel time for simple random tows (0.45 h) was nearly triple that of adaptive tows (0.16 h) for POP, which indicated that ACS can maximize sampling tows for POP when time is limited. In the SR-RE sampling, travel time for adaptive sampling (0.5 h) was about the same as simple random sampling (0.49 h), which was due to long linear samples that are not as close together as POP tows (Fig. 1). Also, determination of CPUE required processing of the catch, which took various amounts of time after the completion of the tow. Because of this delay, we went to the opposite tow on the other side of the random tow when sampling SR-RE with the linear pattern, whereas there were many nearby tows when sampling POP with the cross pattern.

The travel time was added to the average tow time from gear deployment to full retrieval of 0.5 h for POP and 1.0 h for SR-RE to obtain total sampling time (per sample). Travel time was reduced by 31% with adaptive sampling (0.66 h/sample) in relation to simple random sampling

(0.95 h/sample) for POP. Sampling time efficiency for SR-RE was approximately the same for adaptive sampling (1.5 h/sample) and simple random sampling (1.49 h/sample) for SR-RE. These results are confounded by the fact that the random tows are spread apart because of the lesser effort applied to them. The average distance between random tows (20.2 km) was adjusted to a distance of 4.73 km as if there were 106 random tows distributed throughout the area. This distance is still larger than the average distance between tows in adaptive sampling (3.22 km).

From these time and distance data, we re-estimated the precision of SRS under three new sample sizes in order to further compare the relative efficiency of ACS. We denoted the sample size that could have been taken under SRS, using the same amount of time as was used during the adaptive sampling including edge units, as ν_e. An alternative sample size ν_t was the equivalent SRS sample size if the amount of time to sample edge units in ACS was negligible. This statistic would be useful if edge units could be determined (i.e. hydroacoustically or visually [presence or absence]) without actually trawling them. A third alternative was to find the equivalent SRS sample size ν_d that would result from applying the total distance traveled in the ACS design on random stations instead. For ν_e, more random POP samples would have been taken than were included in the adaptive estimators (Table 4). The SEs of ACS were still much lower across all criterion values (Table 2). When we used ν_t (Table 4), SRS was much less precise than ACS (Table 2). Finally, when we used distance instead of time (ν_d), the results were almost exactly the same as those for ν_e (Table 4).

Table 4

Comparison of simple random sampling (SRS) precision estimates with the inclusion of time and distance information. c is the criterion value. ν' is the original adaptive cluster sampling adjusted sample size. ν_e is the time-adjusted sample size, including edge units. ν_t is the time-adjusted sample size with edge unit cost set to zero. ν_d is the distance-adjusted sample size including edge units. $\hat{\mu}$ is the mean SRS density estimate, SE is the standard error for that sample size.

	c (kg/km)			
	>220	>250	>540	>1080
$\hat{\mu}$	904	904	904	904
ν'	74	73	55	48
SE	294	296	341	365
ν_e	81	80	67	55
SE	281	283	309	341
ν_t	59	58	46	41
SE	329	332	373	395
ν_d	80	79	67	54
SE	283	285	309	344

Discussion

Our two hypotheses were that ACS would be more precise than SRS for POP and no more precise for SR-RE combined. The results from the 1999 field study showed that the SEs for the adaptive POP estimates were smaller than both SRS estimates, with n and ν', and thus support the first hypothesis. One curious result is that in both 1998 and 1999, the SRS estimate of density was substantially larger than the ACS estimate, even though, on average, they were both essentially unbiased. We attributed this curiosity to the more variable and skewed SRS distribution in which large sampling error on the high side is possible more often than in the ACS estimation. Of course we fully expected that both estimates would average to be the same value if the experiment could be repeated many times. ACS reduced the influence of one large CPUE in the relatively small initial sample, as illustrated by the symmetric and near-normal shape of the ACS bootstrap distribution. Consequently, we concluded that ACS is a more robust estimator of density than SRS for aggregated populations. One caveat is that the precision of the estimates, if measured in terms of coefficient of variation, is similar between the two methods because of the much larger mean estimate for the SRS estimate. Monte Carlo simulations would be useful to examine the properties of the estimators under different criterion values and population densities along the lines of Su and Quinn (2003).

