The effect of line-transect placement in a coastal distance sampling survey

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ABSTRACT

Distance sampling surveys are commonly used to estimate animal abundance (*N*). The choice of survey design has only recently received attention in the line-transect research literature, which has tended to focus more on the violation of assumptions. In this study, simulation methods were used to assess the effect of line placement on the accuracy and precision of a line-transect survey for estimating dolphin abundance. In 1998, a vessel-based line-transect survey of Hector's dolphin (*Cephalorhynchus hectori*) was carried out around Banks Peninsula (New Zealand). These data were used to generate a spatially realistic dolphin distribution onto which different survey designs were overlaid. Eight types of design were considered, consisting of four types of stratification and two methods for allocating lines: random or systematic. None of the designs showed any evidence of significant bias in the estimate of $N(\hat{N})$. Systematic placement of lines generally provided more precise estimates of *N*, with an average reduction (over all designs) of 14% in the coefficient of variation of $\hat{N}$, $[CV(\hat{N})]$. These results correspond well with those expected from classical sampling theory for the case of estimating a population mean. However, these designs also overestimated $CV(\hat{N})$ by 10-28% (mean=22%). Systematic line-placement has several practical advantages over random placement, including more uniform spatial coverage. For coastal cetacean populations with spatial dynamics similar to the one considered here, we recommend the use of systematic line-placement, with the proviso that the estimate of $CV(\hat{N})$ is likely to be overestimated by 10-28%.

KEYWORDS: ABUNDANCE ESTIMATE, DISTRIBUTION, SURVEY-VESSEL, SURVEY DESIGN, MODELLING

INTRODUCTION

For ecologists, the question of 'how many are there?' is one of the most fundamental and for managers who need information on impacts, or an assessment of efficacy of intervention, it is one of the most crucial. It has always been a difficult question to answer with precision. In cetacean research, abundance information is generally gained via mark-recapture analyses of data from resightings of tags or natural markings, or from sighting surveys, of which line-transect methods are the most important. Line-transect sampling belongs to a more general class of methods called distance sampling (Buckland *et al.*, 2001). In the work presented here, the focus is on the design of a line-transect survey, particularly the effect of line placement.

Line-transect surveys for cetaceans rely on the following critical assumptions (Buckland *et al.*, 2001):

(1) the probability of detection on the trackline equals one (i.e. $g(0) = 1$), or at least known;
(2) animals are detected prior to responsive movement;
(3) measurements are recorded accurately, with no observer bias;
(4) line-transects are located randomly with respect to the distribution of the animals;
(5) detections are independent events.

Violations of these assumptions will result in biased estimates of density and abundance (Hiby and Hammond, 1989; Buckland *et al.*, 2001). As a result, many line-transect studies have focussed on the development of methods to allow for such violations (e.g. Barlow, 1999; Schweder, 1999). Until recently there has been less guidance available for designing robust, cost-effective surveys. Hiby and Hammond (1989) recommend using a saw-tooth (zig-zag) survey design to achieve uniform coverage probability and for efficiency, and this design has subsequently been used in several surveys (e.g. Miyashita, 1993; Forcada *et al.*, 1994; 1995; Forcada and Hammond, 1998).

In fact, zig-zag sampling does not provide uniform coverage probability in many circumstances (Strindberg and Buckland, 2004). A simple zig-zag pattern around a convex coastline, for example, results in a proportionately greater amount of effort inshore, which may bias abundance estimates, particularly if there is an inshore-offshore density gradient. Strindberg and Buckland (2004) provided guidance for both design and analysis of zig-zag surveys to account for uneven coverage probability, however such sampling may still have associated practical problems such as swell and glare, particularly for small-boat surveys (e.g. Dawson *et al.*, 2004).

Buckland *et al.* (2001) made some comments on survey design, noting that there is no compelling reason to use completely random lines, and that systematic designs should often result in greater precision. They also offer advice on how to set out lines, pointing out that parallel lines will provide uniform coverage probability. Buckland *et al.* (2004) provides more detailed discussion of survey design. They introduce many new or recently developed ideas such as integrating geographic information systems (GIS) for automated survey design (Strindberg and Buckland, 2004) and adaptive survey designs (Pollard and Buckland, 2004). However several practical design issues, particularly for small boats, remain unresolved.

In January and February of 1998, a line-transect survey was carried out to estimate Hector's dolphin (*Cephalorhynchus hectori*) abundance between Motunau and Timaru on the east coast of the South Island of New Zealand (Dawson *et al.*, 2000; 2004; and see Fig. 1). Data from this survey were used to investigate the accuracy and precision of different survey designs, focussing on two

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aspects: (1) the effect of stratification, involving both the choice of strata and the effort allocated to each one; and (2) the choice of a systematic or random selection of lines within each stratum.

