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Chapter 10

Estimating detection rates and probabilities

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Abstract

Surveillance activities provide only imperfect detection of biosecurity threats, and so quantifying detection rates and probabilities is important for making reliable inferences from survey data. Furthermore, an understanding of detection rates and probabilities allows for effective survey design and resource allocation to address biosecurity threats. Detection rates and probabilities can be estimated directly via experiments or indirectly during routine surveillance activities. In this chapter we introduce basic presence/absence detection models and provide guidelines for designing a detection experiment. We also summarise other methods available for estimating detection rates and probabilities when pest abundance, as well as presence/absence, affects management.

Introduction

Biosecurity surveillance is inevitably imperfect; not all infestations are guaranteed to be detected immediately on incursion. Furthermore, the probability of successful detection is influenced by the surveillance effort and the detection rate of the target.

The detection rate typically describes the frequency of detection events for a species, population or individual under a given set of survey conditions. It is context-specific and likely to depend on a range of factors such as the targets' appearance and distribution, the surrounding environment and the observation process. Detection rates describe a continuous survey and detection process. They can be used to calculate the detection probability for a specific level of survey effort, such as a discrete survey design.

Detection rates and probabilities can have major implications for survey design and management decisions. They allow a manager to estimate the risk of detection failure and the potential consequences arising from a survey design (for example, see Chapters 4 and 14, this volume). The manager can ensure that sufficient effort is allocated to reduce invasion risk or consequences to an acceptable level. When they are measured in the same currency, surveillance effort and consequences can be traded off directly to determine the survey design that minimises overall costs.

Detection probabilities have informed survey design for proof of freedom and early detection (Mehta *et al.*, 2007; Coulston *et al.*, 2008; Chapter 18, this volume), delimitation and containment (Bode *et al.*, 2009; Homans and Horie, 2011; Epanchin-Niell *et al.*, 2012) and eradication programs (Regan *et al.*, 2006; Cacho *et al.*, 2007; Ramsey *et al.*, 2009; Rout *et al.*, 2009; Hester *et al.*, 2010). Detection probabilities can inform prioritisation of effort across space (Hauser and McCarthy, 2009; Chadès *et al.*, 2011; Emry *et al.*, 2011) and amongst species (Skurka Darin *et al.*, 2011; Chapter 14, this volume), and cost-effective allocation of resources amongst surveillance and other activities (Moore *et al.*, 2010; Ndeffo Mbah and Gilligan, 2010; Baxter and Possingham, 2011; Rout *et al.*, 2011).

Nevertheless, it can be difficult to estimate detection rates and probabilities. Many studies in the literature rely on expert opinion or nominate values for illustration only. It is possible to estimate detection rates or probabilities as part of a broader survey design (D'Evelyn *et al.*, 2008; Ramsey *et al.*, 2009), but they can be measured more reliably by direct experimentation (Bulman *et al.*, 1999; Bulman, 2008; Christy *et al.*, 2010; Britton *et al.*, 2011; Moore *et al.*, 2011; Stringer *et al.*, 2011). In a detection experiment a range of survey conditions are simulated and replicated, and the effort required to successfully detect the target pest is measured.

In this chapter, we will outline basic detection models and provide guidelines for designing a detection experiment. We use two case studies – surveys for invasive hawkweeds in the Australian alps and serrated tussock in native Australian grasslands - to demonstrate analyses of detection data. We also summarise other methods available for estimating detection rates and probabilities when pest abundance, as well as presence/absence, affects management.

Detection at low pest densities

In many biosecurity circumstances, pests are likely to occur at low densities, if at all. These circumstances include using surveillance for proof of freedom, early detection, at the boundaries of delimitation and containment efforts, and in the later stages of a successful eradication programme (see Chapter 8, this volume). Survey designs must ensure there is an adequate probability of detecting the pest where it is present.

When a pest is considered unlikely to be present or present at a low density, it is reasonable to focus simply on pest presence or absence, rather than abundance or density. The first detection of the pest is often sufficient to trigger further action. We can model the relationship between pest detection and surveillance effort most simply as

$$(1) \quad \Pr(D|P) = p = 1 - (1 - d)^X$$

where D indicates that the pest is detected, P indicates that the pest is present at a specific distribution and abundance and p is the probability that the pest is detected given that it is present at that abundance (McArdle, 1990).

