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GUIDELINES FOR USING DOUBLE SAMPLING IN AVIAN POPULATION MONITORING

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Abstract.—Double sampling involves combining information from inexpensive rapid counts with intensive complete counts to provide an efficient population estimate. This technique is a valuable approach for calibrating population indices based on incomplete counts and improving the precision of monitoring studies. Data collected through a double-sampling protocol can be analyzed with either a ratio or a regression estimator. The ratio estimator is recommended when the relationship of the actual count to the rapid count is a straight line through origin, which may not be valid when surveying populations that have a small number of individuals per site and a low detection probability. In such situations, the regression estimator may be more appropriate. I investigated the properties of these two different estimators through a simulation study and tabulated sample sizes to control bias of the population average and standard error. Further, I used the results to evaluate when double sampling is a cost-effective design and how to design surveys that meet precision requirements. The design process is illustrated with the Spring Eastern Waterfowl Survey, which shows that double sampling is not always appropriate. *Received 28 February 2006, accepted 21 December 2006.*

Key words: detection probability, double sampling, ratio estimator, regression estimator, simulation.

Directives pour l'utilisation de l'échantillonnage double dans le suivi des populations d'oiseaux

Résumé.—L'échantillonnage double implique la combinaison d'informations provenant de décomptes rapides et peu coûteux et de décomptes intensifs complets afin de fournir un bon estimé de population. Cette technique est une approche très utile pour calibrer les indices de population basés sur des décomptes incomplets et améliorer la précision des études de suivi. Les données recueillies à l'aide d'un protocole de double échantillonnage peuvent être analysées soit par un rapport, soit par une régression. Le rapport est recommandé lorsque la relation entre le décompte réel et le décompte rapide est une ligne droite passant par l'origine, ce qui peut ne pas être valide lorsque l'on étudie des populations qui ont un petit nombre d'individus par site et une faible probabilité de détection. Dans de telles situations, la régression peut être plus appropriée. J'ai étudié les propriétés de ces deux différents estimateurs par une simulation et j'ai calculé les tailles d'échantillon afin de contrôler les biais de la moyenne et de l'erreur-type de la population. De plus, j'ai utilisé les résultats pour évaluer quand l'échantillonnage double est rentable et comment élaborer un suivi qui rencontre les besoins de précision. Ce processus de conception est illustré avec le Spring Eastern Waterfowl Survey, qui montre que l'échantillonnage double n'est pas toujours approprié.

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Many field techniques for counting birds to monitor populations detect only a portion of the individuals present. This can lead to biased results (Rosenstock et al. 2002, Sauer and Link 2004), and a great deal of effort has been devoted to developing methods that can produce reliable results (Thompson 2002b). One approach to improving reliability is to estimate the probability of detecting all birds during a count and then adjust the basic counts to give an estimate of the true population. Double sampling is one such method, which has been used extensively for estimating waterfowl populations (Smith 1995) and is being advocated as a useful approach for other groups of species (Williams et al. 2002, Bart and Earnst 2002, W. L. Thompson 2002).

Double sampling uses two different techniques for counting individuals: one a primary or rapid count, which detects only a portion of the individuals present, and the other an essentially complete count of the population at a subset of the rapid count sites (intensive or secondary count). If there is a strong correlation between counts made using the complete and rapid methods, combining the data from both surveys can provide a more precise population estimate for the same cost than one based solely on complete counts (Thompson 2002a). Although the technique can potentially be applied to a wide variety of situations, the theory is based on several assumptions. In particular, sample sizes are assumed to be large (Cochran 1977), and in some wildlife surveys it may not be feasible to reach the required levels. The purpose of this paper is to present guidelines on the conditions that must be met for double sampling to provide reliable population estimates for avian population studies.

Analyses of double-sampling data most commonly use a ratio estimator to adjust rapid counts based on data collected in intense counts. The use of a ratio estimator in a double-sampling context is often justified by assuming the model $E(y_i) = \beta x_i$ and $\mathrm{Var}(y_i) = \sigma^2 x_i$ (Royall and Cumberland 1981), where x_i is the number of birds detected by the rapid count and y_i is the number actually present. Under this model, the ratio estimator makes efficient use of the auxiliary counts.

An alternative approach to analyzing data collected through double sampling is to use a regression estimator (Cochran 1977, S. K. Thompson 2002). This approach assumes that the regression line for the complete count against the rapid count does not necessarily pass through origin. Such situations are highly likely when both the number of individuals per site and detection probabilities are small.

Both the ratio and regression estimates are intrinsically biased, because they are based on models that are never exactly true for wildlife populations. For large sample sizes, this bias is negligible (Cochran 1977). However, it is important to know the sample size required to justify using these estimators, and to understand the potential magnitude of bias when designing and interpreting the results of a survey whose sample size is limited by logistics and expense. Some examination of these issues has already been done. For example, Cochran (1977) developed guidelines on the standard error (SE) of the ratio estimator, recommending that the number of secondary sites be >30, and that the coefficient of variation (CV) for both the average rapid count and the average complete count be <0.1. In an assessment of the U.S. Fish and Wildlife Service (USFWS) Waterfowl Breeding Survey, Smith (1995) acknowledged Cochran's (1977) guidelines but was unable to address the implications of failing to meet them. The survey on which he was working used plots of unequal size, which further complicated the analysis. Instead, Smith (1995) set an arbitrary criterion that ≥40 individuals should be recorded in the rapid counts across the set of secondary sample sites before data are analyzed.

Eberhardt and Simmons (1987) conducted simulations of double-sampling analysis, using a ratio estimator and population parameters relevant to wildlife surveys (average population count of five individuals per site; detection probability of 0.5; sample sizes of 5, 10, and 20 secondary count sites). They concluded that under these conditions, the estimated mean number of birds per site has a bias <5%, and that the 95% confidence intervals (CI) are approximately correct. However, they cautioned that their results should not be applied to situations that they had not examined, and that further simulation was needed to address other circumstances. Moreover, the formula they used for optimum allocation of counts (equation 12, below) is appropriate for a regression estimation but not for ratio estimation, so their advice on allocation of survey effort between primary and secondary samples requires further examination.

I present simulations that examine a broader range of detection probabilities and average number of individuals per site, which are then used to give much more detailed guidance on the conditions under which double sampling is a good option, in terms of both statistical precision and cost.

