

An estimator of wildlife fatality from observed carcasses

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Counts of animal carcasses are often used to estimate fatality caused by disease, environmental accidents (oil spills, radiation leaks), or human structures (power lines, sky scrapers, wind turbines). The need to adjust raw carcass counts for imperfect detectability to produce unbiased estimates of fatality has long been recognized, but the accuracy and precision of some estimators used to make the adjustments have not been evaluated. In this paper, I formalize a conceptual model of fatality and the factors that lead to imperfect detection, primarily removal by scavengers before searches can be carried out and inability of searchers to find all remaining carcasses. I propose an estimator of fatality that adjusts for imperfect detectability. Through simulation I evaluate the statistical properties (bias and precision) of this estimator and two others commonly used to estimate fatality at wind power facilities, when sources and magnitudes of imperfect detectability vary. None of the estimators was always unbiased under all conditions. Bias in the proposed estimator never exceeded $\pm 27\%$ whereas bias in the other two estimators was always negative and exceeded that of the proposed estimator in 98% and 93% of the simulated conditions, respectively. The proposed estimator was relatively robust to variation in sources and magnitudes of imperfect detectability, but was sensitive to distributional assumptions regarding carcass removal rates and searcher efficiency. It offers significant improvement over two current estimators and provides relatively unbiased estimates of fatality that can be applied under a variety of conditions and survey protocols. Copyright © 2010 John Wiley & Sons, Ltd.

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1. INTRODUCTION

The problem of estimating numbers of fatalities over an extended period of time is fundamentally one of estimating abundance of a wildlife population (Seber, 1982), albeit a morbid one. Members of the morbid population are imperfectly detected due to any of several possible detection biases: (1) removal by scavengers; (2) imperfect detection by human or canine searchers; (3) site- and carcass-specific covariates that may influence the first two, such as vegetation height, type and density, carcass coloration and size, or microtopography (Wobeser and Wobeser, 1992; Philibert *et al.*, 1993); and (4) fatalities that land outside or injured animals that move outside search plots (Gauthreaux, 1995; Arnett *et al.*, 2008). Carcass searches have been used to provide initial estimates of avian fatality caused by disease (Ward *et al.*, 2006), pesticides (Balcomb, 1986), high-voltage power lines (Lehman *et al.*, 2007), and collisions with stationary as well as moving objects, *e.g.*, cars (Antworth *et al.*, 2005) or wind turbines (Osborn *et al.*, 2000). However, the adequacy of simple carcass counts to reflect actual fatality is influenced by the rate at which carcasses decompose or are removed by scavengers, the time interval between occurrence of fatality and the search, and the accuracy and precision of the search method (Wobeser and Wobeser, 1992). Large differences in detectability of carcasses have been recorded between species and among vegetation types (Philibert *et al.*, 1993). Difference in detectability may bias estimates of relative abundance of species that are not equally conspicuous in mixed species die-offs or fatality events (Philibert *et al.*, 1993).

High numbers of fatalities of raptors (Orloff and Flannery, 1992; Erickson *et al.*, 2001; Erickson *et al.*, 2002) and bats (Fiedler, 2004; Johnson, 2005; Arnett *et al.*, 2007; Kunz *et al.*, 2007) at some wind power facilities have raised concerns regarding the potential environmental impact of this rapidly expanding industry. In response, voluntary post-construction fatality monitoring at wind power generation facilities is now recommended in permitting guidelines developed by several states, and in at least two states, legislation has been proposed to make the guidelines mandatory (Stemler, 2007). The intent of the monitoring is to estimate the total number of fatalities occurring at a site over a specified period of time, usually a year or a migration season.