Here, surveillance effort X is measured in discrete units and the probability of detecting the present pest using one unit of surveillance effort is d . The single-unit detection probability d is specific to the nominated population abundance and in general, d increases with population size. The probability of detection using X units of surveillance effort is one minus the probability that all X units fail to detect the pest population. It can also be thought of as a sequence of X binomial surveillance trials with probability d that the pest population is detected in a single trial; the overall probability of detection is the probability that the pest is detected at least once. Discrete surveillance effort might be applicable when surveillance is conducted by making multiple visits to a site (Wintle *et al.*, 2005), laying out traps or baits or conducting multiple survey methods simultaneously (Barrett *et al.*, 2010).

Equation 1 assumes that the probability of detection from each unit of effort is equal and independent of the others (though this can be relaxed, see MacKenzie *et al.*, 2002; Rout *et al.* 2009; Chapter 5, this volume). It is common to additionally assume that no false positives occur; that is, any detection is a correct identification that the pest of interest is truly present.

Alternatively, surveillance effort x may be modelled continuously. This may apply when effort relates to the number of person hours (Garrard *et al.*, 2008; Moore *et al.*, 2011), trap hours (Britton *et al.*, 2011), trap density (Bogich *et al.*, 2008), or distance travelled in search (Ramsey *et al.*, 2009). The probability of detection can be expressed as

$$(2) \quad \Pr(D|P) = p = 1 - \exp(-\lambda x).$$

Equation 2 uses a Poisson assumption, and is one minus the probability that effort x fails to detect the pest. Parameter λ is the detection rate, and the mean effort required to detect the specified pest population is $1/\lambda$.

The Poisson assumption means that the detection process is random and that outcomes from one moment to the next are independent. Equation 2 can be adapted to accommodate variations on this assumption such as abundance and spatial clumping (McCarthy *et al.*, 2012), changing encounter rates (Gurarie and Ovaskainen, 2012) and dependence on other covariates (this chapter).

Estimating species detection rates experimentally

Imperfect detection acts as a filter between the true pest status and the pest surveillance data we collect. It can be difficult to disentangle the detection process from pest presence. For example, we may visit and survey many sites, some with sparse vegetation and others with dense vegetation. We find our target pest species in some sparsely vegetated sites but not in densely vegetated sites. Was this because densely vegetated sites are unsuitable for the species, or because the species is difficult to detect amongst dense vegetation? Supplementary information about habitat suitability and/or detection is needed.

A powerful method for estimating detection rates is independent controlled experimentation (Hauser *et al.*, 2012). This involves simulating surveillance procedures in an area where the pest (or a benign mimic) occurs. The experimenter can manipulate the underlying conditions, collect data on successful and unsuccessful detections and fit data to a detection model (such as 1 or 2 above). We expand upon the principles of experimental design discussed in Chapter 4, focussing on particular issues that should be considered when embarking on a detection experiment.

Identifying influential variables

The experimenter must first identify variables that are expected to influence detection rates. These may relate to the targets' characteristics (e.g. size, maturity), the surrounding environment (e.g. dominant vegetation or other visual obstacles, temperature) or the observer (e.g. a human searcher's level of training, a trap's size).

It can be useful to measure variables that are thought to influence detection in the experiment, even if they do not exist during real surveillance. For example, allowing many observers to visit the same plot may result in the trampling of vegetation or other clues leading to the easier detection of the pest. Including the number of visits previously made to the plot as a covariate should reveal whether detection rates increase. When the detection model is applied to real surveillance the rate assuming no prior visit can be used.

Any potentially influential variables should be controlled where possible and, at the very least, measured. Stratification, randomisation and replication will strengthen statistical

inference. However, some compromises may need to be made to ensure a detection experiment is practicable and sufficiently represents typical surveillance activities.

Site selection

The site of experimentation should represent the real surveillance sites as closely as possible. The site might be divided into homogeneous plots, each possessing a different combination of important environmental variables. For example, 16 sites might be selected to represent 4 different land uses with each land use represented in 4 replicate plots. Plot and site size should be selected to allow for the full range of typical surveillance activities, e.g. allowing human searchers to walk with their usual pace and gait, or capturing the full radius of a trap's potential attraction.

Targets

In order for a detection experiment to yield informative data, the pest must be detectable at the site. An experiment may involve opportunistic use of the target *in situ*, translocation of individuals from outside the study area, propagation of the species, or the use of benign mimics. The risks of using a pest species should be carefully assessed, with pest placement, recovery and disposal planned to maintain the risk of escape at an acceptable level.