Methods

Throughout the present study, I will use the following notation. Assume that the target area is conceptually divided into N potential sampling sites. A simple random sample of n' sites (primary sample) is surveyed using the rapid method, and a subsample of n sites (secondary sample) is surveyed using the complete count method. Let $\bar{x}$ and $\bar{y}$ denote the average count per site of individuals using the rapid and compete count on the secondary sample, and $\bar{x}'$ denote the average rapid count over the entire primary sample. A ratio estimate, $\hat{y}_R$, of the population density (average count per site) is defined as

$$\hat{y}_R = \frac{\bar{y}}{\bar{x}}\bar{x}' = \frac{\bar{x}'}{\hat{p}} = r\bar{x} \quad (1)$$

where $\hat{p} = \bar{x}/\bar{y}$ is the observed detection probability and $r = \bar{y}/\bar{x}$ is sometimes referred to as the "visibility correction factor" (Smith 1995).

Equation (1) estimates the average count per site, and this can be converted to an estimate of the total population by multiplying by N. The average per site and total population thus differ simply by a scaling factor, and statistical properties of the estimates are equivalent.

The variance of $\hat{y}_R$ is estimated by

$$v(\hat{y}_R) = \left(\frac{1}{n'} - \frac{1}{N}\right)s_y^2 + \left(\frac{1}{n} - \frac{1}{n'}\right)\left(s_y^2 - 2rs_{xy} + r^2 s_x^2\right) \quad (2)$$

where s_x^2, s_y^2, and s_{xy} and are the standard formulae for the variances of x of y and the covariance of x and y from the secondary sample. The term $1/N$ in Equation (2) can usually be ignored, because the proportion of the population covered by a survey is generally small. The corresponding standard error (SE) estimate is

$$\text{SE}(\hat{y}_R) = \sqrt{v(\hat{y}_R)}$$

Using a regression estimator, the average count per site, $\hat{y}_L$, is estimated as

$$\hat{y}_L = \bar{y} + b(\bar{x}' - \bar{x}) \quad (3)$$

where b is the slope of a linear regression line:

$$b = \sum_{i=1}^{n} y_i(x_i - \bar{x}) / \sum_{i=1}^{n}(x_i - \bar{x})^2 \quad (4)$$

The variance of $\hat{y}_L$ is estimated by (Cochran 1977):

$$v(\hat{y}_L) = s_{y.x}^2\left(\frac{1}{n} + \frac{(\bar{x}' - \bar{x})^2}{\sum(x_i - \bar{x})^2}\right) + \frac{s_y^2 - s_{y.x}^2}{n'} \quad (5)$$

where s_y^2 is the standard formulae for the variance of y while

$$s_{y.x}^2 = \left[\sum(y_i - \bar{y})^2 - b^2\sum(x_i - \bar{x})^2\right]/(n-2) \quad (6)$$

and the corresponding SE estimate is

$$\text{SE}(\hat{y}_L) = \sqrt{v(\hat{y}_L)}$$

I examined the properties of the ratio and regression estimators through a simulation study. The numbers of individuals present and recorded on each site were randomly generated, and the two estimators and their variances were then calculated from the sample. The simulations were programmed in C++ using random-number-generation techniques described in Bratley et al. (1987).

The number of individuals present on a site was simulated using a zero-inflated negative binomial distribution. The negative binomial distribution has been found useful for describing wildlife populations (Seber 1982, Williams et al. 2002), and zero inflation was incorporated to model general population surveys where, for a specific species, the sample selection may include some individual survey sites where the species is not present, either because the site is outside the species' range or because it does not have the correct habitat. This model assumes that the survey area comprises two sectors: one where the birds are never seen and one where they may be found. On the sites where they may be found, the number of birds present has negative binomial distribution. Thus, the distribution is described by three parameters: the overall mean number of individuals actually present per site (μ_y), a negative binomial shape parameter (r), and the proportion of sites with no birds present (no-bird sites; F_0). The shape parameter (negative binomial parameter x in Evans et al. [2000]) describes the clustering of the population across sites, with small values indicating a more clumped distribution.

I generated the number of individuals recorded at a site as a two-step process. Individual birds were assumed to have different detection probabilities, which were described by a beta distribution with mean μ_t and coefficient of variation CV_t. For each individual present on the site, a random detection probability was generated, and then a Bernoulli trial with the selected probability of detection was used to generate whether the individual was actually recorded.

Table 1 shows the values selected for the simulations. For each set of simulations, one parameter was varied over the range shown in Table 1, whereas all other parameter values were held constant at the base value. The goal was to identify which parameters have a substantial influence on the precision and bias of the estimators, so base values were selected that were expected to produce bias but that might actually be found in bird surveys; i.e., the average number of birds on a plot was small (3.0) and the detection probability was low (0.2). Simulations were run for each set of conditions using primary (rapid) counts at 100 sites and secondary (complete) counts at 10, 20, and 40 sites. Each simulation was run 50,000 times.

Eberhardt and Simmons (1987) conducted simulations using a gamma distribution for the number of individuals at a site, which is the continuous analogue of the negative binomial distribution. The number of individuals at a site was then generated as a fraction of the true number of individuals at a site plus a random error, and they also added a random error to the number of individuals seen using the precise count. This allowed them to consider the situation in which the putative complete count was imprecise. By contrast, the simulations used here are based on discrete distributions, so that all reported counts of individuals are integers. This was considered to provide a better model for the distribution properties of the estimator when applied to count data, particularly when the counts were low because of a small number of individuals per site or a low detection probability. Investigating the consequences of errors in the complete counts was not attempted, because it would have been difficult to develop a suitable model and it would have added considerably to the complexity and length of the study.

Results

Simulations using the ratio estimator.—The influence of the model parameters on relative bias is shown in Figure 1. Results are shown for three secondary sample sizes: 10, 20, and 40. Bias was smaller for populations with a high density of individuals per site (Fig. 1A) or where the rapid count had a high detection probability (Fig. 1B). The negative binomial shape parameter (Fig. 1C), the variability of the detection probabilities (Fig. 1D), and the proportion of sites where the birds are not present (Fig. 1E) had little influence on the relative bias.

Figure 1A and B suggest that the relative bias could be controlled by ensuring that sufficiently large numbers of individuals are detected. Figure 2 illustrates the effect of increasing the total number of birds recorded during rapid counts across the entire set of secondary count sites, whether by increasing detection probability (Fig. 2A) or by increasing the number of secondary sites (Fig. 2B). Above an expected count of 20–25 individuals, detection probability has very little influence on relative bias; below this number, bias increased with declining detection probability. Table 2 gives the recommended minimum

Table 1. Parameter values used in simulations.

Parameter	Symbol	Base	Range[a]
Average individuals present per site	μ_y	3.000	2.0–50.0
Negative binomial shape parameter	r	2.000	0.5–10.0
Proportion of no-bird sites	F_0	0.000	0.0–0.8
Average detection probability	μ_t	0.200	0.1–0.9
Coefficient of variation of detection probability	C_t	1.333	0.1–2.0

[a] Range of different values examined with other parameters set at base value.

