Monitoring stoat (*Mustela erminea*) control operations: power analysis and design

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Abstract

The aim of this report is to look at the design of a monitoring programme for stoat control operations, using tracking tunnels. We use tracking tunnel data from a study in North Okarito Forest to develop this design and to evaluate the success of different survey protocols. We discuss the importance of understanding statistical power. Power is a measure of the likelihood of reaching the correct conclusion about the success of a monitoring programme, and is one of the crucial factors that should be considered in designing a monitoring programme. The relative costs of falsely concluding a control operation was successful when it was not, or of falsely concluding a control operation was not successful when it was, need to be carefully evaluated.

We recommend for monitoring stoats with tracking tunnels that tunnel-stations be spaced 1 km apart and multiple tunnels be used at each station. The actual survey design for monitoring will depend on the acceptable error rates, the desired level of power, the target percentage kill, and the pre-control stoat density. We have constructed a model to estimate the number of stations and inspections for combinations of type I error rates, power, target percentage kill and pre-control stoat density.

1. Introduction

1.1 MONITORING STOAT CONTROL OPERATIONS

Monitoring is an essential component of conservation management. The Department of Conservation conducts *operational* monitoring to assess whether its control targets (e.g., percentage kill of a pest animal) have been achieved, and *performance* monitoring to assess how well a management action has protected a specific resource (e.g., reduction in predation of eggs and chicks).

The success or failure of a monitoring programme can be measured by its ability to detect a biologically significant population change. This ability depends on the way the monitoring programme is designed (Green 1979; Norton 1996). Poorly designed monitoring programmes can increase the risk of making either of two potentially costly mistakes:

1. concluding that an operation was successful in reducing pest numbers to a target level, when in fact it was not, or
2. concluding the control operation was not successful, when in fact it was.

The first mistake may result in the loss of the conservation resource that was thought to have been protected, and the second may result in a manager repeating a control operation when it was not necessary.

There are an increasing number of stoat control operations being carried out by the Department of Conservation. The monitoring component of these operations must be appropriately designed. This is particularly important when control operations are conducted over large areas to protect widely dispersed or wide ranging species, e.g., kiwi in North Okarito forest, or a suite of species as in a mainland island restoration project. King & Edgar (1977, and see also King 1994) recommended that tracking rates from ink-print tunnels were the best means to monitor stoats in New Zealand and Moors (1978) first used tracking tunnels at Kaikoura to monitor the seasonal and annual changes in a mustelid population. To date, however, there has not been a standardised protocol developed for the spatial layout of tracking tunnels to monitor stoat control operations; however, this is currently being worked on (E. Murphy, pers. comm.)

1.2 AIM

The aim of this report is to look at design of tracking tunnel monitoring for stoat control operations. We used tracking tunnel data from a study in North Okarito forest (Miller & Elliott 1997) and data from a current study on stoat home ranges in podocarp forest (Miller *et al.*, unpubl. data) to develop a stoat-monitoring design and to evaluate the success of different survey protocols.

1.3 TWO IMPORTANT CONCEPTS FOR ENVIRONMENTAL MONITORING

1.3.1 Independent data

Ideally, monitoring should be designed so the data are independent in order to maximise the information gained from the field surveys. For example, if two tracking stations are placed close together and the same stoat tracks both stations there is little gained by way of new information from the second tracking station, because the presence of stoats has been detected in the first station. Similarly, if a repeat inspection of tracking tunnels is conducted soon after the first inspection, and in that time there is little change in the stoat population, most of the information on the tracking rates will have been collected during the first inspection.

In both these examples the survey effort would be better used in checking stations that were further apart in space or time. In the first example, the stations should be placed further apart on the ground (spatially), and in the second example the station inspections should be further apart in time (temporally). Therefore, with adequate spatial and temporal spacing, for a given population size, whether or not one tunnel-station is tracked has no influence on whether the neighbouring tunnel-station will be tracked. This is known as statistical independence.