Fatality monitoring has been conducted at wind power facilities since the late 1980s (Orloff and Flannery, 1992). Typically, searchers traverse the area under a turbine recording the number of carcasses found that show signs of having been killed by the turbine. The need to adjust simple carcass counts for imperfect detectability has long been recognized (Anderson *et al.*, 1999; Morrison, 2002). Detection probabilities, in turn, must be estimated and the accuracy and precision of these estimates have strong impact on the accuracy and precision of the final fatality estimates. The first two primary sources of imperfect detectability, carcass removal and lack of detection by observers, are

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often estimated in independent trials conducted at the monitoring sites. The average number of days a carcass is expected to persist unscavenged is estimated by placing trial carcasses under turbines and monitoring their continued presence for a specified period of time, then calculating the average persistence time. The probability of a searcher observing a carcass given that it has not been scavenged is estimated by placing trial carcasses under turbines and calculating the average proportion of carcasses found.

While the general methods for conducting fatality searches and estimating parameters necessary to adjust for reduced detection rates are well established (e.g., Anderson *et al.*, 1999; Morrison, 2002), the actual estimator of fatality based on these parameters is not. In addition, the sampling protocol dictating the interval between searches and the number of carcasses to be used in independent trials to estimate detection rates are not well established and are inconsistent among studies. Searches for carcasses are conducted at intervals varying from 1 to >28 days (Arnett *et al.*, 2008). The number of trial carcasses used to estimate detection rates ranges from 6 (or fewer) to over 200 (Arnett *et al.*, 2008). From these, estimates of average carcass persistence (CP) time range between 2 days (Fiedler *et al.*, 2007) and 52 days (Tierney, 2007) and the probability of a searcher actually observing a carcass ranges between 13% (Schnell *et al.*, 2007) and 88% (Erickson *et al.*, 2000). Estimators of fatality that correct for imperfect detection vary widely among studies (e.g., Erickson *et al.*, 2000; Erickson *et al.*, 2003, 2004; Johnson *et al.*, 2003b; Jain *et al.*, 2007) and there currently appears to be no consensus on which to use. Conceptual models underlying these estimators have not been given in any publicly available documents and definitions of parameters vary among studies making it difficult to compare estimators. The statistical properties of only one fatality estimator have been formally investigated and it was found to produce biased estimates (Barnard, 2000). I am not aware of any other published evaluations of the statistical properties of any estimators currently used in fatality monitoring.

My objectives in this paper are to (1) formalize the conceptual model of fatality and define relevant parameters to provide a unifying framework for discussion; (2) develop an estimator of fatality when probability of detection <1; (3) evaluate the bias and precision of the estimator using simulation and examine its sensitivity to magnitude of parameters as well as to assumptions regarding distributions of parameters; (4) compare the bias and precision of this estimator with two other estimators in common use.

In this paper, I first develop the conceptual model of fatality and sources of imperfect detection and define relevant parameters. I develop this model for wind power facilities in particular, but it can easily be generalized to apply to other situations, e.g., high tension power lines or contaminated areas. I propose an estimator of the total morbid population at a site based on that of Thompson (1992) for unequal probability sampling of groups with unequal detection probabilities. I describe the proposed estimator and two others in current use in terms of the parameters in the conceptual model and I evaluate the bias and precision of the three estimators through simulation. I simulate the actual fatality, carcass removal, imperfect detection by searchers, and the search process itself to produce the carcass count resulting from searches at different intervals. I simulate carcass removal and imperfect detection using different assumptions to determine the sensitivity of the estimators to the assumptions inherent to the simulation. I apply the three estimators to the simulated carcass count to estimate fatality and compare the accuracy and precision of the estimators under different conditions. Finally, I compare the three estimators using published data from two monitoring projects.