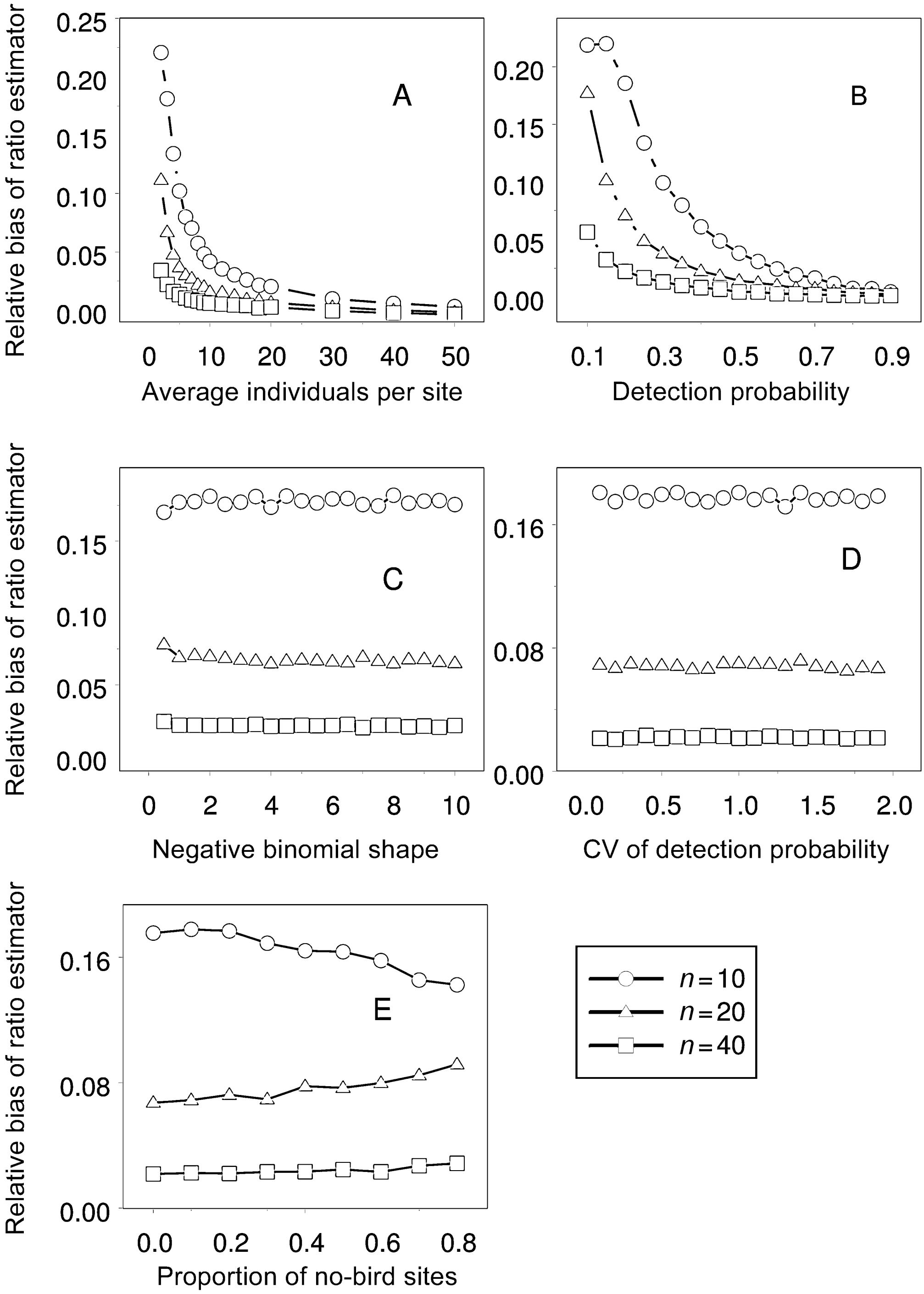

Fig. 1. Relative bias of population estimates (birds sample^{-1}) calculated from the ratio estimator when model parameters are varied.

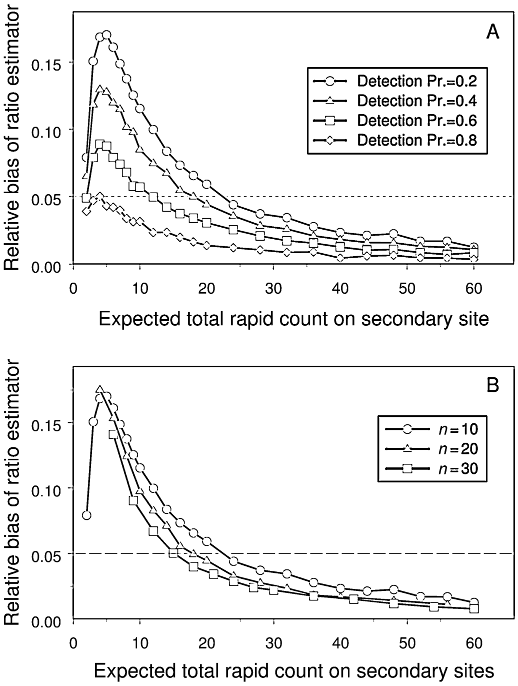

Fig. 2. Relative bias of ratio estimations when the proportion of no-bird sites is 0.2, the coefficient of variation (CV) of the detection probability is set to a large value (CV = 1.333 for μ_t = 0.2, CV = 0.75 for μ_t = 0.4, CV = 0.500 for μ_t = 0.6, and CV = 0.333 for μ_t = 0.8), and the number of primary samples is 100. (A) Effect of varying detection probabilities when number of secondary sites is 10. (B) Effect of secondary sample size when average detection probability is held at 0.2.

number of individuals to be recorded on the rapid counts at the secondary sites if selected levels of relative bias are to be met, given a range of detection probabilities. Table 2 was developed assuming a secondary sample size of 10, and the recommended minima will be conservative if the secondary sample size is increased.

The simulations indicated that the calculated SE of the ratio estimator (equation 2) tends to be an underestimate (Fig. 3). Figure 3 shows the relative bias in the SE of the ratio estimator plotted against CV($\bar{x}$), to which it is logically related. The bias in the SE of the ratio estimator is generally <5% when CV($\bar{x}$) < 0.15; if the detection probability is high (0.8), the bias is <5% when CV($\bar{x}$) < 0.20.

Table 2. Minimum total number of individuals that should be detected on rapid counts across all secondary sample sites, given various detection probabilities, if the relative bias using the ratio estimator is to be kept below target levels.