1.3.2 Statistical power

Statistical power is one of the crucial factors that should be considered in designing a monitoring programme (Taylor and Gerrodette, 1993, Fairweather 1991). Statistical power can be viewed as a measure of the likelihood of a monitoring programme reaching the correct conclusion. If a stoat population has declined after a control operation then a monitoring programme with sufficient power should be able to detect this change.

Consider the following example. If a stoat population has been reduced by 50% after a control operation, and the population were monitored pre- and post-control, the correct change in population size would be estimated if the tracking rates had reduced by 50%. However, if the tracking rates reduced by a smaller amount, or not at all, and the statistical test did not provide sufficient evidence to say with confidence that the population has been reduced, the manager may decide that the control operation was not successful. The manager may then repeat the control operation unnecessarily, or erroneously conclude that their control strategy was not effective and change strategies. Statistically, this is known as committing a type II error, which has a probability β (Peterman 1990, Steidl *et al.* 1997). A type II error is when the statistical test fails to detect a real change.

With stoat control the cost of making this type of error, where the success of the control operation is not detected, can be considered ‘long term’, i.e. unnecessary resources may be expended, or feedback on the effectiveness of control strategies for refinement may be delayed. In the short term, kiwi would still be protected because the control operation will be repeated, but at extra and unnecessary cost.

The alternative ‘error’ scenario is that the stoat control operation was not effective, i.e., the population was not reduced, but the data indicated that there had been a reduction in stoat numbers. This is quite plausible because, by chance, on one survey the tracking rates may be high, and by chance, on a post-control survey the rates may be low, even with no real change in the stoat population. If the tracking rates reduced by 50%, the manager could infer that the control operation was successful and fail to take the necessary remedial action to protect kiwi from predation. Statistically, this is known as committing a type I error, which has a probability α (Peterman 1990, Steidl *et al.* 1997). A type I error is when the statistical test used detects a non-existent change. There is an obvious immediate and high cost in making this type I error because no further protection may be given to the kiwis even though stoat numbers remain high.

These type I and II errors can be estimated, and are usually defined as being a probability, e.g., 0.05 is commonly used as the acceptable type I error rate, and 0.2 is often considered acceptable as the type II error rate. It is important to note that these two values are not ‘written in stone’, and that the choice of acceptable probabilities, α and β, should be relevant to the situation.

Statistical power is estimated from the type II error rate, and is calculated as $1-\beta$. Consider the example above where a population has been reduced with a successful control operation. If the probability of making a type II error is 0.2 ($\beta = 0.2$), then the probability of making the correct decision is 0.8. In this example, having statistical power of 0.8 means there is an 80% likelihood that the statistical test will lead to the correct decision that the population has been reduced.

Because of the risks involved in conservation management by making a wrong decision, monitoring programmes should be designed to achieve high power. There are three ways of doing this:

3. Increase the survey effort by using more tracking stations. With greater effort the tracking rates are likely to be more reliable and are more likely to accurately reflect the true situation.
4. Achieve high tracking rates and larger reductions in tracking rates.
5. Change the acceptable type I error rate, e.g., from 0.05 to 0.1, because the type I error rate is positively related to power. When the type I error is high, the type II error rate is low and power is high (Peterman 1990).

Choosing which combination of these is acceptable or achievable becomes a management decision, based on factors such as resources, and the consequences of making the wrong decision.

2. Method

2.1 DATA COLLECTION

Tracking tunnel data were collected during an experiment to test the effectiveness of poisoned eggs for controlling stoats (Miller & Elliott 1997). The study was conducted in North Okarito forest at two sites known as River and Loop. The sites were approximately 4 km apart. The same grid layout of 29 stations, with one tracking tunnel at each station, was used at both sites (Fig. 1). Tunnels, either 500 m or 250 m apart, were baited with a small piece of beef and inspected for stoat tracks every one to two weeks. The tracking tunnels were used to monitor the relative abundance of stoats, and to estimate the percentage kill achieved. The difference between pre- and post-control tracking rates was used as an estimate of the percentage reduction in the stoat population.