These two aspects of survey design have been considered at length in classical sampling theory (Cochran, 1977). It is known that the sample mean is unbiased for both random and systematic sampling, and that systematic sampling will often lead to a more precise estimate than random sampling. These results involve 'design-based' inference and do not automatically apply to a line-transect survey, as distance sampling involves a combination of model-based and design-based inference, the former arising as a consequence of estimating the detection function, and the latter being used to estimate density given a detection function (Fewster and Buckland, 2004).

The survey data were used to develop a spatial model of dolphin distribution, which was repeatedly sampled using different survey designs. The spatial model was not intended to characterise the true distribution of Hector's dolphin, and the aim was not to provide an exhaustive assessment of survey design for populations displaying different characteristics. Rather, our objective was to compare the accuracy and precision of the different designs for a realistic 'population'.

METHODS

The idea behind generating a spatial model for the dolphin distribution was to allow any sample of transect lines to be chosen. Each line would have an expected number of sightings, $E(n)$, predicted by the spatial model. The observed number of sightings on each line could then be generated from a Poisson distribution with mean equal to $E(n)$. In this way, the original survey data can generate appropriate perpendicular distances and group sizes for each 'sighting'.

Data simulation and analysis

Data from the 1998 survey formed the basis of the spatial model of dolphin distribution described here.

Buckland *et al.* (2001) recommended sampling across known density gradients. For the 1998 survey, it was known that there were both alongshore and offshore density gradients of Hector's dolphins, therefore lines were plotted at 45° to the shore. Sampling in this manner helps to minimise encounter rate variance, and has practical advantages since it means alternative transects can be plotted, depending on daily swell and glare conditions (Dawson *et al.*, 2004). Designing the simulations in this manner ensures consistency with the survey on which this analysis is based. This design is in contrast to the simulation work of Fewster and Buckland (2004), who generated a hypothetical population with two density gradients, but sampled in a unidirectional manner. In other word, lines were plotted horizontally or vertically and sampled across one gradient only.

To ensure uniform coverage around Banks Peninsula the coast was divided into short sections, plotting lines at 45° to the baseline of each. Transect lines extended to 4 n.miles offshore (Fig. 7). In the 1998 survey, transects were spaced 2 n.miles (New Brighton to Rakaia River) or 4 n.miles apart (New Brighton to Motunau; Rakaia River to Timaru; Dawson *et al.*, 2000).

The first step in simulating the data was to create plots showing the coastline that had been surveyed in 1998, together with contour lines of density based on observed encounter rates (number of groups seen per kilometre of survey trackline, nL^{-1}). This process created continuous density zones, extrapolated from our data, using Surfer Surface Mapping System (Smith *et al.*, 1995). The Minimum Curvature method was chosen, which attempts to generate the smoothest surface while honouring the original data as closely as possible. A digitiser was used to generate base maps onto which contours were overlaid (e.g. Fig. 1). A further overlay was created that contained a theoretical set of possible transect lines (e.g. Fig. 2). Note that the contouring in these figures is different for illustrative purposes only, for the actual analysis all plots used the same contour intervals as in Fig. 2. These lines were separated by a distance equal to our estimated effective strip width from the 1998 survey; hence the complete set of lines provided full sampling coverage of the survey area.

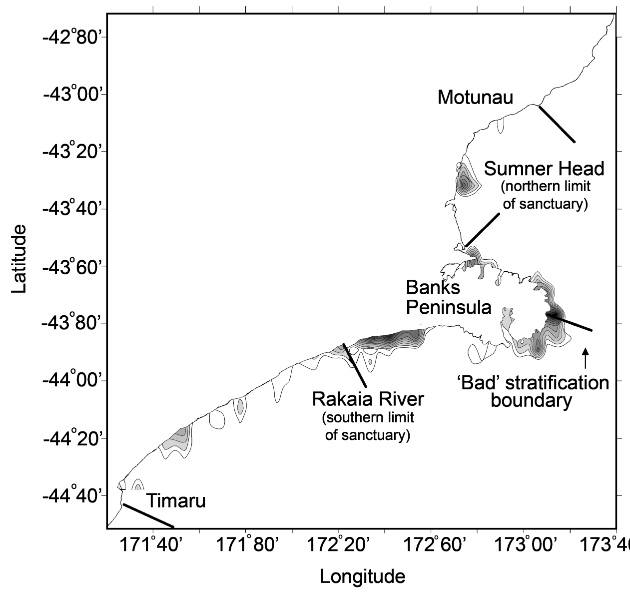

Fig. 1. Survey area with encounter rate (sightings/km trackline) contours. Highest encounter rates are off the eastern side and to the south of the peninsula. Contouring interval is 0.2 (groups seen per kilometre of trackline).