Detection probability	Target levels of relative bias 0.05	0.02	0.01
0.2	24	48	>60
0.3	20	44	>60
0.4	18	36	60
0.5	16	32	56
0.6	12	28	48
0.7	<10	24	44
0.8	<10	16	28

The SE is often used to calculate a 95% CI for the estimate, using the equation $\hat{y}_R \pm 1.96\text{SE}(\hat{y}_R)$, based on the assumption that the sample size is large enough so that the estimator has an approximately normal distribution. Figure 4 shows the proportion of simulations in which the nominal 95% CI actually included the true population mean (coverage). If CV($\bar{x}$) < 0.15, the CI had an actual coverage >0.91; if CV($\bar{x}$) < 0.1, the CI had a coverage >0.93.

Simulations using the regression estimator.—Concurrently with the simulations for the ratio estimator, I tabulated comparable results for the regression estimator. Bias using the regression estimator is negligible under most conditions (Fig. 5). However, bias was substantial when there was a high proportion of sites at which no birds were present (no-bird sites; Fig. 5E). Some simulations were unable to produce an estimate, and this was most likely to occur with parameters similar to those that produced estimates with large bias. Even with a high proportion of no-bird sites, however, bias was reasonably small when the number of secondary sites was ≥20 (Fig. 5E).

Similar to the results for the ratio estimator, the simulations indicated that the calculated SE of the regression estimate (equation 5) tends to be an underestimate (Fig. 6), though to a lesser degree. When CV($\bar{x}$) < 0.2, bias was <0.05, independent of the detection rate or the proportion of secondary sites. Further, if CV($\bar{x}$) < 0.1, bias was <2%. The coverage of a nominal 95% CI was similar to that for the ratio estimator.

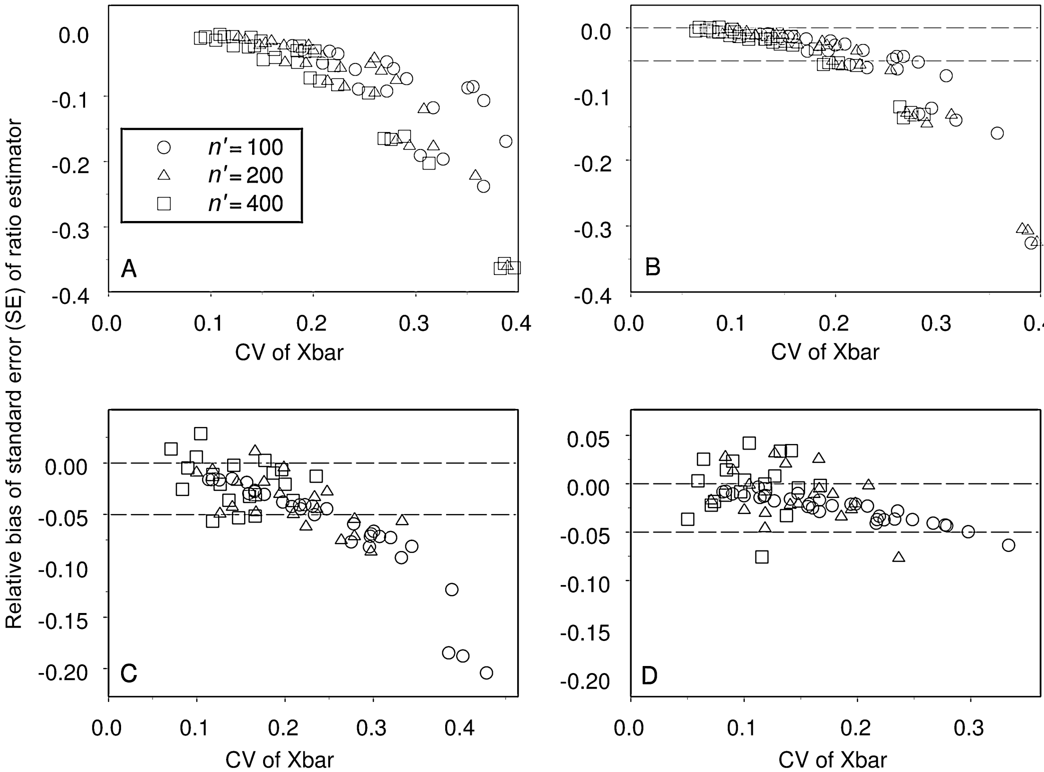

Fig. 3. Relative bias of the SE of the ratio estimator related to the CV($\bar{x}$). (A) Detection rate (t) = 0.2 and n/n' = 0.2, (B) t = 0.2 and n/n' = 0.4, (C) t = 0.8 and n/n' = 0.2, and (D) t = 0.8 and n/n' = 0.4. The number of secondary sample points is n'= 100 (circles), 200 (triangles), and 400 (squares). In all panels, there are 32 points for each secondary sample size that examine a combination of the two negative binomial parameters: eight values for the mean (0.5, 1, 2, 4, 8, 16, 32, or 64) times four values for the shape parameter (0.5, 1, 2, or 4). The coefficient of variation (CV) of the detection probability is set to a large value (CV = 1.333 when the detection rate is 0.2 and CV = 0.333 when the detection rate is 0.8).

Application to design of double-sampling surveys.—An efficient double-sampling survey design balances the effort spent on the primary and secondary samples, taking into account cost and survey precision. Assume that the total survey cost can be written as

$$T = cn + c'n' \tag{7}$$

where c and c' are the costs of surveying one site using the exact and rapid techniques, respectively. The costs should include the average cost of traveling from one site to another, which can be substantial in wildlife surveys covering large or remote areas. However, it may be difficult to estimate costs that are appropriate for a wide range of samples sizes, because ferrying distances will decrease as the number of sites is increased. One approach may be to use initial estimates of cost per site to develop a tentative sample size and then reassess whether the assumptions used to develop the preliminary costs are appropriate and revise and reallocate survey effort if necessary.

Cost is not the only consideration, of course, and it should be determined whether the planned sample design will include enough sites and bird detections to justify use of either double-sampling estimator. I will assume that data are available from a pilot or similar survey to estimate the variances for the each of the design–analysis schemes. This is a necessary prerequisite to designing a survey that will meet precision requirements and efficiently use the resources allocated to field work.