2. CONCEPTUAL MODEL OF FATALITY

Fatality monitoring at wind power facilities represents one of many cases in which the abundance of an animal (number of carcasses) is incompletely observed due to imperfect detectability during searches (Thompson, 1992, 1994). Carcass detectability is <1 because a proportion of fatalities are removed by scavengers or obscured from view by vegetation or topography. Thus, the total number of carcasses observed (C) can be expressed as the product of the actual number killed (F), the proportion of carcasses that persist unscavenged (r), and the proportion of the those carcasses that an observer actually sees (p), $C = Frp$. r is dependent on the amount of time elapsed between an animal's death and the time of the search, as well as other potential covariates, such as carcass size or time of year. p is a function of several potential covariates such as size, color, and surrounding vegetation (Morrison, 2002). Thus, the total number of carcasses counted at a turbine at the end of any particular interval will be a non-linear function of the actual number of animals killed, the length of the interval, the proportion of carcasses persisting to the end of the interval, and the proportion of these remaining carcasses that are detected by searchers. If there were a direct (linear) relationship between the number of observed carcasses and the number of animals that were killed, there would be no need to develop an estimator that adjusts observed counts for imperfect detectability; observed counts could be used as a simple index of fatality. But the relationship is not direct and counts recorded using different search intervals, in areas with different carcass removal rates and searcher efficiency rates are not directly comparable. In addition, the density of carcasses of some species has been shown to decrease with increase in distance from the turbine, yet there can be significant portions of a designated search plot that are inaccessible to searchers (Arnett *et al.*, 2009a). The configuration of the searchable area of a plot surrounding a turbine can vary among turbines and determines the proportion of the actual fatality that can possibly be detected.

2.1. Estimator of fatality

If not all designated search areas are equally searchable, their differences can be represented by unequal probability sampling weights, and if not all carcasses are equally detectable, their differences can be represented by unequal detection probabilities. Using this concept, the following is taken directly from the estimator described by Thompson (1992) to estimate animal abundance from unequal probability sampling of groups with unequal detection probabilities, but adapted to reflect the current context. We are interested in estimating the total fatality, F , that has occurred over a period of time, T , at a wind power generation facility with N turbines, from the carcasses observed during searches conducted at a random sample of n turbines. For any arbitrary turbine i , we can subdivide the time period into S_i consecutive

intervals of length I_{ij} , such that $\sum_{j=1}^{S_i} I_{ij} = T_i = T$ for all turbines, where I_{ij} is the number of days in the j th interval at turbine i and S_i is the total number of intervals at turbine i . Let F_{ij} denote the total number of fatalities at the i th turbine in the j th interval. These can be grouped into K_{ij}

sets, e.g., carcasses of similar size and/or in similar habitat, for which the detection probability is the same for all carcasses in the set and represented by a variable, y_{ijk} , the number of fatalities in the k th group with probability of detection, g_{ijk} . Then the total number of fatalities at turbine i , $F_i = \sum_{j=1}^{S_i} F_{ij} = \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} y_{ijk}$, and $F = \sum_{i=1}^N \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} y_{ijk}$. If the probability of detection of a carcass, g_{ijk} is known, an unbiased estimator of

F , based on the Horvitz–Thompson estimator, is $\hat{F} = \sum_{i=1}^n \frac{1}{\pi_i} \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} \frac{c_{ijk}}{g_{ijk}}$, where c_{ijk} is the number of the y_{ijk} actually observed, and π_i is a known modified weight representing an unequal probability sample. Because a search plot is of limited size, it is likely that it will not contain all of the actual fatality occurring at turbine i , but only a fraction. So π_i is the product of the proportion of the actual fatality at turbine i that is contained in the searchable area of the plot and the probability of including turbine i in the sample. While the probability of including turbine i in the sample is often known, both the proportion of the actual fatality at turbine i that is contained in the search plot and the probability of detection of a carcass are usually not known, but must be estimated. The estimator $\hat{F} = \sum_{i=1}^n \frac{1}{\pi_i} \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} \frac{c_{ijk}}{g_{ijk}}$ when g_{ijk} and π_i are estimated, is no longer unbiased for F , although it might be approximately so (Thompson, 1992). While Thompson (1992) provides an estimator for the variance of \hat{F} when g_{ijk} and π_i are known, a bootstrapped estimate of the variance can provide an easily implemented solution when these parameters are estimated (Efron and Tibshirani, 1986).

2.2. Estimators of detection probability

In practice, g_{ijk} is unknown and is potentially unique to each carcass and search interval. It can be modeled as the product of at least two probabilities, r and p , each of which is a function of carcass-specific characteristics such as size, color, surrounding vegetation, and/or time of year. Subscripts ijk are dropped from the following for clarity without loss of generality.