Variation amongst individuals that might affect detection, such as size or colour, should be measured and, where possible, controlled and replicated. Ideally, the number and arrangement of targets will be controlled by the experimenter but this is not always feasible. The design and analysis of these two types of detection experiment – controlled target arrangement and uncertain target arrangement - will be discussed in more detail below.

Observers

Variation in observers should also be controlled and measured where possible, applying different observer types to the same site scenario. Full control of observer variation is probably most challenging when the observers are human searchers: it may not be possible or ethical to control the knowledge, behaviour or other characteristics of individual searchers, although most variables should be measurable. Often human searchers can be grouped in the design, e.g. by identifying highly trained and inexperienced participants.

The use of human observers requires other extra planning. In addition to issues of health, safety and ethics, humans are inclined to act strategically. They may act counter to their usual surveillance activities if there are incentives to do so. Strategies may include but not be limited to: noticing patterns in the experimental design (e.g. target abundance or placement, variable times allocated to tasks) and adjusting behavior to detect more targets, unusually high or low motivation to detect targets, and observing fellow participants' detection successes for accelerated detection during their own search. The design of the experiment and experience of searchers should be arranged to avoid such changes in behaviour wherever possible. This may conflict with idealised sampling designs, which typically replicate target abundances in a predictable pattern. The information conveyed to the searchers prior to and during the experiment should be carefully considered. For example, do searchers usually see a sample pest? Do they have expectations regarding target abundance?

Testing the design

A computer-based simulation analysis using realistic parameter values can help identify the replication of plots, targets and observers that is sufficient to detect the influence of measured variables. In a budget-constrained environment, trade-offs may exist, e.g. many observers searching few plots each vs few observers searching many plots each. The former will reveal more about the influence of observer variables on detection rates and less about the influence

of plot variables. In addition, where target presence is not controlled or known, more visits to fewer plots will be useful for estimating occupancy.

Data collection

Understanding detection rates is likely to require data collection additional to that made during typical surveillance procedures. Much site and target information can be recorded before surveillance is simulated. However, detailed information on detection events, such as the time and location, observer and target identity must be collected during simulated surveillance activities. Disruption to standard procedures should be minimised. Additional staff might be required to ‘observe the observers’ in real time and collect these important additional data.

Controlled target arrangement

Controlling the arrangement of targets, by managing the location of the pest *in situ* or by introducing targets to the site, offers the best opportunity for strong statistical inference. The abundance and arrangement of individual targets should mimic the range of circumstances that may be encountered during real surveillance procedures. These circumstances should be measured, randomised and replicated throughout the experiment where possible.

Replication can often be achieved by allocating targets at high abundance, and therefore providing observers with many opportunities to encounter the pest. However this approach may not be appropriate for pests that occur at low densities; target encounters might no longer be independent of each other, and human observers may alter their search behaviour. At the other extreme, allocating targets at very low densities will yield few detections and therefore few data from which to draw inference. A detection experiment must strike a balance between realistic encounters and adequate replication.

When the pest prefers specific local conditions, plots may need to be stratified by micro-habitat to ensure that targets are positioned realistically. Regardless of stratification, target position should be randomised amongst suitable locations.

When target arrangement is controlled, the pest is known to be present at each site that a model is fitted and its density is known. When X discrete units of survey effort are deployed at a site (equation 1) and the pest is detected by n of those units, then the likelihood function for our unknown detection probability d is

$$(3) \quad L(d \mid n, X) = d^n (1 - d)^{X-n}$$

The maximum likelihood estimate (MLE) for the detection rate is $\hat{d} = n/X$, that is the number of survey units detecting the pest divided by the total number of survey units.

When continuous survey effort x is applied at a site and n detections are made, we set $t_1, t_2, \dots, t_n$ to be the units of effort expended at each detection. The likelihood function for detection rate λ is

$$(4) \quad L(\lambda \mid t_1, t_2, \dots, t_n, x) = \begin{cases} \exp(-\lambda x), & n = 0 \\ \lambda^n \exp\left(-\lambda \sum_{i=1}^n t_i\right), & n > 0 \end{cases}$$

The MLE for the detection rate is $\hat{\lambda} = n / \sum_{i=1}^n t_i$; that is, the number of pest detections divided by the sum of effort required to detect each pest. This is equivalent to the inverse of the mean detection time.

More complicated likelihood functions are required when detection rates are linked to common covariates across sites, as in the first case study below.