The variance for the ratio estimator (Equation 2) can be recast as:

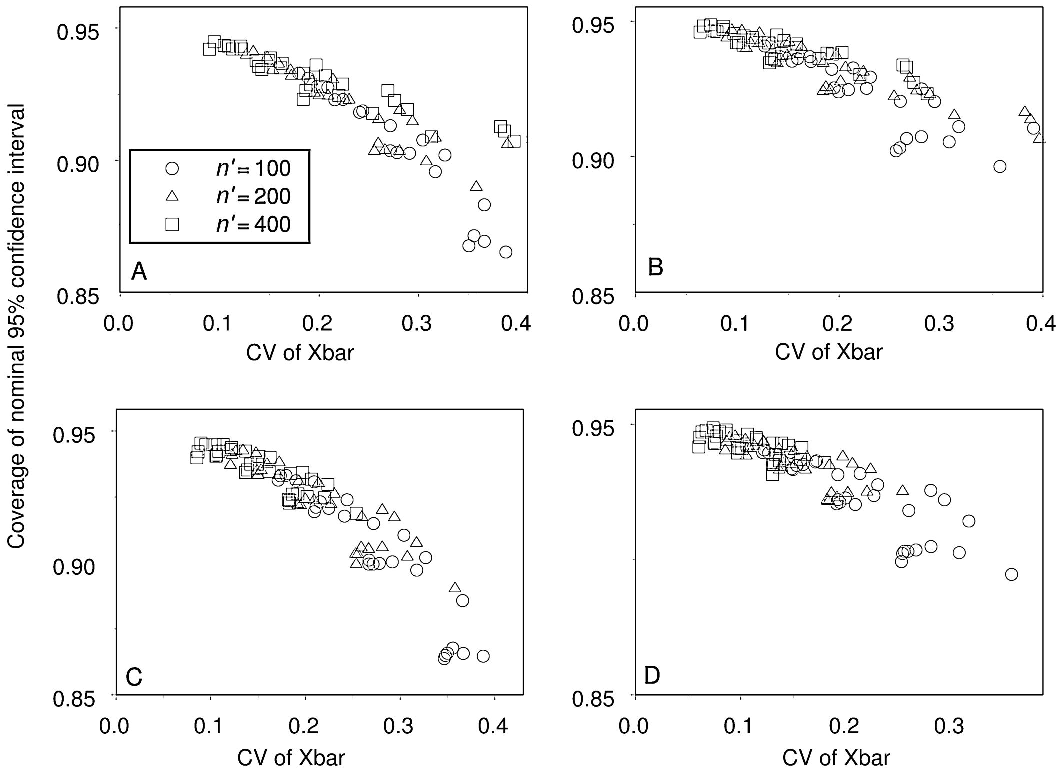

Fig. 4. Proportion of simulations in which estimated 95% confidence interval for the ratio estimation includes the true population mean (coverage). Figure uses the same panels and symbols described in Figure 3. (A) Detection rate (t) = 0.2 and n/n' = 0.2, (B) t = 0.2 and n/n' = 0.4, (C) t = 0.8 and n/n' = 0.2, and (D) t = 0.8 and n/n' = 0.4.

$$v(\hat{y}_R) = \left(\frac{1}{n} - \frac{1}{N}\right)s_y^2 + \left(\frac{1}{n} - \frac{1}{n'}\right)\left(r^2 s_x^2 - 2rs_{xy}\right) \tag{8}$$

The first term is the variance of the simple average of the exact count from the secondary sample, and the second term gives the variance reduction that results from including the rapid count data. Hence, for a ratio estimator to improve the precision of the estimate, the second term should be negative. This leads to the condition

$$\rho_{xy} > \frac{rs_x}{2s_y} \tag{9}$$

where $\rho_{xy} = s_{xy}/s_x s_y$ is the correlation between the rapid and exact counts.

The first step in deciding whether to employ a double-sampling scheme using a ratio estimator should be to determine whether equation (9) is valid. If it is, there is a potential for double sampling and ratio estimation to improve survey efficiency. Otherwise, the rapid count is of such poor quality that incorporating it degrades the precision of the estimate. This would be true even if a complete census of the population using the rapid count were available at no cost.

The large-sample optimum balance between the primary and secondary samples using the ratio estimator can be derived as

$$f_R = \frac{n}{n'} = \sqrt{\frac{(s_y^2 - 2rs_{xy} + r^2 s_x^2)c'}{(2rs_{xy} - r^2 s_x^2)c}} \tag{10}$$

For the ratio estimator, given a total cost available for sampling (T), one can combine Equations (10) and (7) to obtain a preliminary estimate of the optimal number of primary and secondary sample sites. The preliminary allocation can then be used to calculate the total