A total of 672 lines were overlayed onto the density contours. For each line, the proportion that fell within each density zone was calculated. This provided a mean encounter rate for the line and was multiplied by the length of the line to obtain an expected number of encounters, $E(n)$. For example, if the mean encounter rate for a transect was nL^{-1} = 0.5 groups km^{-1}, and the length of the transect was 7.5km, then $E(n) = 0.5 \times 7.5 = 3.75$. The rationale was that each line had a unique encounter rate based on where it fell, that could then be converted to $E(n)$. The 'observed' number of encounters was selected at random from a Poisson distribution with mean equal to the value of $E(n)$ for that line (Buckland *et al.*, 2001).

For each encounter, group size, s, was determined by randomly selecting a value from the distribution of group sizes observed in the original survey. Perpendicular distances, x, were generated by replicating the uniform key function model with two cosine adjustments, as this was the model that best fitted data from the 1998 survey. Using one metre increments from 0-594m (our truncation distance from the 1998 survey), this model was used to generate values for $g(x)$, and these were used to randomly select sighting distances.

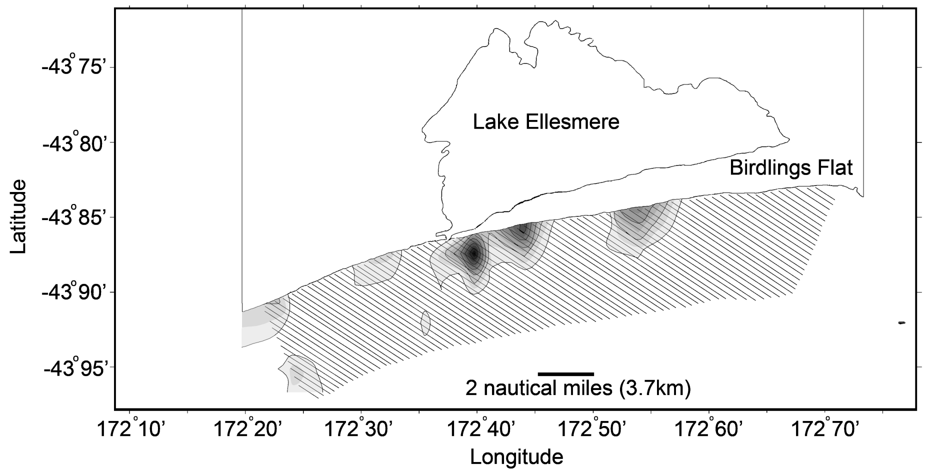

Fig. 2. The sub-region of Birdlings Flat-Rakaia River, encounter rate contours and full set of transect lines for this section. Contouring interval is 0.5 (groups seen per kilometre of trackline).

Data generation therefore resulted in a single spreadsheet containing the following information:

(1) a total of 672 possible lines, each with a sample of 200 possible values for *n*;
(2) 1,000 possible sighting distances and group sizes.

These numbers for *n* and sighting distances were chosen because when plotted on frequency histograms they closely approximated the appropriate distributions.

The simulations were run within Microsoft *Excel*, and followed the steps described in the flow chart in Fig. 3. Data analysis was carried out using the program *Distance 3.5* (Thomas *et al.*, 1998). It is worth noting the two main steps in the simulation process.

Step 1. A 'snapshot' of the population was created by generating a fixed number of sightings on each of the 672 possible lines (together with a group size and distance from the transect line).

Step 2. A survey for each of the eight designs being considered was conducted, by selecting a subset of the lines in Step 1.

There was some choice as to how often Steps 1 and 2 were carried out and in order to cover a wide range of plausible spatial distributions Step 1 was performed 199 times and Step 2 just once. Carrying out Step 1 once and Step 2 many times, would assess the performance of the designs for only one spatial distribution.