Case study – estimating detection rates for hawkweed plants

We undertook a search experiment (Moore *et al.*, 2011) to estimate the detection rates for orange hawkweed (*Hieracium aurantiacum*), an invasive weed in Australia, New Zealand and the United States. We carried out our experiment on the recently invaded Bogong High Plains in north eastern Victoria, Australia. We planted individual rosettes in a 2 ha area of suitable habitat which was then searched by observers. Because we knew the locations of all the plants and recorded which individuals were detected by each observer we were able to calculate the number of times that each individual was detected or not detected, increasing the power of our study.

Study design

The trial took place in a 2 ha area. Search teams would typically be allocated areas on this spatial scale and conduct parallel line searches across it. We identified influential variables likely to affect the detection rate to be the number of rosettes in a single location (i.e. the group size), the surrounding vegetation type, and the experience level of the observer. We planted 70 potted greenhouse-grown Orange Hawkweed rosettes in 34 groups (4 groups of 5 plants, 10 groups of 3 plants and 20 individuals; Fig. 10.1). Plants were placed using stratified randomisation to ensure that they were spread relatively evenly over the trial area, but we did not stratify by vegetation type. The study area consisted of a mosaic of grassland, short open heath and tall dense heath, representing the range of the vegetation types encountered when surveying for new hawkweeds. The vegetation type surrounding the plant groups was recorded as grassland (8 plant groups), short open heath (17 plant groups) or tall dense heath (9 plant groups).



Figure 10.1. a) An example of the potted hawkweed rosettes that were used in the experiment and b) the layout of potted plants in the 2 ha trial area (black circles). Larger circles indicate larger groups of hawkweeds (1,3,5). Two observer tracks are also shown (dark and light grey lines) which illustrate the varying line search intensities used by different observers.

Twelve observers participated in the experiment. All observers were engaged in the management of hawkweed to some degree but their level and type of survey experience varied widely. To maximise replication and our ability to distinguish observer-level influences on detection, we requested that observers search in parallel lines across the plot as individuals instead of teams. Observers were taken to a starting location (which differed between

observers) and directed to search until they were requested to stop (approximately 1.5 hours). Start locations and start-times were staggered to minimise contact between observers, with a maximum of three observers searching simultaneously. Each observer was provided with a GPS (Garmin GPS60) which tracked their position every 15 seconds. Observers recorded the location of any plants found as a waypoint on the GPS. Observers were interviewed directly after their search. They indicated their level of experience and were also asked specify any cues that they used to find or identify the plants, ways in which the experiment differed from real surveys and any other relevant observations.

Statistical methods

We used a variation of equation 2 to estimate the detection rate λ_{ij} for each observer i and plant group j . We were unable to record time to detection for each plant group detected. Instead we calculated search effort as the average area searched per unit of time for each observer x_i and modelled each potential detection as a draw from the Bernoulli distribution with parameter p_{ij} , which is the probability of detecting the plant group given it was present (equation 2). We modelled $\ln(\lambda_{ij})$ as a linear function of influential factors:

$$(5) \quad \ln(\lambda_{ij}) = a + b_s(g_j) + b_v(h_j) + b_e(k_{ij}) + \text{obs}_i,$$

where a is the intercept term, $b_s(g_j)$ is the effect of plant group j being of size g_j , $b_v(h_j)$ is the effect of the plant group j occurring in vegetation type h_j , $b_e(k_i)$ is the effect of observer i having experience k_i , and obs_i is a random effect for observer i . The categorical variables $s(g_j)$, $v(h_j)$ and $x(k_i)$ were modelled using a reference class set arbitrarily to zero for plant group size 1, grassland and inexperienced observers.

We fit the model using Winbugs 1.4.2 (Lunn *et al.*, 2000). The model fit to each observer track i was described as:

```
log(lambda[i]) <- a + bv[veg[i]] + bs[size[i]] + be[exp[i]] + re_obs[obsID[i]]
p[i] <- 1 - exp(-lambda[i] * effort[i])
seen[i] ~ dbern(p[i])
```

To represent a lack of prior information, and to ensure that the parameter estimates were driven by the data, we used vague prior distributions for the intercept and linear predictors of the categorical effects ($\text{dnorm}(0, 0.000001)$) and the observer random effect ($\text{dunif}(0, 100)$). We fit a full model (three levels of vegetation type and group size, two levels for observer experience and a random effect for observer) and used DIC analysis to identify a minimum model (McCarthy, 2007). Parameter estimates are based on 200,000 samples after a 100,000 sample burn-in*.