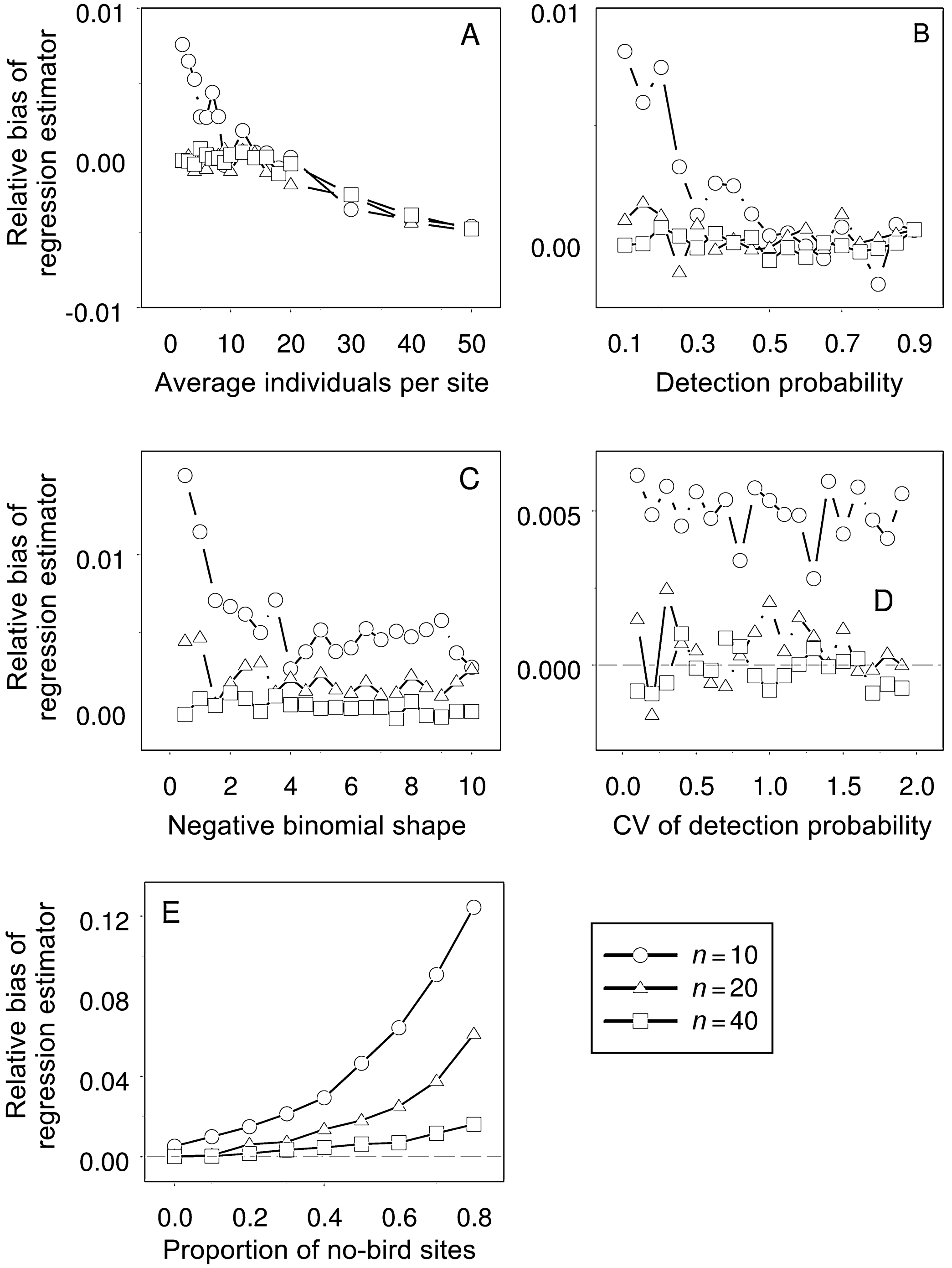

Fig. 5. Relative bias of population estimates (birds sample^{-1}) calculated from the regression estimator when model parameters are varied.

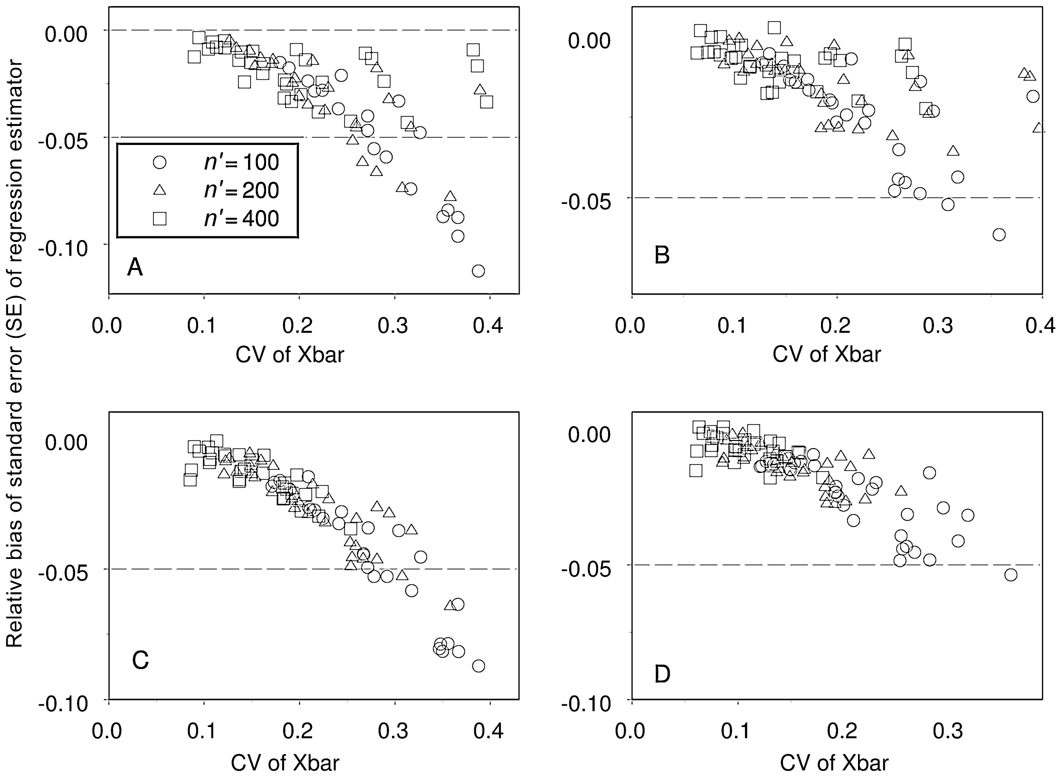

FIG. 6. Relative bias of the standard error (SE) of regression estimations related to the CV($\bar{x}$). Figure uses the same panels and symbols described in Figure 3. (A) Detection rate (t) = 0.2 and n/n' = 0.2, (B) t = 0.2 and n/n' = 0.4, (C) t = 0.8 and n/n' = 0.2, and (D) t = 0.8 and n/n' = 0.4.

number of individuals that would be expected in that number of rapid counts, and the resulting variance (2). This expected number of individuals counted in the rapid survey can be compared with Table 2 to see whether the resulting survey would produce results with an acceptable level of bias, and the variance of the estimator should be calculated to determine whether the design provides estimates with adequate precision. If the design is inadequate, more resources are required to increase sample sizes.

For the regression estimator, there is no equivalent to equation (9). However, the regression estimator equation (5) describes how to estimate the variance conditional on a set of observed rapid counts. At the survey-design stage, one must work with the expected value of equation (5) when a random sample has been selected. This expected value is difficult to derive mathematically but can be written in the form

$$v(\hat{y}_L) = s_y^2(1-\rho^2)\left[\frac{1}{n}+\left(\frac{1}{n}-\frac{1}{n'}\right)\frac{1}{h(n)}\right]+\frac{\rho^2 s_y^2}{n'} \quad (11)$$

where $h(n)$ is a general function that depends on the data distribution. For the normal distribution, it is known that $h(n) = n - 3$ (Cochran 1977), but the expected value has not been solved for other distributions. However, the rapid counts from typical population monitoring studies will be small-integer values, and it is not reasonable to assume that they have a normal distribution. To provide some useful guidelines for the design of the double sampling using the regression approach, I ran a simulation study to describe $h(n)$, assuming that the rapid counts had a negative binomial distribution. The negative binomial distribution was chosen because it has been useful for describing wildlife populations (Seber 1982, Williams et al. 2002).