Probability of persisting, r: the probability that an animal killed at a turbine persists unscavenged and observable until the next search, depends on the removal rate by scavengers and its time of death relative to the next search. Many statistical models of survival time distributions are available (Klein and Moeschberger, 2003). The simplest persistence time model is the exponential distribution, with estimated probability that a carcass will persist for d days, $\hat{r} = e^{-d/\hat{\tau}}$, where $\hat{\tau}$ is the estimated mean persistence time. As noted above, the time of death of an animal found during a search is unknown, so d is unknown and r cannot be calculated exactly for each carcass. However, assuming that animals enter the morbid population at a constant rate during the interval and persistence time follows an exponential

distribution, the average probability of persistence of a carcass can be estimated as $\hat{r} = \frac{\int_{x=0}^I e^{-x/\hat{\tau}} dx}{I} = \frac{\hat{\tau}(1 - e^{-I/\hat{\tau}})}{I}$ and applied to all animals found at the end of the interval of length I . It should be noted that this average probability of persistence is not equivalent to the probability of persistence at the average point in the interval, $\hat{r} = e^{-I/2\hat{\tau}}$, and is generally higher.

Probability of being observed given persistence, p: the probability that a carcass that has remained unscavenged will be observed by a searcher, p , depends on its individual characteristics such as size and/or color as well as the density of the surrounding vegetation in which it died. A simple model of observability, often referred to as searcher efficiency, is the binomial with \hat{p} = number observed/number available.

Effective search interval, v: in addition to r and p , I suggest that there is a third factor affecting the probability that a carcass is detected. When the interval between searches greatly exceeds the amount of time a carcass could be expected to persist, the average probability of persisting through the interval approaches 0. Estimating r for all animals as an average probability of persistence through the entire interval will result in small values of \hat{r} , inflating the consequent estimates of F . I define the *effective search interval* \tilde{I} , as the length of time beyond which the probability of a carcass persisting is $\leq 1\%$. Any carcass observed at the end of a search interval will almost surely have died less than \tilde{I} days prior, so estimates of fatality derived from these carcasses are representative only of fatalities over an interval of length \tilde{I} , not I . Assuming the deaths during the last \tilde{I} days in the interval are representative of deaths throughout the interval, the probability of detection must include the effective proportion of the interval actually sampled, $\hat{v} = \min(1, \tilde{I}/I)$, to estimate fatality for the entire interval. Again, if we assume that the exponential or failure time distribution is a reasonable model for time to removal, then \tilde{I} can be estimated as $\hat{\tilde{I}} = -\log(0.01) * \hat{\tau}$ and applied to all animals in the interval to which $\hat{\tau}$ applies.

Probability of detection, g: an estimate of the probability of detection of a carcass is then $\hat{g} = \hat{p}\hat{v}$, where \hat{r} is calculated using $\min(\hat{\tilde{I}}, I)$.

3. METHODS

3.1. Simulations

The simulation included four major components: (1) simulate the fatality process; (2) simulate the CP process using three distributional assumptions; (3) simulate the search process, using six search intervals; (4) simulate the observation process using two distributional assumptions to produce simulated carcass counts. The details of the simulation can be provided upon request.

Because activity patterns of bats and birds appear to be quite erratic, I used actual recorded bat echolocation calls in my simulation to reflect this pattern, rather than generating fatality data from a statistical process. Echolocation data recorded at 15 proposed turbine locations in Casselman, Pennsylvania in 2005 (Arnett et al., 2006) formed the basis for my simulation to reflect temporal (night to night) and seasonal variation in bat activity. The final activity dataset comprised 17 811 total calls representing a fixed number of calls at each of 15 turbines on each of 112 nights and was used as the basis for generating fatality data. By generating fatality as a function of activity, I preserve the inherent

variation and temporal autocorrelation in the fatality data and avoid potential biases that could be introduced by assuming a particular fatality process.