The SR-RE adaptive estimates all have higher SEs than the SRS estimates, and this finding supports the second hypothesis. More than twice as many samples were directed toward POP than SR-RE, yet the POP density estimates are much more variable than those for SR-RE. This much larger variability for POP was indicative of the clustering that we expected.

This experiment showed that for POP, ACS with a fixed criterion has some distinct advantages over simple random sampling and over adaptive cluster sampling with order statistics, which was used in the previous 1998 survey. Lower SEs were obtained, at one third less effort than if we just added an equivalent number of random samples. Sampling over a broader area yielded better results than the tightly stratified 1998 design. Our study also assumed stationary aggregations of fish. This assumption may have been better satisfied with a fixed criterion because the adaptive sampling was conducted immediately after a sample exceeded the criterion value.

Although the fixed criterion eliminates bias induced by a variable criterion value, we still used stopping rules. If bootstrapping is a good indicator of bias, then the bias induced by stopping rules is negligible. Additionally, we have shown that a relatively high criterion value could be used to help minimize the use of these stopping rules.

Our study showed that ACS is a fast and efficient way to gain a large number of samples. However, if edge units do not contribute to a better estimate and they have a similar cost or time expense as included samples, then little is gained. This deficiency shows the need for some method of determining edge units without actually sampling them. In fisheries surveys, this use might be a double sampling design with hydroacoustics as an auxiliary variable

(Fujioka[3]) or a design called TAPAS that hydroacoustically delineates clusters (Everson et al., 1996). In other surveys, it might be possible to detect the presence of the item of interest without actually surveying the unit (as in aerial surveys.)

An ACS design should not be attempted without some prior knowledge of the population distribution. Populations for which the design would be useful should have an aggregated distribution that can be described by correlated variation with distance, not just a large variance in relation to the mean. One way to examine the data is to fit variograms to examine spatial autocorrelation (Hanselman et al., 2001). If no prior data exist, it would not make sense to attempt ACS as an initial sampling design. We have shown that a wide range of criterion values can be used without considerable differences in the results. Therefore, only enough prior data are needed so that an adequate range of population density can be estimated. If the criterion value chosen resulted in too many or too few samples, the criterion could be adjusted, and then the design stratified into two different areas.

Most commercial fish species have survey data that can be used to determine a fixed criterion. If possible, criterion values should be determined prior to the survey, so that maximum efficiency can be attained. We have shown that it may be appropriate to choose a relatively high sampling criterion such as the 80th percentile of past CPUE without sacrificing estimation capabilities. This high sampling criterion has several practical advantages. First, the design is attractive for commercial boats to perform the adaptive phase at no-cost because only large catches are sampled. The current design does not use the fish sampled during the survey, which, in the case of deepwater rockfish, would cause certain mortality. Under an adaptive design, a commercial boat would take the larger catches and could put them to use. Second, fewer overall networks would be sampled because the higher criterion would evoke less adaptive sampling, which may mean less overall sampling in the survey. Finally, precision would be gained at a minimal cost and effort. Stopping rules would be unnecessary, ensuring an unbiased estimate. However, cluster sampling is most effective when the cluster samples are as heterogeneous as possible. Therefore, caution is required not to set the criterion too high, or the resulting clusters will be either too homogeneous or contain only edge units, leading to no improvement in the estimators. Similarly, if there are large changes in density from year to year, a fixed criterion may not be appropriate. In conclusion, adaptive cluster sampling is appropriate for surveys of highly clustered species with low temporal fluctuations, for which a fixed criterion can be determined beforehand.

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Appendix I

CPUE (kg/km) data from the 1999 adaptive cluster sampling survey. CPUE is given in kg/km. The format of "Adaptive 26-1" corresponds to the first adaptive tow around haul no. 26. POP = Pacific ocean perch; SR-RE = shortraker and rougheye rockfish combined.

Summary table

Tow type	Initial random	2nd phase random	Adaptive network	Adaptive edge unit	Total[1]
POP	13	25	49	32	106 (119)
SR-RE	10	9	21	5	35 (45)
Total	23	34	70	37	141 (164)

[1] Values in parenthesis include initial random tows that are not included in estimation results.