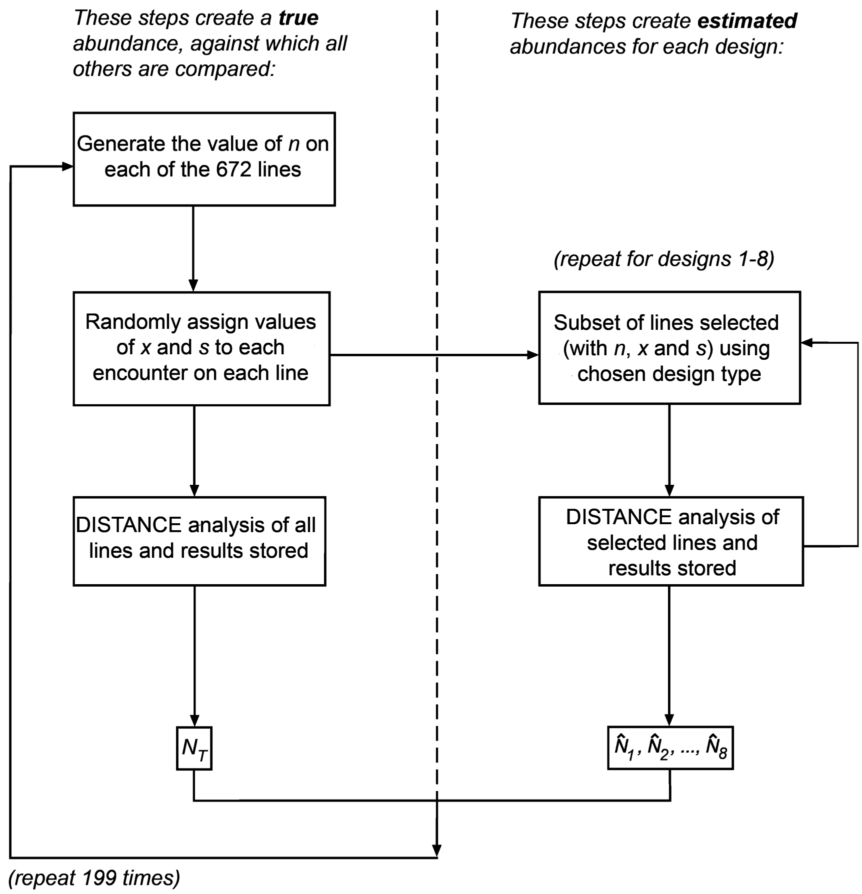

Fig. 3. Flow diagram illustrating the simulation procedure run within *Excel* using the programming language Visual Basic for Applications.

For each iteration, the 'true' abundance (N_T), was calculated from all the lines, and compared with the estimated abundances ($\hat{N}_1$, $\hat{N}_2$, ..., $\hat{N}_8$) obtained from the subsets of lines selected by the eight survey designs.

Survey designs

For each survey design, the overall effort was chosen to be roughly the same as in the inshore zone (0-4 n.miles) of the original survey (440km), in order to represent what could realistically be achieved in one field season. The offshore zone and harbours and bays strata of the 1998 survey were both excluded.

Eight survey designs were compared (Figs 4-11), consisting of four types of stratification and two methods for allocating lines (random or systematic). The four types of stratification are summarised below.

Type 1 —single stratum.

Type 2 —stratification and effort as per 1998 survey. Areas to the north and south of the sanctuary were treated as one stratum. Effort was split roughly 40:60 for non-sanctuary:sanctuary. Effort intensity was greater in the sanctuary, with lines spaced at 2 n.miles compared to 4 n.miles outside the sanctuary for systematic line selection.

Type 3 —two strata (split at Goughs Bay, Banks Peninsula), effort was split roughly 38:62 for north:south. Intensity of effort (i.e. line-spacing) was the same for both strata.

Type 4 — stratification as per 1998 survey, with equal effort in the two strata. Effort was split 53:47 for non-sanctuary:sanctuary.

Design type 2 represents a 'good' stratification scheme, type 3 a 'poor' stratification scheme (given the hypothetical density illustrated in Fig. 1), and type 4 a 'good' scheme with 'poor' effort allocation. For systematic selection, the first line in each coastline block was selected randomly, with subsequent lines at regularly spaced intervals. Examples of each survey are given in Figs 4-11.

Measures of accuracy and precision

For each design, the relative bias, B_1, on the i^{th} iteration was calculated as:

$$B_i = \frac{\hat{N} - N_{Ti}}{N_{Ti}}$$

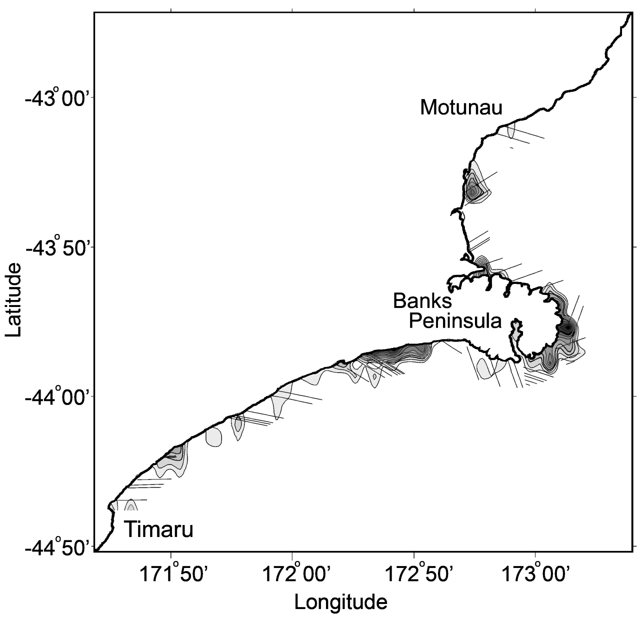

Fig. 4. Single stratum with random line selection.

where

$\hat{N}$ = estimated abundance from the 'survey',

N_{Ti} = the 'true' abundance of the i^{th} iteration (i.e. estimated using all lines).