Data were collected from 21 tunnel inspections at Loop site over a period of 5 months (22 August 1996 to 25 March 1997), and at River site, from 20 tunnel inspections over a similar period (21 August 1996 to 26 March 1997). Hens eggs poisoned with 1080 were placed in the River site after the 12th tunnel inspection, and in the Loop site after the 19th tunnel inspection.

2.2 DATA ANALYSIS

2.2.1 Definition of tracking rate

We defined the tracking rate as the average proportion of tunnel-stations found tracked on an inspection. For example, if 20 stations are inspected five times and the number of stations found to be tracked were 2, 3, 2, 1, and 2, the tracking rate would be $\{(2 + 3 + 2 + 1 + 2)/5\}/20 = 0.1$. If, after the control operation, the number of stations tracked were 1, 0, 1, 2, and 1, the post-control tracking rate would be $\{(1 + 0 + 1 + 2 + 1)/5\}/20 = 0.05$. The estimate of the kill rate was calculated from the difference between the pre- and post-control tracking data: $(0.1 - 0.05)/0.1 = 0.5$, or 50% control.

FIGURE 1. TUNNEL-STATION LAYOUT, NORTH OKARITO FOREST. THERE WERE 29 TUNNEL-STATIONS IN TOTAL, SPACED EITHER 500 m OR 250 m APART.

2.2.2 Independence of tunnel-station counts

We calculated the distances between all pairs of stations and the correlations between pre-control station counts to see if there was any relationship between the spatial distance between stations and the differences in station counts. Station pairs were divided into 500 m intervals of inter-station distances. The station count was the number of times the station was found tracked during the pre-control inspections. If there was spatial correlation then stations located close to each other would have similar counts.

The relationship between the temporal distance between stations and the differences in station counts was examined with a Mantel randomisation test (Manly 1997, p. 174). Station counts in this temporal-analysis were the number of tunnels that were found tracked at an inspection. If there were temporal correlation inspections spaced 1 or 2 weeks apart would have similar counts. To estimate the significance level of any observed correlation, the correlation between the matrix of the station count differences and the matrix of the inverse of the spatial differences between inspections were calculated and compared to 9999 correlations where one of the matrices was randomly shuffled.

2.2.3 Estimation of power

The power to detect a difference in station counts pre- and post-control was estimated by simulation (Beier and Cunningham 1996) using a binomial model. The post-control probability of a station being tracked was calculated as a proportional reduction of the pre-control probability. Individual station counts were the number of inspections the station was tracked and the observed difference in the sum of the station counts between pre- and post-control was calculated. The observed difference was compared to differences when the pre- and post- station counts were pooled and the pool sampled, with replacement, 999 times. The proportion of simulated samples where the difference was at least as small as the observed difference was used as the 1-tailed significance level.

The generation of station counts, and simulated resampling, was repeated 1000 times and power was estimated as the proportion of the significance levels less than the type I error rate. The factors varied in the simulation were: the number of stations, the number of station inspections in the survey (the same number of inspections were used for pre- and post-control surveys), the initial probability of a station being tracked and the percentage decrease in this probability, and α.

Factor levels were:

- pre-control probability of a station being tracked: 0.08, 0.15, and 0.30
- % control: 25%, 33.33%, 50%, and 75%
- number of station inspections (for either pre- or post-survey): 5, 10, and 15
- number of stations: 10, 25, and 50
- 0.05, 0.1, and 0.2

The initial probability levels were chosen based on the data from North Okarito forest. The average tracking rate over 19 inspections at Loop site was 1.5517, or 0.07 of the tunnels on an individual inspection were tracked. At River site the average tunnel count over 12 inspections was 1.0690, or 0.09 for an individual inspection.

The simulation method assumes that the probability of a station being tracked at an inspection is independent of the probability of it being tracked on a previous or subsequent inspection, and is independent of the tracking rate at neighbouring stations.

3. Results

3.1 STATION SPACING—SPATIALLY

The number of the tunnels found to be tracked at an inspection ranged from 0 to 12 (Fig. 2). On average, the overall tracking rate was 0.08, i.e., 8% of the tunnels were tracked at any individual inspection. Some of the tunnels, e.g., tunnel 16, were never tracked during the monitoring (Fig. 3), while tunnel number 6, located 500 m away (Fig. 1) was tracked on five inspections at Loop site, and on 3 inspections at River site.