Criterion determining random tows

Tow	Latitude	Longitude	Tow type	POP CPUE	SR-RE CPUE
3	59.59	–143.81	POP random	39.3	43.7
4	59.54	–143.55	POP random	49.2	13.7
5	59.51	–143.55	SR-RE random	3.4	870.9
6	59.58	–143.28	POP random	174.8	112.0
7	59.56	–143.28	SR-RE random	17.7	582.3
8	59.67	–143.01	POP random	72.7	21.0
9	59.69	–142.75	POP random	21.3	6.1
10	59.64	–142.75	SR–RE random	6.3	6.3
11	59.60	–142.49	POP random	9.6	36.2
12	59.59	–142.48	SR–RE random	3.8	608.0
13	59.40	–142.22	POP random	20.7	113.0
14	59.28	–141.96	POP random	25.3	394.4
15	59.27	–141.96	SR–RE random	19.1	713.1
16	59.17	–141.68	POP random	185.4	68.5
17	59.16	–141.68	SR–RE random	24.9	48.5
18	59.04	–141.41	SR–RE random	1.7	450.4
19	59.03	–141.41	POP random	196.5	21.9
20	59.01	–141.14	SR–RE random	30.0	676.9
21	58.78	–140.88	POP random	2271.6	0.0
22	58.75	–140.88	SR–RE random	65.9	80.6
23	58.67	–140.61	POP random	80.6	101.1
24	58.66	–140.35	POP random	98.2	55.0
25	58.66	–140.35	SR–RE random	21.2	140.5
			Beginning of adaptive random tows		
26	58.70	–140.64	POP random	576.7	0.0
27	58.68	–140.65	SR–RE random	16.3	115.8
28	58.73	–140.71	POP adaptive 26-1	138.1	12.0
29	58.72	–140.65	POP adaptive 26-2	138.4	9.7
30	58.69	–140.62	POP adaptive 26-3	2294.2	0.0
31	58.70	–140.64	POP adaptive 26-4	290.1	0.4
32	58.70	–140.63	POP adaptive 26-8	334.8	0.0
33	58.69	–140.62	POP adaptive 26-9	56.5	21.2
34	58.69	–140.63	POP adaptive 26-10	16.4	1.9
35	58.71	–140.67	POP adaptive 26-11	20.7	3.7
36	58.72	–140.67	POP adaptive 26-12	30.2	1.0

continued

Appendix I (continued)

Criterion determining random tows

Tow	Latitude	Longitude	Tow type	POP CPUE	SR-RE CPUE
37	58.69	–140.61	POP adaptive 26-18	1299.4	1.2
38	58.69	–140.61	POP adaptive 26-17	965.0	55.9
39	58.70	–140.75	POP random	62.0	148.0
40	58.76	–140.85	POP Random	3591.0	58.4
41	58.79	–140.89	POP adaptive 40-1	5934.1	0.0
42	58.77	–140.86	POP adaptive 40-2	4521.0	0.0
43	58.74	–140.83	POP adaptive 40-3	515.7	9.1
44	58.76	–140.86	POP adaptive 40-4	4453.7	37.3
45	58.79	–140.90	POP adaptive 40-5	1338.8	0.0
46	58.79	–140.88	POP adaptive 40-6	393.9	0.0
47	58.77	–140.86	POP adaptive 40-7	109.4	0.0
48	58.75	–140.82	POP adaptive 40-8	85.0	0.0
49	58.73	–140.80	POP adaptive 40-9	67.9	0.1
50	58.74	–140.83	POP adaptive 40-10	128.0	17.6
51	58.76	–140.86	POP adaptive 40-11	1597.3	0.0
52	58.78	–140.89	POP adaptive 40-12	268.5	3.8
53	58.80	–140.90	POP adaptive 40-24	1282.9	0.0
54	58.81	–140.92	POP adaptive 40-13	2304.4	0.0
55	58.80	–140.90	POP adaptive 40-14	776.2	0.0
56	58.79	–140.88	POP adaptive 40-15	882.6	0.0
57	58.75	–140.86	POP adaptive 40-22	168.1	2.7
58	58.78	–140.89	POP Adaptive 40-23	253.9	0.2
59	58.83	–140.95	SR-RE random	24.1	290.2
60	58.88	–140.95	POP random	12001.5	0.0
61	58.87	–140.96	POP adaptive 60-4	10659.3	0.0
62	58.91	–140.97	POP adaptive 60-1	1179.0	0.0
63	58.89	–140.95	POP adaptive 60-2	3050.4	0.0
64	58.86	–140.95	POP adaptive 60-3	2984.7	0.0
65	58.86	–140.95	POP adaptive 60-10	3590.4	0.0
66	58.88	–140.96	POP adaptive 60-11	1086.9	0.0
67	58.91	–140.98	POP adaptive 60-12	1311.7	8.7
68	58.92	–140.98	POP adaptive 60-5	1581.0	0.0
69	58.91	–140.96	POP adaptive 60-6	4148.4	0.0
70	58.89	–140.95	POP adaptive 60-7	1297.4	0.0
71	58.86	–140.94	POP adaptive 60-8	214.1	0.0
72	58.84	–140.94	POP adaptive 60-9	2190.3	0.0
73	58.84	–140.94	POP adaptive 60-20	1502.2	0.0
74	58.83	–140.93	POP adaptive 60-19	2828.9	0.0
75	58.84	–140.93	POP adaptive 60-18	102.9	0.0
76	58.86	–140.94	POP adaptive 60-17	46.6	0.0
77	58.89	–140.95	POP adaptive 60-16	27.8	0.0
78	58.89	–140.95	POP adaptive 60-15	53.4	0.0
79	58.92	–140.97	POP adaptive 60-14	495.7	0.0
80	58.93	–140.98	POP adaptive 60-13	1323.4	0.0
81	59.05	–141.05	POP random	1448.8	0.4
82			Coral encountered	N/A	N/A
83	59.03	–141.08	POP random	560.6	102.8
84	59.03	–141.19	POP random	283.6	298.5
85	59.04	–141.19	POP adaptive 83-1	1119.7	101.3
86	59.04	–141.26	POP adaptive 83-2	1407.0	21.7
87	59.02	–141.22	POP adaptive 83-3	398.1	29.2