Random samples were generated for different numbers of secondary sites, and the value of g (which is the portion of equation 5 that is held fixed through conditioning) was calculated as

$$g = \frac{(\bar{x}' - \bar{x})^2}{\sum (x_i - \bar{x})^2}$$

The expected value of g, $E(g)$, was then estimated as the average of 50,000 simulations. For each simulated expected value $E(g)$, $h(n)$ was calculated as

$$h(n) = \left(\frac{1}{n} - \frac{1}{n'} \right) \frac{1}{E(g)}$$

Results were plotted against number of secondary sample sites (n) for different values of the population means and CV (Fig. 7). The relationship of $h(n)$ with sample size varied with the CV of the distribution. For CV < 2, $h(n)$ was linear with sample size and was virtually identical for all population means (in Fig. 7A, population means 2 and 30 were coincident). For CV of 3.0 or 4.0 (Fig. 7E, F), the relationship varied with the population mean. If the average rapid count per site was large (>30), $h(n)$ varied linearly with the number of secondary sample sites; if the average rapid count was <30, $h(n)$ was curvilinear with the number of secondary sample sites. Slopes and intercepts were estimated for the lines and are presented in Table 3. For CV < 2.0, the linear form for $h(n)$ is a reasonably good working relationship for survey design. For CV > 3.0, the linear form presented in Table 3 will underestimate the true variance for populations with a small average number of birds detected per primary site.

If the number of secondary sample sites is large, the optimum balance between primary and secondary samples for the regression estimator can be derived as

$$f_L = \frac{n}{n'} = \sqrt{\frac{(1-\rho^2)c'}{\rho^2 c}} \qquad (12)$$

For survey designs using the regression estimator and a total survey cost (T), one can combine Equations (7) and (12) to provide a preliminary estimate of the optimum number of primary and secondary sample sites. Using the pilot estimate of the CV, one can look up the value of $h(n)$ in Table 3 and determine whether the number of secondary sites is adequate to meet the minima given there. The resulting expected variance for the preliminary design can then be calculated through substituting $h(n)$ into equation (11). If the survey fails to meet the desired precision requirements, the budget could be increased to allow a larger sample to be collected.

For comparison purposes, a single-stage sample using complete counts with sample size of $n = T/c$ can be selected for the same budget, and the resulting variance for the sample mean would be

$$V(\bar{y}) = \left(\frac{1}{n} - \frac{1}{N} \right) s_y^2 \qquad (13)$$

This variance can be compared with that for a double-sample ratio and regression estimators using the appropriate allocation of survey effort for each estimator as described above.

Discussion

Cochran's (1977) guidelines for using the variance estimator for the ratio estimator (>30 secondary sites and CV for both the average rapid count and the average complete count < 0.1) are conservative compared with the results from the simulations done here. My results indicated that higher CV had little effect on bias (Fig. 1D). As long as the average rapid count is ≥1 per site across the sample of secondary sample points, 30 secondary sites will produce population estimates with low bias even when detection probability is low (Fig. 2A). Smith's (1995) target of detecting ≥40 individuals on rapid counts across the set of secondary count sites is conservative (Table 2) unless both detection probabilities and required bias are low. My simulations confirm the conclusions of Eberhardt and Simmons (1987), which were based on a smaller set of scenarios.

The simulations addressed only the situation in which the complete count is accurate, and rapid counts report some fraction of the actual birds present. Further work is needed for other scenarios—for example, where the putative complete count is error prone or the rapid count includes individuals from outside the survey plot. Until such work has been done, following the original guidelines proposed by Cochran (1977) should help ensure that bias in the estimated SE is low.

The regression estimator has been less utilized than the ratio estimator, possibly because

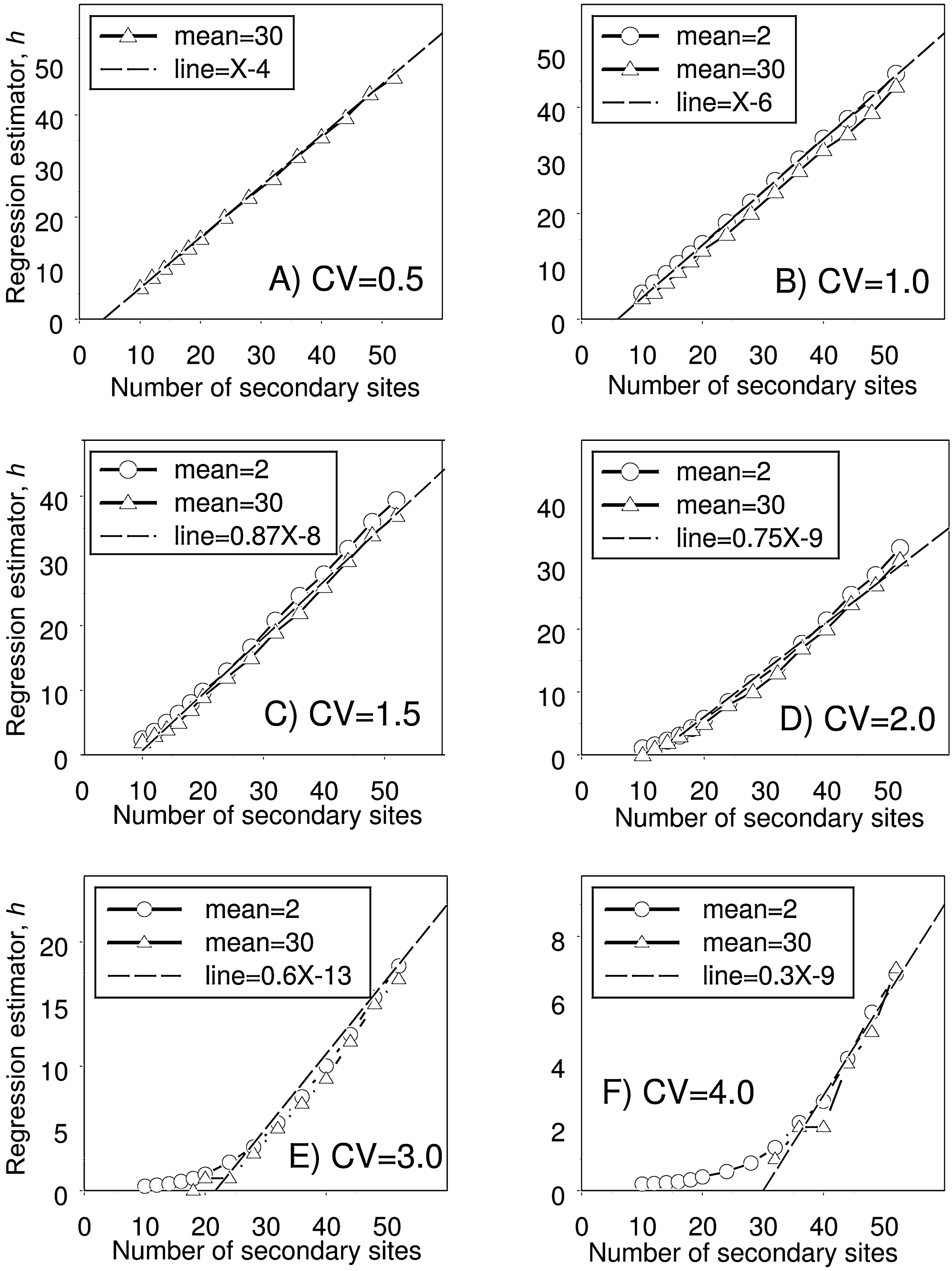

FIG. 7. Simulated expected value of regression estimator variance term, h, plotted against number of secondary sample sites for different coefficients of variation of the rapid counts.