Findings

Overall, each observer searched only a small proportion of the 2 ha experimental area, with only two observers covering more than half of the area in the allocated time. All planted hawkweed groups were encountered at least twice (median = 4, range 2-6) but the number of times that each group was detected was often less (median = 1, range 0-6). The number of plant groups encountered also varied considerably between observers from as few as 3 to a maximum of 23. Detection also varied widely, with the proportion of encountered groups detected ranging from 9-100%.

* WinBUGS uses a Markov chain to sample from the posterior distribution. Parameter estimates should only be estimated from a Markov chain that has converged to the posterior distribution, which may happen immediately or after many iterations. The 'burn-in' refers to the initial samples from the Markov chain prior to convergence.

The most substantial influence on detection in this experiment was the size of the plant group, specifically whether there was more than one individual in the group (Table 10.1, Model A). There was also weak evidence that vegetation type influenced detection rates, with groups in grassland vegetation slightly easier to detect than groups in heath vegetation (Table 10.1, Model B). We expected vegetation to have a larger effect and suspect that this was not observed because we did not stratify plant group location over vegetation type or ensure that observers divided their time between vegetation types. The openness of the vegetation was the most common factor cited by the observers as important in affecting the difficulty of the search. Other factors mentioned as increasing the difficulty of the search were slope, observer fatigue, bad weather, wet grass, walking direction relative to orientation of vegetation, and light levels or time of day.

Table 10.1. Parameter estimates for the two best DIC-ranked hawkweed detection models. Lower DIC values indicate a more parsimonious model. a is the intercept of the linear predictor, b_s is the effect of plant group size > 1 relative to a plant group size 1, b_v is the effect of shrubby surrounding vegetation relative to grassy surrounding vegetation, and sd_obs is the standard deviation of a Normally distributed random effect for each observer.

Parameter	Mean	2.5% CI	97.5% CI
Model A (DIC = 150.747)			
a	-1.959	-2.653	-1.348
b_s	1.087	0.4477	1.743
sd_obs	0.6376	0.09669	1.431
Model B (DIC = 151.388)			
a	-1.734	-2.621	-0.8564
b_s	1.066	0.4219	1.729
b_v	-0.3111	-1.081	0.4552
sd_obs	0.7104	0.15	1.547

We are confident that the planting process did not produce sufficient disturbance to act as a cue for detection as none of the observers indicated that they used signs of disturbance as a cue to find the plants. However, it was noted that the plants were larger and healthier than those observed in the region because they were grown in a green house. Observers indicated that while this probably didn't make them easier to locate, it did make plants easier to positively identify once noticed. Since there are a number of other species in the area (e.g. *Picris hieracioides*, *Hypochaeris radicata*, *Taraxacum* spp, *Microseris lanceolata*) that are similar to *Hieracium aurantiacum* rosettes, the usual need to check each rosette in detail would likely increase actual search time. Hence, it is likely that this experiment overestimated detection rates compared to rosettes grown in field conditions.

Estimates of detection rate can be used to plan surveillance and to retrospectively assess the probability that areas surveyed still contain undetected individuals. The estimates from the experiment described here (plus additional unpublished work) have been used by managers of the hawkweed eradication program to plan alpine hawkweed surveys using an optimal surveillance methodology (Hauser and McCarthy, 2009).

Uncertain target arrangement

While it is ideal to experimentally control occupancy, density and arrangement of the target species when estimating detection, detection probability may be estimated using targets *in situ*. This scenario poses some particular challenges, the most obvious of which is that pest presence/occupancy may not be known. This situation can be addressed by adding a new parameter Ψ to the simple binomial and exponential models described above (equations 1 &

2). Ψ is the probability that the target pest is present at the site, and can be modelled simultaneously with detection probability (MacKenzie *et al.*, 2002; Tyre *et al.*, 2003; Wintle *et al.*, 2005) or detection rate (Garrard *et al.*, 2008).

Then equation 1 becomes

$$(6) \quad L(d | y_1, y_2, \dots, y_x) = \begin{cases} 1 - \Psi + (1-d)^n \Psi, & n = 0 \\ d^n (1-d)^{x-n} \Psi, & n > 0 \end{cases}$$

which is known as a zero-inflated binomial model (Wintle *et al.*, 2005).