Counts from stations over 2 km apart were negatively correlated, i.e., when one station tunnel count was high the other was low (Fig. 4). This suggests that within the study site stoats were not uniform in density, but were patchily distributed at a scale greater than 2 km. Similar trends were observed at both sites. Stations that were 500 m apart had similar counts (positive correlation).

Positive correlation would be expected if the same stoats were running through adjacent tunnels. Stoat home ranges for podocarp forest have been reported to be 27 to 190 ha for females, and 145 to 453 ha for males (Miller *et al.*, unpubl. data). For these home ranges, with a station spacing of 500 m, more than one

FIGURE 2. TUNNEL COUNTS—THE NUMBER OF TUNNELS TRACKED AT EACH INSPECTION.

FIGURE 3. TUNNEL COUNTS—THE NUMBER OF INSPECTIONS THE TUNNELS WERE TRACKED.

FIGURE 4. SPATIAL CORRELATION BETWEEN TUNNEL COUNTS.

tunnel would be within a home range and tracking rates of adjacent stations could be correlated.

3.2 STATION SPACING—TEMPORALLY

Station counts varied between inspections (Fig. 2). For example, at Loop site, 12 of the 29 tunnels were recorded as tracked in the inspection on the 12th week while in week 13 only 3 were tracked. The variation in weekly counts differed between the two sites. Week 12 had the highest count at Loop site, and at River site week 5 had the highest count.

In the first 4 weeks at Loop site no tunnel was tracked, and at River site the first 2 weeks had low counts. These results are consistent with a behavioural response where an initial period is spent in discovering or learning to run through the tunnels.

There was no evidence that the counts were correlated between the weekly inspections at either Loop site ($r = 0.06$, $P = 0.79$), or River site ($r = 0.04$, $P = 0.62$).

3.3 ESTIMATES OF POWER TO DETECT CONTROL RATES

The binomial model fitted the study data reasonably well and was a useful tool for approximating the effect of changes in survey design on statistical power. A comparison of the expected counts from the binomial model with the actual counts show that in the initial period of the survey the model slightly overestimated the station counts and underestimated the counts in the later period for Loop site (Fig. 5). This trend was the opposite of that at the River site.

3.3.1 Survey effort

The results from the model suggest that the ability to detect the true control rate (power) for stoats improves with more survey effort (Fig. 6). With five weekly inspections the chance of detecting that a control operation achieved a 50% kill was estimated to be 40% when there were 25 stations, the initial probability of a station being tracked between inspections was 0.08, and $\alpha = 0.05$. With twice as many stations, 50, the chance increased to 60%. Extra survey effort can be either by way of more stations, or more inspections. For example, we estimated that to have an 80% chance of detecting a 50% control either 50 stations should be used and inspected weekly over 10 weeks, or 30 stations inspected over 15 weeks.

3.3.2 Control rate

Our model shows that larger reductions in stoat populations can be more reliably detected than smaller reductions. With 5-weekly inspections the chance of detecting a 50% control with 25 stations was estimated to be 25%, but the chance of detecting a 75% control was estimated to be 65% (Fig. 7). With more survey effort large kill rates can be even more reliably detected. With 10 inspections and 25 stations the chance of detecting a 50% control was estimated to be 50% and the chance of detecting a 75% control was estimated to be 85%. With ten inspections and 50 stations the ability to detect a 75% control was estimated to be 90%.

LOOP SITE

RIVER SITE

FIGURE 5. COMPARISON OF ACTUAL AND SIMULATED COUNTS FOR LOOP AND RIVER SITES.

3.3.3 Tracking rate

Higher pre-control tracking rates improved the chance of detecting the percentage kill with our model. For example, with a pre-control tracking rate of 0.15 the ability to detect a 50% control with five weekly inspections and 25 stations was 50%, but increased to 90% when the pre-control rate was 0.30 (Fig. 8). With a pre-control tracking rate of 0.08, even with 50 stations and five inspections the chance of detecting a 50% kill was only 50%, although with 15 inspections the power increased to 90% (Fig. 8).