For each design, an average bias, $\bar{B}$, was calculated as:

$$\bar{B} = \frac{\sum_{i=1}^{199} B_i}{199}$$

The precision associated with each design was summarised by:

$$CV(\hat{N}) = \frac{SD(\hat{N})}{\bar{\hat{N}}}$$

where $\bar{\hat{N}}$ is the mean and SD ($\bar{\hat{N}}$) is the standard deviation of the 199 estimates of N for that design. This is denoted the true $CV(\hat{N})$ to distinguish it from the mean of the estimates of $CV(\hat{N})$ provided by the program *Distance*.

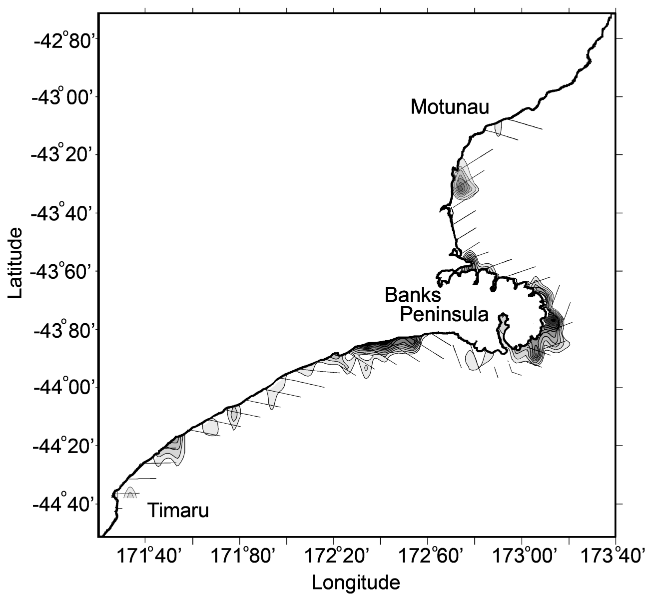

Fig. 5. Single stratum with systematic line selection.

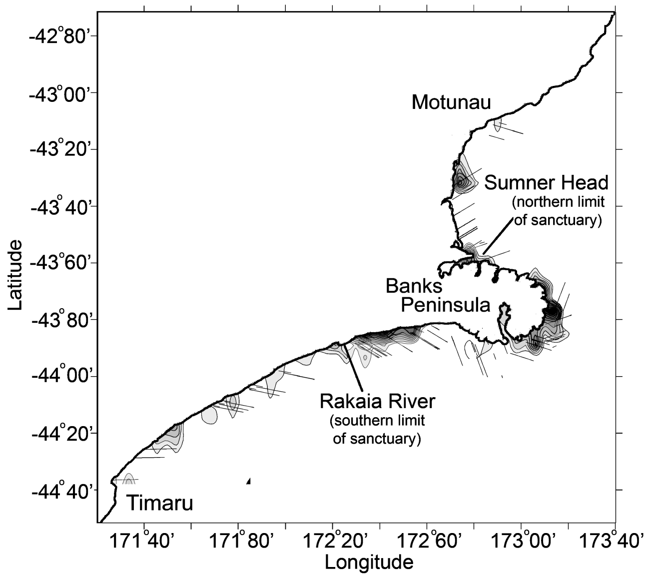

Fig. 6. Sanctuary treated as separate stratum, line selection is random, with greater effort inside the sanctuary. Effort is split roughly 40:60 for non-sanctuary:sanctuary, and intensity is double within the sanctuary.