Equation 2 becomes

$$(7) \quad L(\lambda | t_1, t_2, \dots, t_n, x) = \begin{cases} 1 - \Psi + e^{-\lambda x} \Psi, & n = 0 \\ \lambda^n \Psi \exp\left(-\lambda \sum_{i=1}^n t_i\right), & n > 0 \end{cases}$$

So, for each model we now have two probability statements. The first describes the situation in which the species is not detected in any survey ($n=0$); the second describes the probability of detections and non-detections for multiple surveys where the species is detected ($n>0$). In the former case, the model allows for two possibilities: that the species was present and it escaped detection in every instance; and that the species was truly absent from the site.

These models have a number of assumptions. Individual visits or surveys are assumed to be independent, observer-target encounters are assumed to be independent and random, and targets are assumed to occur in equal densities across occupied sites (although density covariates can also be included in the model). In practice, it will be difficult or even impossible to demonstrate that assumptions about target distribution and density are met, and this may have implications for detection rate estimates. It is also assumed that the target population does not change over the duration of the study, which means these methods may not be applicable to relatively short-lived targets such as some insect pests.

Occupancy models are typically replicated across multiple scenarios that are linked via common covariates, as in the case study below.

Case study – estimating detection rates for serrated tussock in Australian native grasslands

Serrated tussock (*Nassella trichotoma*) is considered one of the worst weeds in Australia because of its invasiveness, potential to spread and economic and environmental impacts (CRC for Australian Weed Management, 2003). It is a weed of concern in many places outside its native range, including New Zealand, South Africa, northern America, parts of Europe, and Australia, where it is listed as a Weed of National Significance (Weeds Australia, 2008). This species, along with other exotic perennial grasses, is regarded as a significant threat to agriculture and remnant native grassland communities in south eastern Australia (Department of Sustainability and Environment, 2003).

In this case study, the detection rate for serrated tussock in native grassland communities was estimated in a field trial in which the presence or absence of the species was unknown (Garrard *et al.*, 2009). Detection of this species in native grasslands is thought to be hampered by the fact that, when it is not flowering, the species is visually similar to a number of native tussock grasses, such as *Poa* species.

Study design

Time-to-detection data were collected during a multi-site, multi-observer field study undertaken in consecutive Spring seasons in 2006 and 2007. Multiple observers conducted flora surveys in 16 one-hectare plots in native grasslands to the west and north of Melbourne, Australia. Surveys were 90 minutes in duration, during which time observers were asked to

record the time at which they first saw each new species[†]. Observers searched each site in one of two ways: systematic searches were those where the observer followed a repeated pattern when covering the full hectare, while observers conducting an unsystematic search were able to roam within the site as they pleased, using prior knowledge and intuition to determine their search path. In both cases, observers were instructed to cover as much of the site as possible during the 90 minutes allocated for the survey. Starting points for each observer were randomised around the perimeter of the site to avoid biases towards plants in specific locations, and the number of observers surveying each site at any one time limited to two to avoid “copy cat” detections.

Statistical methods

We fit an exponential time-to-detection model, which assumes that the rate of detection, λ , is constant, and allows for uncertain site occupancy, Ψ (equation 7). We modelled $\ln(\lambda)$ as a linear function of explanatory variables. (In this model, $1/\lambda$ is equal to the average time to detection). Influential factors considered were observer experience (whether or not the observer had specific experience in grassland surveys), time of day (morning, midday or afternoon), weather conditions (sunny, sunny with cloudy periods, overcast or raining), percentage cover of the dominant grass, *Themeda triandra*, and the date of survey (reported as days since October 1st: chosen because it coincides with the beginning of the peak survey period for native grasslands). Due to data restrictions, we chose not to include random effects for site or observer, however these could be accommodated in the linear predictor as described in the previous example. Models were run in WinBUGs 1.4.2 (Lunn et al. 2000).

The WinBUGS code divides the likelihood into components depending on whether the species was detected by any observers at a given site, in which case we know it is present, or whether it was not detected by any observers, in which case it may be present or absent. This is done by using two indicator functions: $dd[i]$ is 1 if the species is found during survey i and zero otherwise, $d[site]$ is 1 if the species was detected at least once at $site$ and zero otherwise.

```
for(site in 1:16){
  for(i in 1:visitors[site]) {
    dd[i] <- step(50-x[i])
  }
  d[site] <- step(sum(dd[1:visitors[site]]) - 0.5)
}
```

where $visitors[site]$ is the number of observers who surveyed $site$. In surveys where the species is detected, $x[i]$ is set to zero, $50-x[i]$ is positive using the indicator function $step$, and $dd[i]$ is set to 1. When the species is not detected, $x[i]$ is the total time spent searching $site$ without detection – 90 minutes in this case – and so $50-x[i]$ is negative and $dd[i]$ is set to 0.