FIGURE 6. ESTIMATED POWER WITH DIFFERENT NUMBER OF INSPECTIONS FOR DETECTING A 50% KILL, WITH A PRE-CONTROL TRACKING RATE OF 0.08.

FIGURE 7. ESTIMATED POWER WITH DIFFERENT NUMBERS OF INSPECTIONS AND PERCENTAGE KILL, WITH A PRE-CONTROL TRACKING RATE OF 0.08.

FIGURE 8. ESTIMATED POWER WITH DIFFERENT PRE-CONTROL TRACKING RATES FOR DETECTING 50% KILL WITH FIVE INSPECTIONS.

3.3.4 Type I error rate

The ability to detect the true control rate was improved by accepting higher risks of falsely detecting a successful control operation when in fact there has been no reduction in the stoat population (type I error, α). For example with $\alpha = 0.05$, and with 25 stations the chance of detecting a 50% kill was 50%, but when $\alpha = 0.10$, the chance was 70%. Power improved further to 85% if $\alpha = 0.2$ were used (Fig. 9).

FIGURE 9. ESTIMATED POWER WITH DIFFERENT TYPE I ERROR RATES, α, FOR DETECTING 50% KILL WITH FIVE INSPECTIONS.

4. Discussion

4.1 SPACING OF TUNNEL-STATIONS

Tunnel-station spacing should preferably be at the rate of one per home range to reduce the chance of the same stoat running through more than one tunnel (Diefenbach *et al.* 1994). Spring home ranges for stoats of between 27 and 190 ha for females and 145 to 453 ha for males have been recorded in south Westland podocarp forest (Miller *et al.* unpubl. data). The home ranges of stoats in beech forest are of a similar size (Murphy and Dowding 1994). However the shape of home ranges varies. In their study Murphy and Dowding (1994) found ranges in beech forest to be quite linear, with an average range length of 2.3 ± 0.3 km for females and 4.0 ± 0.9 km for males, whereas the home ranges of stoats in podocarp forest appear to be less linear and more angular or circular. Murphy and Dowding (1995) report that the outer limits of stoat home ranges often overlap, although there appears to be a core area which does not. Home ranges may also contract or expand between years and within a year (Murphy and Dowding, 1995, Miller *et al.*, unpubl data).

From the data reported in Miller and Elliott (1997) and used in this study, stations spaced more than 4 km apart would help ensure that the counts of tracked stations were independent. However, this spacing is impractical for logistical reasons, and the uneven spacing and potential overlap of stoat home ranges in the landscape, and the presence of transients, will reduce the independence of the data, even at very wide spacing. Consequently there is unlikely to be any one ideal spacing. The choice will be a compromise between logistic realities and scientific rigour. On balance, we suggest that a spacing of 1 km between stations will allow at least one station to fall within a stoat's home range, and will be logistically feasible. In this study, there was little correlation between stations 1 km apart.

4.2 STATION INSPECTIONS

There was no evidence in our study that station counts spaced a week apart were correlated, and for future monitoring we recommend continuing with weekly counts. There is a potential problem with this design in that the same stoat may be tracking the same station each week (Kendall *et al.* 1992).

We did observe that the initial station counts were low, which is consistent with other tracking tunnel studies. When tracking data are used to estimate control rates, initial low station counts will cause an underestimate of the true control rate. Therefore, we recommend that the initial 3 weeks of data be excluded from the analysis.

4.3 MULTIPLE TUNNELS AT A STATION

The ability to detect a reduction in the stoat population with our model improved with higher tracking rates, i.e., with a greater number of tunnels tracked. Therefore, within the design we want to increase the likelihood that an animal will encounter a tunnel and will then go through it. One way to achieve higher tracking rates is to use multiple, baited, tunnels at each station. This will reduce the bias from not detecting a stoat if it was in the area but hadn't encountered, or did not want to go through, a particular tunnel (Zeilinski and Stauffer 1996, Diefenbach *et al.* 1994).