continued

Appendix I (continued)

Criterion determining random tows

Tow	Latitude	Longitude	Tow type	POP CPUE	SR-RE CPUE
88	59.03	–141.16	POP adaptive 83-4	264.6	87.0
89	59.05	–141.20	POP adaptive 83-5	416.6	47.3
90	59.04	–141.29	POP adaptive 83-6	2186.1	7.0
91	59.04	–141.25	POP adaptive 83-7	482.0	8.7
92	59.03	–141.22	POP adaptive 83-8	115.2	36.6
93	59.02	–141.19	POP adaptive 83-9	182.5	36.4
94	59.02	–141.13	POP adaptive 83-10	41.4	45.5
95	59.02	–141.16	POP adaptive 83-11	29.2	41.1
96	59.04	–141.20	POP adaptive 83-12	261.4	80.6
97	59.04	–141.25	POP adaptive 83-24	109.3	32.0
98	59.04	–141.29	POP adaptive 83-23	62.0	69.4
99	59.05	–141.26	POP adaptive 83-13	186.4	56.2
100	59.05	–141.32	POP adaptive 83-14	443.8	4.5
101	59.04	–141.29	POP adaptive 83-15	1497.1	5.4
102	59.04	–141.25	POP adaptive 83-16	892.0	21.4
103	59.03	–141.22	POP adaptive 83-17	604.8	26.1
104	59.03	–141.16	POP adaptive 84-3	123.5	91.4
105	59.03	–141.22	POP adaptive 84-4	129.3	285.3
106	59.04	–141.26	POP adaptive 84-1	231.2	602.5
107	59.02	–141.32	SR-RE random	49.3	721.9
108	59.05	–141.26	POP adaptive 84-5	214.6	1408.9
109	59.04	–141.35	POP adaptive 84-6	215.0	123.6
110	59.04	–141.31	POP adaptive 84-12	61.5	664.5
111	59.04	–141.32	SR-RE adaptive 107-1	57.5	758.1
112	59.02	–141.37	SR-RE adaptive 107-2	0.0	490.7
113	59.05	–141.20	SR-RE adaptive 107-3	0.0	408.6
114	59.01	–141.42	SR-RE adaptive 107-4	0.0	669.1
115	59.00	–141.14	SR-RE adaptive 107-6	0.0	760.8
116	58.97	–141.09	SR-RE adaptive 107-8	0.0	1540.6
117	58.11	–141.06	SR-RE random	0.0	443.2
118	59.14	–141.60	SR-RE adaptive 117-1	0.0	1052.8
119	59.09	–141.64	SR-RE adaptive 117-2	0.0	1042.0
120	59.16	–141.50	SR-RE adaptive 117-3	51.3	621.6
121	59.07	–141.69	SR-RE adaptive 117-4	25.7	2096.7
122	59.05	–141.46	SR-RE adaptive 117-6	68.4	480.5
123	59.19	–141.40	SR-RE adaptive 117-5	41.2	924.3
124	59.21	–141.73	SR-RE adaptive 117-7	189.0	731.9
125	59.04	–141.78	SR-RE adaptive 117-8	82.3	772.2
126	59.14	–141.34	POP random	61.9	4.8
127	59.15	–141.60	POP random	82.6	55.8
128	59.21	–141.65	POP random	68.5	8.1
129	59.29	–141.75	POP random	84.6	0.0
130	59.23	–141.85	SR-RE random	6.1	1024.1
131	59.27	–141.85	SR-RE adaptive 130-1	2.6	626.9
132	59.21	–141.94	SR-RE adaptive 130-2	1.5	451.9
133	59.27	–141.81	SR-RE adaptive 130-3	4.2	2208.3
134	59.28	–142.00	SR-RE adaptive 130-5	7.4	1605.6
135	59.31	–142.06	SR-RE adaptive 130-7	5.0	1305.2
136	59.19	–142.11	SR-RE adaptive 130-4	0.0	432.4
137	59.17	–141.75	SR-RE adaptive 130-6	1.6	457.4
138	59.39	–141.70	POP random	181.8	25.9
139	59.36	–142.05	POP random	62.9	12.2