Fatality was simulated by imposing an average fatality rate of 10% (generated as a random draw from a Beta(50 450)) to each turbine on each night. The simulated fatalities were summed to give the total “known” fatalities at the site. Persistence of each of these “carcasses” was simulated using three random distributions ($1/2$ Normal, Exponential, and Gamma) each with seven different means ($\bar{t} = 1, 2, 4, 8, 16, 32,$ and 64 day). The exponential distribution represented a constant carcass removal rate, and has been suggested as a reasonable model in some fatality monitoring studies (Erickson *et al.*, 2004). The half-Normal represents an initially reduced rate of removal, followed by a more rapid rate as time since death increases. The Gamma complements this distribution by representing an initially rapid removal rate, followed by a reduced rate as time since death increases. While the shapes of the distributions differed the means of the distributions were set at the given rates, above, for comparability.

The search process was simulated using six different search intervals ($I = 1, 2, 4, 7, 14,$ and 28 days). Searcher efficiency was simulated using two random distributions (simple binomial and conditional binomial) using three different rates ($p = 0.2, 0.5,$ and 0.8). The simple binomial distribution represented the assumption that a carcass that was not seen on the first search would not be seen on any subsequent searches. The conditional binomial represented the assumption that a carcass that was missed on a search but persisted unscavenged would have some probability of being observed on a subsequent search, but that probability would diminish in time. The probability of a searcher observing a carcass that was present after s searches was modeled as $p * 0.25^{(s-1)}$. Prior studies (Arnett, 2006; Arnett *et al.*, 2009a) have shown that in anything but the most uniform and unvegetated habitat, there can be a significant proportion of carcasses that will go unobserved, no matter how many searches occur.

Each simulation of known total fatality resulted in $756 (= 7 \times 3 \times 6 \times 3 \times 2)$ simulated values of the number of carcasses counted. Finally, each of three estimators was applied to the 756 simulated carcass counts. All three of the estimators compared in this study are functions of four parameters: $c, \bar{t}, p,$ and I . In this simulation, $\bar{t}, p,$ and I were constant for all carcasses allowing estimates of total fatality to be calculated from total observed carcass counts.

3.2. Estimators compared in this study

Two estimators have been commonly used in fatality monitoring studies at wind power generation facilities. The naïve estimator was first published by Johnson *et al.* (2003a), but informally adopted for use in several earlier studies (*e.g.*, Erickson *et al.*, 2000; Johnson *et al.*, 2000) and continues to be used in monitoring studies (*e.g.*, Fiedler *et al.*, 2007; Miller, 2008). No conceptual basis for the naïve estimator has been given in any of the published reports and it is not clear what assumptions regarding fatality rates, persistence rates or conditional detectability are inherent to it.

The modified estimator was developed by P. Schoenfeld (West Virginia Highlands Conservancy, unpublished report 2004) and first used by Kerns and Kerlinger (2004). This estimator was developed as an attempt to correct what was calculated to be a 15–20% underestimate of fatality using the naïve estimator. It assumes that fatalities, carcass removal, and even search intervals are all Poisson processes. In addition it assumes that all birds killed are eventually either found (and removed) by researchers or removed by scavengers. This estimator is a modification of the naïve estimator, but includes an additional term relating to persistence and observability.

The estimator I propose and the two commonly used estimators are given below, using notation defined earlier and $C = \sum_{i=1}^n \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} c_{ijk}$:

$$\text{Proposed estimator } \hat{F} = \sum_{i=1}^n \frac{1}{\bar{t}_i} \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} \frac{c_{ijk}}{\hat{r}_{ijk} \hat{p}_{ijk} \hat{v}_{ijk}} \text{ where } \hat{r}_{ijk} = \frac{\hat{t}_{ijk} (1 - e^{-\min(\hat{t}_{ijk}, I_{ijk}) / \hat{t}_{ijk}})}{\min(\hat{t}_{ijk}, I_{ijk})}, \hat{I}_{ijk} = -\log(0.01) * \hat{t}_{ijk}, \text{ and } \hat{v}_{ijk} = \min\left(1, \frac{\hat{t}_{ijk}}{I_{ijk}}\right).$$

Naïve estimator $\hat{F} = \frac{NIC}{\bar{t} \bar{p}}$ where $\bar{t} = \frac{1}{n} \sum_{i=1}^n \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} \frac{c_{ijk}}{\hat{t}_{ijk} \hat{p}_{ijk}}$. In practice, it is often assumed that $\hat{t}_{ijk} = \hat{t}_{i'j'k'}, \hat{p}_{ijk} = \hat{p}_{i'j'k'}$, and $I_{ijk} = I_{i'j'k'}$, so this estimator can be written as $\hat{F} = \frac{NIC}{\bar{t} \bar{p}}$.