With this design, the station is considered the sample unit, not the tunnels. Therefore, the data recorded at each station is only the presence or absence of a stoat. At each station, with a spacing of 1 km, the tunnels should be clustered closely together. The optimum number of tunnels within a station needs further research and field validation, but as an estimate we recommend three tunnels per station, spaced 50 m apart.

The question will be asked 'since 1 km has to be walked between stations, why not have some intermediate tunnels along the path, e.g., 100 m apart, which can be inspected for little extra cost?' This is best answered by an example. Consider a design with a line of 10 stations of 3 tunnels where the stations were 1 km apart, and another with a line of 30 tunnels 100 m apart. Both designs use 30 tunnels. In the first design the sample size is 10 since there are 10 stations, and in the later the sample size is not 30 as would be expected but 1 since the tunnels are not independent.

It is erroneous to use the number of tunnels as the sample size, when tunnels are not independent and with no adjustment for this bias. Typically, pest monitoring uses multiple lines of stations (e.g., lines of tunnels or traps). If the stations are not independent but the lines are, the size of the sample is the number of lines and there may be too few lines to ensure large enough sample sizes to achieve sufficient power to detect the true control rate. With the proposed design of widely spaced tunnel-stations, each station contributes to the sample size and adequate sample sizes may be achieved without excessive effort. If lines of non-independent tunnels are used, the only way to increase the sample size would be to increase the number of lines, which can be costly.

Estimates of control rates calculated using the difference between pre- and post-control tracking rates should use the proportion of stations that had at least one tunnel tracked in an inspection, averaged over inspections.

An alternative method for increasing the tracking rate is to use a time interval longer than 1 week between station inspections. The risk with this strategy is that the total time interval between the start and finish of the monitoring survey will be long, and stoat numbers may change within this time through, for example, behavioural changes. For example, as alternative food sources become available in the forest the stoat's attraction to the tunnel bait may change. After a control operation stoats may also begin re-invading the control area, and if the post-control monitoring extends into the period when re-invasion begins to occur, control rates will be underestimated.

4.4 RANDOM LOCATION OF STATIONS

Another design to avoid the problem of lack of independence of station counts is to randomly locate the stations in the study area (van der Meer 1997, Lesica and Steele 1996). However, this can be an impractical design for sites such as North Okarito where access between stations is very difficult.

These results from our analysis are based on grids of stations, but more commonly stations are laid out along a line and this may lead to some variation between our results and those of other studies (e.g., Brown *et al.* 1996). When stations are systematically laid out along the survey line, the stations should still be 1 km apart. The first station should be randomly located within the first km and subsequent stations should be at 1 km intervals.

4.5 SURVEY EFFORT

The level of survey effort (the number of stations, and the number of inspections) for monitoring can be estimated by using our model and Figs. 6 to 9, provided the pre-control tracking rate can be estimated. If this is not available, the previous year's data, or data from similar sites can be used. Any estimate should be conservative to ensure monitoring is at least as effective as predicted. Our estimate of the ability to detect the true control rate improved with more survey effort, either by having more inspections or more stations. For example, we estimated a 50% kill could be detected with 60% power with either: 20 stations and 15 inspections; 30 stations and 10 inspections; or 55 stations and five inspections (Fig. 6). In most studies it would be more cost efficient to increase the number of stations rather than the number of inspections to improve power.

Our model can also be used to estimate power retrospectively once the monitoring has been conducted (Thomas 1997). If a control operation was thought to have not been effective, retrospective power analysis can be used to estimate the likelihood of detecting the target percentage kill given the monitoring effort. If the likelihood was low, e.g., 20%, the manager should have little confidence in the monitoring results, and their survey protocol for monitoring should be refined.

The survey design used will depend on the required amount of power and the acceptable error rates. The ability to detect the actual control-rate is a compromise between making errors in reporting a control as being successful when it was not, and failing to detect a successful control (type I and II errors respectively). The ratio between the costs of these two errors can be used to help decide on appropriate levels of power rather than choosing an arbitrary value (Mapstone 1995).