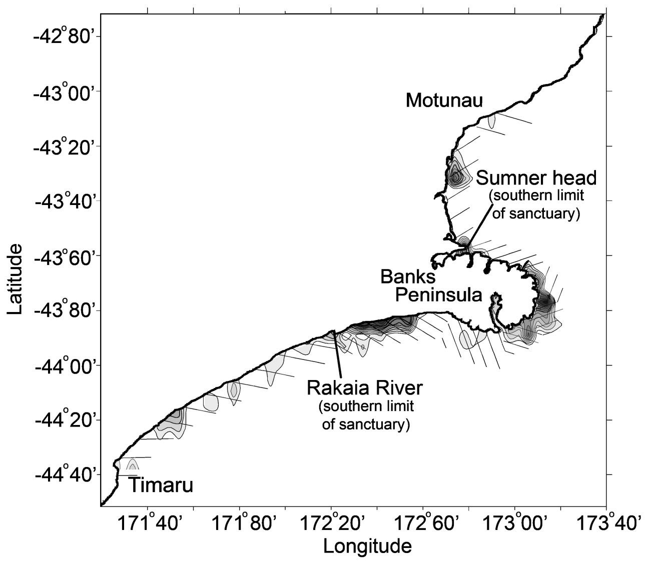

Fig. 7. Sanctuary treated as separate stratum, line selection is systematic. Lines spaced at 2 n.miles within sanctuary and at 4 n.miles to the north and south.

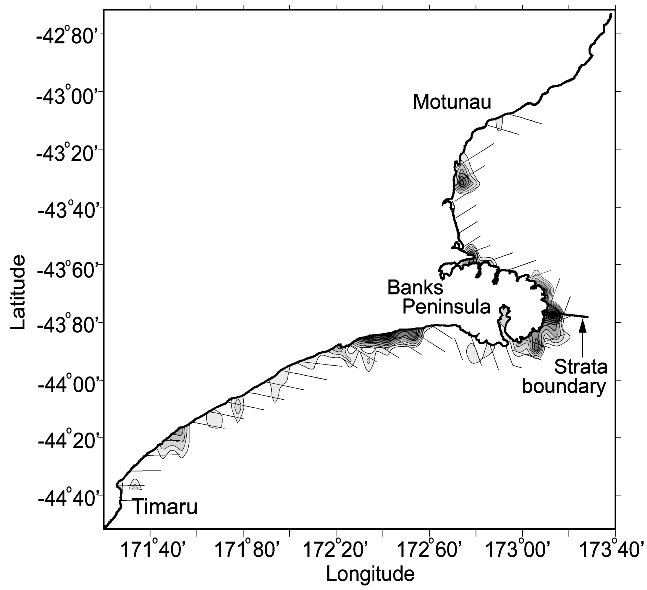

Fig. 9. Poor stratification scheme, effort is equal in both strata, and is weighted by area, line selection is systematic.

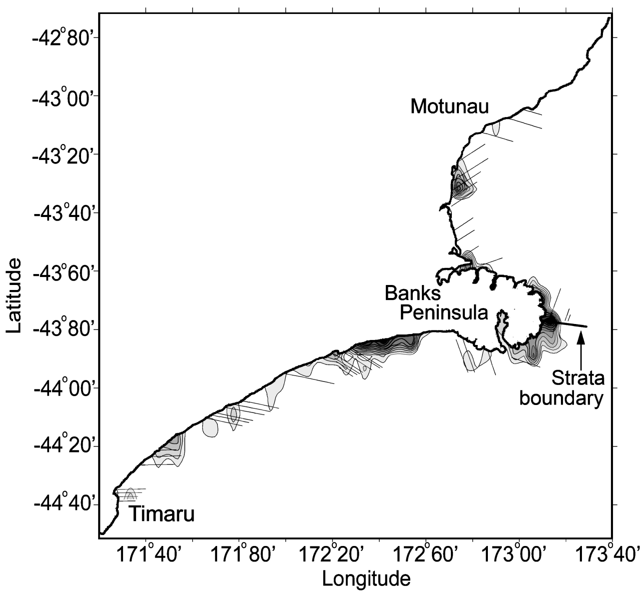

Fig. 8. Poor stratification scheme, effort is equal in both strata, and is weighted by area, line selection is random.

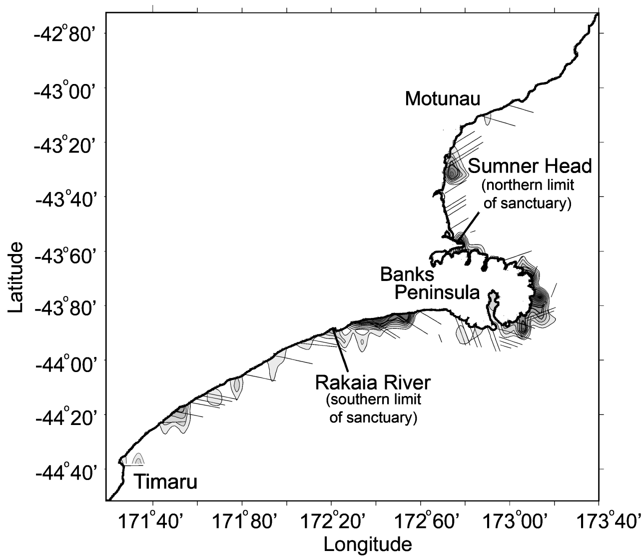

Fig. 10. Sanctuary is treated as a separate stratum, effort is equal in both strata.