Modified estimator $\hat{F} = \frac{NIC}{\bar{t} \bar{p}} \sum_{i=1}^n \sum_{j=1}^{S_i} \sum_{k=1}^{K_{ij}} \frac{c_{ijk} I_{ijk}}{\hat{t}_{ijk} \hat{p}_{ijk}} \left(\frac{e^{I_{ijk} / \hat{t}_{ijk} - 1 + p}}{e^{I_{ijk} / \hat{t}_{ijk} - 1}} \right)$. In practice, it is often assumed that $\hat{t}_{ijk} = \hat{t}_{i'j'k'}, \hat{p}_{ijk} = \hat{p}_{i'j'k'}$, and $I_{ijk} = I_{i'j'k'}$, so this estimator can be written as $\hat{F} = \frac{NIC}{\bar{t} \bar{p}} \left(\frac{e^{\bar{t} - 1 + p}}{e^{\bar{t} - 1}} \right)$.

3.3. Comparison of estimators

A total of 1000 fatality scenarios were simulated, each generating a known total fatality as well as 756 different carcass counts generated using different values of the parameters and distributional assumptions. The three estimators of fatality were applied to each of the counts in each of the 1000 known fatality simulations. I used two metrics to compare the statistical properties of these three estimators: the percent relative bias (PRB = $100(\hat{F} - F)/F$) and the root mean squared error (RMSE) (Cherry *et al.*, 2007). For each of the 756 estimates of fatality for each of the three estimators, I calculated the mean, and 2.5th and 97.5th quantiles of PRB over the 1000 simulation runs. I will refer to the

interval between the 2.5th and 97.5th quantiles as the range of PRB. I calculated the RMSE as $RMSE = \sqrt{\sum_{m=1}^{1000} (\hat{F}_m - F_m)^2 / 1000}$.

Because PRB is asymmetric, it can be difficult to interpret. To aid in interpretation of the results, I also calculated the multiplying factor (MF), $MF = F/\hat{F}$, the factor by which the estimated fatality \hat{F} would have to be multiplied to give the actual fatality. The MF provides an intuitive interpretation of PRB. For an unbiased estimate, the MF is 1. The MF for an underestimate of 50% is 2, whereas an overestimate of 100% is required to result in a reciprocal MF of 1/2. As bias approaches $-100%$, the lower limit, the asymmetry in the relationship becomes

more pronounced. An underestimate of 95% has a MF = 20, however, an MF = 1/20 is associated with an overestimate of 1900%. I calculated the mean and 2.5th and 97.5th quantiles of the MF over the 1000 simulation runs.

4. RESULTS

4.1. Percent relative bias (PRB)

Fatality estimates of all three estimators were always ordered, with those when the generating CP model was $\frac{1}{2}N > \text{Exp} > \text{Gamma}$. The PRB of the proposed estimator showed some sensitivity to all factors but never exceeded $\pm 27\%$ (Table 1) and its MF never exceeded 1.37, nor dropped below 0.79 (Table 2). When a simple binomial (SE) model was used, PRB was primarily a function of the CP model; the magnitude of p had little influence on PRB (Figure 1). When the search interval was short (≤ 7 days) and the generating CP model was exponential or $\frac{1}{2}N$, the PRB of the proposed estimator was never more than 11% (Table 1) with largest bias occurring when the interval was approximately equal to the average CP time. When the generating CP model was Gamma, the proposed estimator always underestimated fatality, but never by $> 27\%$ (Table 1). The greatest absolute bias occurred when both persistence time and search interval were short or when both were long (Figure 1).