We suggest that the ratio of the cost of a type II error to a type I error could be set at 0.5, i.e., the immediate short term loss of falsely concluding a control was successful when it was not, is twice the long term cost of failing to detect a successful control. This is because if a control were not successful managers would need to undertake extra protection work for the conservation resource. If the unsuccessful control operation were thought to be successful extra work may not be carried out.

With this cost ratio, the ratio between the type I and II error rates should therefore be 0.5, e.g., if the type I error rate were chosen to be 0.1 then the type II error rate should be set at 0.2, which will give power of 0.8. If a lower type I error rate were used, e.g., 0.05, then the type II error rate would be 0.1, with power of 0.9. With this procedure of setting power based on relative error costs, managers need only decide on the acceptable type I error rate.

4.6 OTHER DESIGN CONSIDERATIONS

The ability to detect a control-rate may be improved by using a stratified design, where additional survey effort is focused on areas within the study site of either lower stoat numbers, or higher variability among stations (Zeilinski and Stauffer 1996). In the more usual stratified design extra effort is allocated to areas of higher abundance, but for monitoring stoats more power would be achieved by putting more effort into the areas of low tracking rates. Extra effort can be either via more station inspections, or with more tunnels in each station. We observed that for Loop site some groups of tunnels were tracked more than others and this would suggest that within this site the area could be stratified.

4.7 OTHER CONSIDERATIONS

It is important to note that the data used in this study come from a very large forested area, and that the density of stoats appeared to be low (Miller *et al.* unpubl. data) which may affect the spacing of individual stoats and the degree of home range overlap. Most existing mainland island restoration sites are in the order of 800 to 1500 ha, and stoat density and home range overlap may be relatively high (E. Murphy pers. comm.). Consequently, trying to fit, say, 50 stations into this area at 1 km spacings is ridiculous. Determining an appropriate station spacing for situations such as these requires further research.

The results and recommendations from this study are also developed using a before-after design, however the use of a BACI (before - after, control - impact [treatment - non-treatment]) design is recommended for all monitoring where possible. In using this approach it may be possible increase management confidence in the achievement of a real reduction, while compensating for a lower level of power achieved when using fewer stations per area. Further research is required.

5. Recommendations—management and research

5.1 MANAGEMENT

We recommend for monitoring stoats with tracking tunnels in large (e.g., > 2000 ha) areas that:

- That a treatment site and non-treatment site be monitored where possible,
- Tunnel-stations be spaced 1 km apart,
- Stations be inspected weekly,
- Multiple tunnels are used at each station. We recommend stations of three tunnels spaced 50 m apart,
- Data from the first 3 weeks after tunnels are first laid out and baited should not be used in estimating control rates.

The actual survey design for monitoring and the amount of survey effort; the number of stations and inspections, will depend on the acceptable error rates, the desired level of power, the target percentage kill, and the pre-control stoat density.

The decision on what are acceptable error rates should be evaluated carefully by considering the consequences of making such an error. The relative cost of the type I and type II errors can be used to estimate the type II error rate, and the desired level of statistical power. We recommend that the ratio, $\mathrm{cost}_{II}/\mathrm{cost}_{I} = 0.5$ be used as a preliminary estimate, and with a type I error of 0.1, the type II error should be 0.2, which will result in 80% power.

5.2 RESEARCH

- Field trials should be conducted, and results analysed for statistical power, to more accurately define optimum tunnel layout and to field validate our results.
- A computer model should be produced for managers to use in planning monitoring and deciding on the number of stations and inspections. We have compiled the mathematical part of such a model for combinations of type I error rates, power, target percentage kill and pre-control stoat density but with further funding the programme could be developed to make it more user-friendly and accessible to managers. With some modifications the model also could be used for planning monitoring of other pest species, e.g., possums, rats, mice, and for use with traps and wax blocks. Such a model will be useful for evaluating future monitoring strategies. Retrospectively it can be used to check that there was sufficient statistical power in a monitoring operation to be able to detect the true kill rate.

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