continued

Appendix I (continued)

Criterion determining random tows

Tow	Latitude	Longitude	Tow type	POP CPUE	SR-RE CPUE
140	59.40	–142.15	SR-RE random	3.7	772.3
141	59.45	–142.25	SRRE adaptive 140-1	1.1	222.7
142	59.38	–142.31	SRRE adaptive 140-2	0.0	209.0
143	59.42	–142.22	POP random	177.2	36.0
144	59.67	–142.25	POP random	45.4	33.5
145	59.60	–142.35	POP random	8.3	117.8
146	59.71	–142.45	POP random	4.3	32.0
147	59.67	–142.65	SR-RE random	2.0	47.0
148	59.64	–142.65	POP random	18.0	50.8
149	59.67	–142.95	POP random	34.2	3.4
150	59.61	–142.85	POP random	125.0	18.8
151	59.57	–143.05	SR-RE random	3.6	530.5
152	59.59	–143.05	POP random	139.0	39.7
153	59.56	–143.15	SR-RE adaptive 151-1	5.1	555.2
154	59.59	–143.16	SR-RE adaptive 151-2	2.6	255.5
155	59.55	–143.00	SR-RE adaptive 151-3	0.0	314.5
156	59.56	–143.22	POP random	23.5	567.4
157	59.57	–143.25	POP random	43.3	399.3
158	59.54	–143.35	SR-RE random	9.3	82.2
159	59.58	–143.36	POP random	74.9	493.0
160	59.55	–143.45	POP random	2838.5	1.8
161	59.57	–143.65	POP adaptive 160-1	1674.5	54.5
162	59.53	–143.69	POP adaptive 160-2	2912.8	1.8
163	59.55	–143.63	POP adaptive 160-3	196.5	0.0
164	59.52	–143.65	POP adaptive 160-4	148.2	0.5
165	59.52	–143.60	POP adaptive 160-5	75.6	21.0
166	59.58	–143.63	POP adaptive 160-6	863.1	9.4
167	59.56	–143.69	POP adaptive 160-7	41.3	0.0

Appendix II

Results of estimation with haul no. 60 changed from 12000 kg/km to 540 kg/km. c is the criterion value (kg/km), $\hat{\mu}$ is the mean Pacific ocean perch density (kg/km) for each estimator, n is the random sample size, ν' is the adaptive sample size without edge units. SE is the standard error of the mean.

	c (kg/km)					c (kg/km)			
	>220	>250	>540	>1080		>220	>250	>540	>1080
$\hat{\mu}_{srs}(n)$	445	445	445	445	SE	148	149	175	158
SE	179	179	179	179	$\hat{\mu}_{HT}$	442	443	536	413
SE (ν')	104	104	104	104	SE	149	149	175	158
$\hat{\mu}_{HH}$	470	473	535	412					