Table 3. Equation for $h(n)$ term in expected variance for regression estimator given the coefficient of variation (CV) of the rapid counts and minimum number of secondary sites for applicability of the equation.

CV	$h(n)$	Minimum number of secondary sites
0.5	$n - 4$	10
1.0	$n - 6$	10
1.5	$0.87n - 8$	10
2.0	$0.75n - 9$	12
3.0	$0.60n - 13$	24
4.0	$0.30n - 9$	30

its justification is less intuitive, and the expected value of the variance estimator is complicated to derive. The ratio estimator will be efficient when the intercept for a regression of exact counts on rapid counts has a zero intercept, but this is unlikely when both the average count per site and the detection rate are small. In such a situation, the regression estimator should be given serious consideration whenever a double-sampling monitoring program is being developed.

The criteria of Cochran (1977) and Smith (1995) provide guidelines to ensure that bias in the SE for the ratio estimator will be low; my results show that they are overly restrictive. The CV of rapid counts had little effect on bias of population estimates, but $CV(\bar{x}) > 0.15$ can result in a variance estimator that substantially underestimates the true variance. This is a broad guideline, however, and if the $CV(\bar{x}) > 0.15$, the design is not invalidated but should be evaluated more thoroughly to ensure that the variance estimate is appropriate. A good way to illustrate how the steps outlined here can be used to evaluate survey options and determine optimal design is to work through an actual survey.

The Spring Eastern Waterfowl Survey (USFWS 2003) consists of a rapid count made from an airplane traveling at high speed along transect lines, whereas the complete count is done from a helicopter at a much lower velocity. Waterfowl counts from an airplane are known to undercount waterfowl, particularly in the boreal forest in Eastern Canada; the helicopter provides a much more complete count (as evidenced by the higher count per square kilometer). However, airplanes can cover much larger areas for the same cost as helicopters. Detection probabilities vary among crews, and analysis is done separately by crew area. In the 2003 field season, both techniques were used on one hundred 29-km transect segments in crew area 1. For the American Black Duck (*Anas rubripes*), the average counts from the airplane and helicopter were 0.76 and 3.30, respectively, with standard deviations of 0.77 and 6.56. Thus, the ratio of helicopter to airplane counts is 4.34, and equation (9) indicates that a correlation of 0.25 would be required for double sampling to provide increased precision. The actual observed correlation was 0.16, which is substantially below the required level. Because the observed correlation fails to meet the condition in equation (9), the optimum survey allocation f_R cannot be calculated and f_R was set equal to 0.62 (the sample allocation for the regression estimator) so that the precision of the ratio estimate could be calculated for purposes of comparison. The results are shown in Table 4. Comparing the expected total rapid count ($E[n\bar{x}]$) with Table 2 (detection probability 0.3) indicates that the relative bias in the ratio estimator would just meet a target of 0.05 at a cost of \$20,000. However, it should also be noted that even when survey expenditures exceed \$100,000, $CV(\bar{x}) > 0.15$ and, hence, the SE of $\hat{y}_R$ could be underestimated (Fig. 3). As would be predicted by failure to meet the condition in equation (9), the ratio estimator is substantially less precise than the single sample estimator for all sample sizes. For this project, simply estimating the population from random helicopter-transect data would provide more precise estimates than can be obtained using double sampling and ratio estimation

The regression estimator performs much better for this survey. The data indicate $CV(x) = 0.77/0.76 = 1.0$, and the approximate value of $h(n)$ for the regression estimator for this CV is $n - 6$ (Table 3). For the 2003 field season, costs for flying a single segment were tabulated as \$61 and \$1,135 per segment for the fixed-wing and helicopter surveys, respectively, and the optimum allocation of survey effort using equation (12) is $f_L = 0.62$. The precision that would result from running surveys at selected total costs (from equation 11 with $h(n) = n - 6$) is shown

Table 4. Precision obtained for various total costs for example 1 (Black Duck, Eastern Spring Waterfowl Survey).

Total cost	Single sample		Double-sample ratio estimator[a]					Double-sample regression estimator[a]			
($000)	n	$CV(\bar{y})$	n	n'	$E(n\bar{x})$	$CV(\bar{x})$	$CV(\hat{y}_R)$	n	n'	$CV(\bar{x})$	$CV(\hat{y}_L)$
20	17	0.211	16	26	16	0.40	0.32	16	26	0.40	0.220
40	35	0.147	32	52	31	0.28	0.23	32	52	0.28	0.152
60	52	0.121	48	78	48	0.23	0.19	48	78	0.23	0.124
80	70	0.104	64	104	64	0.20	0.16	64	104	0.20	0.107
100	88	0.093	80	130	80	0.18	0.14	80	130	0.18	0.095

[a] $f_R = f_L = 0.62$.

in Table 4. As was noted for the ratio estimator, even if survey expenditures exceed $100,000, $CV(\bar{x}) > 0.15$ and, hence, the SE of $\hat{y}_R$, could be underestimated (Fig. 6). The precision of the double-sampling regression estimator is almost identical to the single-sample average.

The actual survey design involves rapid counts in an airplane done along transects (which can be considered aggregates of 29-km segments), and these transects have variable length, which further complicates the data analysis. The optimal design with $f_L = 0.62$ would be impractical for such a survey, because the operating procedures presume that several long transects will be run. If the airplane is used only on short transects, the cost of flying an individual segment may be different. The implemented survey is further complicated by pooling the secondary sample for several years, which requires different variance estimators.

Wildlife surveys are often used to monitor many species simultaneously. The best design and allocation of survey effort will likely be different for each species, requiring a compromise in allocation of survey effort between primary and secondary sites. One solution is to identify one species of primary importance and design the survey for that species, simply accepting whatever precision results for other species. Another approach is to identify a group of important species, derive the optimum allocation separately for each of the identified species, apply each optimal allocation to all other species, and select the design that minimizes the maximum CV across all species.

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