In order to assess the precision of our estimate of the true $CV(\hat{N})$ from 199 iterations, its standard error was also calculated using the formula:

$$SE[CV(\hat{N})] = \frac{CV(\hat{N})}{\sqrt{2n_s}}$$

where n_s (=199) is the number of iterations for each design (Kotz and Johnson, 1982, p.29).

A further measure of interest is the confidence interval error rate, i.e. how often the interval does not contain the 'true' *N*. The upper rate (how often the true value of *N* is higher than the upper limit of the interval) will not necessarily be equal to the lower rate (how often the true value of *N* is less than the lower limit of the confidence interval) because the intervals are log-based and therefore asymmetrical.

The nature of the simulations means that survey design affects the encounter rate, but not the mean group size nor the effective strip width. Therefore, summary statistics for the encounter rate were calculated in addition to those for the estimate of abundance.

RESULTS

The results indicate that for the type of situation presented here, a systematic survey design generally provides a more precise estimate of abundance than a random design (Table 1a), with an average gain in true $CV(\hat{N})$ of 14% (the relative difference between true $CV(\hat{N})$ for systematic and random designs). However, for systematic designs this CV is over-estimated by an average of 22%. For all designs, the estimated amount of bias is small (mean = 2.7%) and the standard errors indicate that there is no evidence of real bias for all but one of the designs. The average 'true' abundance was 777 (CV=7%), and the estimates ranged from 648-905.

Encounter rates were similar for all designs, while the CVs were on average 8% lower for systematic designs (Table 1b). For the stratified designs, some differences in

Table 1a

Mean $\hat{N}$, mean bias $\overline{B}$ (%) and relevant statistics.

Survey		Results					
Type	Random or systematic	$\hat{N}$	True $CV(\hat{N})$	SE[True $CV(\hat{N})$]	Mean estimated $CV(\hat{N})$	$\overline{B}$ (%)	$SE(\overline{B})$ (%)
1	Random	775	0.340	0.017	0.388	0.1	2.4
	Systematic	784	0.301	0.015	0.385	0.9	2.0
2	Random	809	0.365	0.018	0.400	4.1	2.7
	Systematic	800	0.319	0.016	0.395	3.1	2.3
3	Random	796	0.354	0.018	0.394	2.2	2.5
	Systematic	819	0.303	0.015	0.388	5.3	2.2
4	Random	801	0.340	0.017	0.390	3.6	2.5
	Systematic	796	0.281	0.014	0.309	2.6	2.0

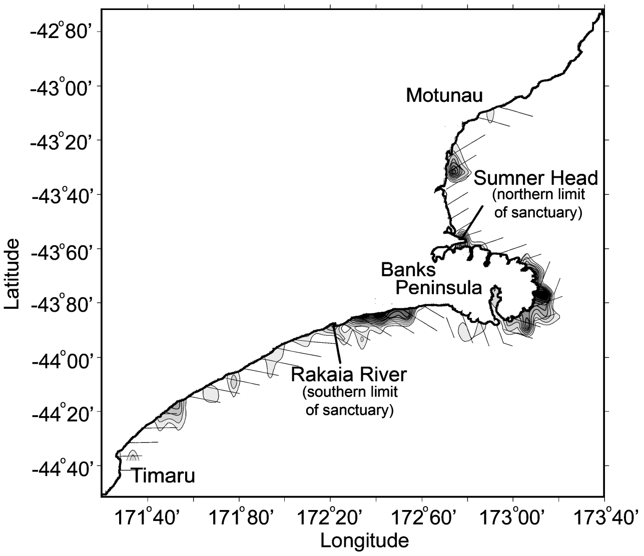

Fig. 11. Sanctuary is treated as a separate stratum, effort is equal in both strata.

Table 1b

Results for encounter rate, *n/L*.

Survey		Overall		Stratum 1		Stratum 2	
Type	Random or systematic	Mean	CV	Mean	CV	Mean	CV
1	Random	0.111	0.262				
	Systematic	0.110	0.180				
2	Random	0.113	0.279	0.112	0.427	0.114	0.356
	Systematic	0.113	0.211	0.104	0.372	0.123	0.182
3	Random	0.114	0.277	0.116	0.456	0.111	0.329
	Systematic	0.103	0.181	0.068	0.312	0.138	0.218
4	Random	0.113	0.267	0.110	0.396	0.117	0.383
	Systematic	0.112	0.182	0.104	0.277	0.120	0.239

Table 2

Confidence interval error rates.