The likelihood function for each site, $L[site]$, uses the indicator variable $d[site]$ to select from the two forms in equation 7. Note that $t[i]$ is the time at which an observer successfully detects the species, and $x[i]$ is the total time spent searching $site$ if the species is not detected; in other words, the duration of the survey. The probability of occurrence is ψ , which is assumed to be the same across all sites.

```
for(site in 1:16){
  # Likelihood at each site
  L[site] <- (1-d[site]) * (1 - psi + psi*prod(pn[1:visitors[site]]))
    + d[site] * psi * prod(pp[1:visitors[site]])
  for(i in 1:visitors[site])) {
```

[†] This study was designed to simulate impact assessment surveys of a site, and required a comprehensive species list at each site. Detection rates for a single species are likely to be higher in a targeted search.

```

# Likelihood if seen at the site (by anyone) and by the observer
pp1[i] <- lambda[i]*exp(-lambda[i]*t[i])

# Likelihood if seen at the site (by anyone), but not by this observer
pp2[i] <- exp(-lambda[i]*x[i])

# Selects likelihood if seen at this site (by anyone)
pp[i] <- dd[i]*pp1[i] + (1-dd[i])*pp2[i]

# Likelihood if not seen (by anyone)
pn[i] <- exp(-lambda[i]*x[i])

# Link detection rate lambda to covariates
log(lambda[i]) <- alpha + exper[obsr[i]] + search[method[i]] + beta*days[i]
               + yr[year[i]]
}
}

```

We specified vague prior distributions for Ψ ($\text{dunif}(0,1)$), and the intercept and variable coefficients of the linear predictor ($\text{dnorm}(0,0.0001)$) to ensure that the posterior estimates were driven by the data. We fit a full model and used DIC analysis (McCarthy, 2007) to identify a minimum model. Parameter estimates are based on 100,000 samples after discarding a burn-in of 10,000 samples.

Findings

Serrated tussock was detected at 15 of the 16 sites surveyed. The naïve-high estimate of detection probability (assuming the species was truly absent from the site where it was not detected (Wintle et al., 2004) was 0.65, indicating that the species remained undetected during 35% of surveys undertaken at sites where it was known to be present.

Multiple models were compared using DIC. Observer experience, search method, date, *T. triandra* cover and year were all included in models with a DIC value within 2 units of the DIC-best model (Table 10.2). Detection rate was higher for experienced observers, non-systematic searches and at later dates in the survey period. Detection rate was lower in 2007 than in 2006, and there is some evidence that detection rate was negatively affected by increasing cover of *T. triandra*, however this effect is less certain (Fig. 10.2).

Table 10.2. Candidate detection models and DIC rankings for *N. trichotoma* in native grassland communities. Lower DIC values indicate a more parsimonious model. There is little support for models with a DIC value more than 2 units greater than the best model. α is the intercept of the linear predictor, exper is experienced observer, search is a non-systematic search pattern, date is the date of survey, year is year of survey, %themedra is the cover of *T. triandra*, and time of day is the time at which survey began.

Model	ΔDIC
A. $\ln(\lambda) = \alpha + \text{exper} + \text{search} + \text{date} + \text{year}$	0
B. $\ln(\lambda) = \alpha + \text{exper} + \text{search} + \text{date} + \text{year} + \%\text{themedra}$	1.08
C. $\ln(\lambda) = \alpha + \text{exper} + \text{search} + \text{date}$	1.68
D. $\ln(\lambda) = \alpha + \text{search} + \text{date} + \text{year}$	2.99
E. $\ln(\lambda) = \alpha + \text{exper} + \text{search} + \text{date} + \text{year} + \text{time of day}$	3.33
F. $\ln(\lambda) = \alpha + \text{exper} + \text{date} + \text{year}$	4.00
G. $\ln(\lambda) = \alpha + \text{exper} + \text{search} + \text{year}$	7.74

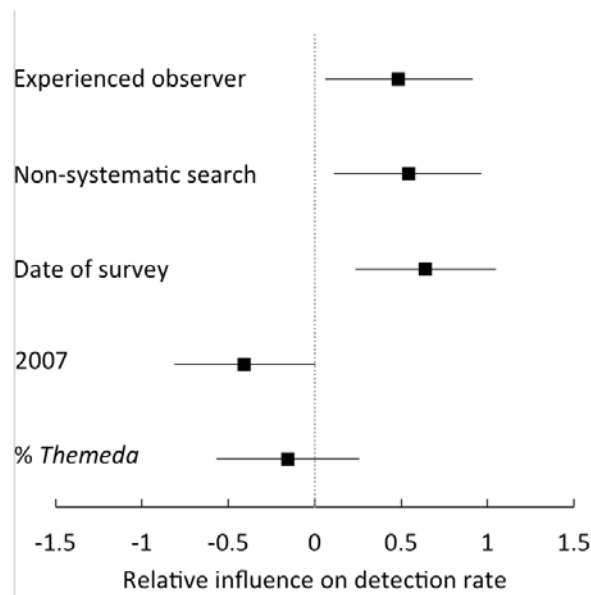


Figure 10.2. Relative size of the influence of explanatory variables on detection rate of *N. trichotoma* in native grasslands. Points and error bars represent the mean and 95% credible intervals of the posterior distributions. Estimates are taken from 100,000 samples after discarding a burn-in of 10,000 samples.

Using the DIC-preferred model (Model A), the average detection rate under favourable conditions (experienced observer, non-systematic survey, late in the 2006 survey season) is 0.030 plants per minute (1.82 plants per hour). This is equivalent to an average time to detection of 33 minutes per hectare. Average detection time increases to 53 minutes for less experienced observers and 57 minutes when a systematic search pattern is used.

Equation 2 can be used to estimate the probability of detecting *N. trichotoma* at a site where it is present after a given survey effort. Under the favourable survey conditions described above, the probability of detecting *N. trichotoma* in a 60-minute survey of a site where it is present is 0.84.

Estimating detection and abundance

Numerous methods exist for estimating the probability of detecting an individual in a population and correcting for bias in abundance estimates. We give a brief description of the most commonly used methods, and key references are provided for those readers who wish to explore this area further. Mark-recapture (Otis *et al.*, 1978; Nichols, 1992) and *N*-mixture models (Royle, 2004; Joseph *et al.*, 2009) simultaneously estimate population size and the detection probability of individuals within the population from observed count data. These methods require temporally- (mark-recapture) and spatially- (*N*-mixture) replicated sampling events. These methods can accommodate variation in detection rates among individuals (both methods), sampling events (mark-recapture) and sites (*N*-mixture). Mark-recapture models are extremely popular for monitoring animal populations, and can provide estimates of additional parameters including survival, recruitment and growth rate. They can be implemented in the program MARK (White and Burnham, 1999). To date, application of these methods to plant populations has been limited (but see, for example, Kéry and Gregg, 2003).

Distance sampling enables the estimation of population density in a known area from detection data characterised by distances from a sampling location (Buckland *et al.*, 2001). Sampling generally takes place from a point or line transect. A key feature of these methods is the recognition that the probability of detecting individuals declines with distance. A detection function is used to describe the rate at which detection probability declines with distance. Fitting this detection function from distance data may be completed in program DISTANCE

(Thomas *et al.*, 2010). In the case of insect trapping, the decline in capture probability with distance can be summarised in terms of the “effective sampling area” of a trap (Turchin and Odendaal, 1996) which can then be used to estimate the probability of target detection within a trapping grid.

When applied to pest species, it is often beneficial to remove individuals as they are detected. Mark-recapture estimation methods and catch-per-unit-effort methods from fisheries literature can be usefully adapted to estimate detection probability as the pest is removed and simultaneously estimate abundance (D’Evelyn *et al.*, 2008; Ramsey *et al.*, 2009).

Conclusion

Determining detection rates or probabilities for a population under the full range of potential survey conditions is not trivial. Detection experiments that are designed and controlled independently of standard pest surveys offer the greatest potential for quality estimation. In this chapter we have discussed how such experiments can be designed, implemented and analysed. This approach cannot be realistically implemented for all pest species. Linking detection rates to species traits (Garrard *et al.*, 2012) offers some potential for predicting detection rates for other related species.

Methods from other environmental management disciplines such as occupancy modelling, mark-recapture, distance sampling and catch-effort modelling can be adapted to estimate detection probability during an independent detection experiment or in tandem with standard surveillance. We have briefly reviewed these methods and direct the reader to cited references for further detail.

Detection estimates indicate not just what surveillance has detected, but what may continue to lie undetected. They are therefore vital for informing surveillance design at all stages of pest management.

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