Survey		Confidence interval error rates (%)		
Type	Random or systematic	Lower	Upper	Overall
1	Random	0.0	3.5	3.5
	Systematic	0.0	0.5	0.5
2	Random	0.5	2.5	2.0
	Systematic	0.0	1.0	1.0
3	Random	0.0	3.0	3.0
	Systematic	0.5	0.5	1.0
4	Random	1.5	2.5	4.0
	Systematic	0.0	0.5	0.5

encounter rate between strata were apparent for systematic designs, but not for random designs (Table 1b). In general, stratification did not offer any clear benefits, with no significant gains in precision (Table 1a).

All the overall error rates were less than 5%, indicating that the intervals were a little too wide (Table 2). This was particularly true for the systematic designs. For all designs, the lower limit was too low, while for systematic designs the upper limit was too high.

Note that the standard errors for the true $CV(\hat{N})$ are all small, indicating that there would be little gain from increasing the number of iterations in the simulation.

DISCUSSION

The pattern of lower true $CV(\hat{N})$ for systematic surveys is consistent with results from classical sampling theory (Cochran, 1977). The mean estimated precision was broadly the same for random and systematic designs. This would be expected, as the calculation of $CV(\hat{N})$ within *Distance* assumes a random line placement.

It is worth noting that the systematic designs used in these simulations have an element of randomisation, resulting in uniform coverage probability throughout the area. However, because only the first line in each block is selected randomly, the lines are not selected independently. Hiby and Hammond (1989) argue that this can result in a biased variance estimator. Our results confirm this, and suggest that there is also some bias associated with random designs, albeit to a lesser extent. This is in contrast with classical sampling theory, where random sampling leads to an unbiased estimate of variance for the sample mean. For a systematic design, $\hat{N}$ would be biased if dolphin density varies according to a repeating pattern that matches the distance between transects, because we may consistently sample all high or all low density areas. Also, true $CV(\hat{N})$ would be underestimated**,** as there would be little variation in encounter rate between transects. In practice, this situation seems highly unlikely to occur.

When sampling clumped distributions, randomly placed transects may fall predominantly in areas of especially high or low density. This is far less likely in a systematic design such as that shown in Fig. 7, and explains why the CV for encounter rate, and therefore $\hat{N}$, is greater for random designs.

Our results are in agreement with Strindberg and Buckland (2004) who showed that systematic grids of parallel lines have better spatial distribution than random lines. Other simulation work has also confirmed that when there is a trend in density, systematic surveys will show less variation in $\hat{N}$ (Fewster and Buckland, 2004; Strindberg and Buckland, 2004). Additionally, if density is variable, precision of estimates of N are often poor; a situation which is somewhat improved by using a systematic design (Strindberg and Buckland, 2004).

A practical consideration for cetacean surveys is that the presence of the survey vessel is likely to influence animal distribution, either as a result of vessel avoidance or vessel attraction (Turnock *et al.*, 1995; Dawson *et al.*, 2004). It would make little sense to re-sample an area, a situation possible with a random design, unless sufficient time had passed that the assumption of uninfluenced distribution was again satisfied. Providing line spacing is adequate, this problem is minimised by using a systematic design.

These results do not show any clear benefit of the stratification schemes considered, there being little gain in terms of precision. Stratification will generally lead to a more precise estimate if the variation between stratum means is high. It is unlikely to offer any gains if the spatial scale of the patchiness of the population is smaller than the scale of the stratification. An important consideration is that stratification will be beneficial when substantial distribution data are available during the design phase. When this is not the case there are alternatives such as a two-phase design and post-stratification (although this comes at a cost, since it can lead to biased abundance estimates; Buckland *et al.*, 2001). However, there are often practical reasons for stratification, such as when there are areas of specific interest to management, as in the case of the original Banks Peninsula survey. Stratification may also offer more benefits (in terms of precision) in areas where animals are more highly clustered than in the population considered here.

In summary, these results suggest that systematic designs should be given preference over random designs, even though variance is overestimated. Systematic designs have important practical advantages (see Dawson *et al.*, 2004) and provide better information on spatial distribution than random designs. Where there are no existing data with which to decide upon an appropriate stratification scheme, or if there are no areas of intrinsic interest, a non-stratified, systematic survey would be the best choice and provide data necessary for potential stratification of future surveys.

The approach taken in this study, creating a spatial model of density and overlaying different survey designs in order to explore their performance, is useful beyond what has been considered here. An obvious next step could be to vary the degree of clustering to see under what circumstances stratification makes appreciable differences to precision.

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