Fatality estimates of the proposed estimator were generally higher when the conditional rather than the simple SE model was used, resulting in a positive shift in PRB (Figure 2). This was more pronounced when the magnitude of p was low (0.2) than when it was high (0.8). The general ordering of estimates as a function of CP model remained the same. When p was high, and the interval short, estimates of fatality when the generating CP model was $\frac{1}{2}N$ or exponential are almost unbiased, with maximum absolute value of PRB < 12 and 8% , respectively. The PRB of the proposed estimator when the generating CP model was gamma was more responsive to average CP. The largest bias occurred when both persistence time and search interval were short or when both were long (Figure 2). As the magnitude of p decreased, there was a general increase in \hat{F} , with the greatest change occurring when the interval was short.

The naïve estimator always underestimated fatality, under some conditions by as much as 98.5% (MF = 66.7) (Table 1). The effect of the conditional observability model was the same as for the proposed estimator, *i.e.*, to generate a positive shift in the PRB, but the shift was never more than 6.5%. Because the naïve estimator always underestimated fatality, and estimates when the conditional SE model was used were slightly less biased than when the simple SE model was used, only results from the conditional model are shown (Figure 3). The naïve estimator was extremely sensitive to average persistence time, with bias increasing as average persistence time increased. Increasing the interval reduced the bias, particularly when average persistence time was short. PRB of the naïve estimator was close to 0 only when the search interval was long and average persistence time was short. When searches were conducted daily, the MF under any condition was never < 1.35 , but was > 60 when average persistence time was long (Table 2).

The modified estimator underestimated fatality for all but 10 of the 378 factor combinations possible in the simple observability model. The effect of the conditional observability model was again the same as for the proposed estimator, *i.e.*, to generate a positive shift in the PRB, but because the shift was never more than 9%, and the conditional model was slightly less biased than the simple model, only results from the

Table 1. Maximum absolute PRB over all combinations of searcher efficiency (p) and average CP time (\bar{t}) for each estimator and each simulated search interval, SE model and CP model

Estimator	SE model	CP model	Search interval (day)					
			1	2	4	7	14	28
Proposed	Simple	$\frac{1}{2}$ Normal	9.3	10.0	9.9	8.4	5.6	5.2
		Exponential	4.0	4.3	4.9	3.5	5.3	10.7
		Gamma	24.3	21.1	20.0	20.3	22.6	26.7
	Conditional	$\frac{1}{2}$ Normal	26.5	25.7	24.3	21.6	16.1	6.4
		Exponential	25.7	24.5	22.2	18.4	11.1	8.3
		Gamma	22.3	18.8	17.9	18.3	20.7	25.2
Naïve	Simple	$\frac{1}{2}$ Normal	98.4	96.9	93.9	89.7	80.7	66.4
		Exponential	98.5	96.9	94.0	89.9	81.4	68.3
		Gamma	98.5	97.2	94.6	91.2	84.3	74.0
	Conditional	$\frac{1}{2}$ Normal	98.3	96.7	93.6	89.1	79.9	65.4
		Exponential	98.4	96.7	93.7	89.3	80.6	67.5
		Gamma	98.5	97.0	94.3	90.7	83.7	73.3
Modified	Simple	$\frac{1}{2}$ Normal	78.6	77.5	75.1	71.8	65.0	54.2
		Exponential	78.7	77.6	75.4	72.3	66.1	56.8
		Gamma	80.1	79.5	78.0	75.9	71.5	64.4
	Conditional	$\frac{1}{2}$ Normal	73.1	71.8	69.1	65.6	58.5	48.6
		Exponential	73.3	72.0	69.6	66.5	60.3	51.9
		Gamma	75.2	74.7	73.2	71.2	66.9	60.5

Table 2. Minimum and maximum MFs over all combinations of searcher efficiency (p) and average CP time (\bar{t}) for each estimator and each simulated search interval, SE model and CP model

Estimator	SE model	CP model	Search interval (day)											
			1		2		4		7		14		28	
			Min	Max	Min	Max	Min	Max	Min	Max	Min	Max	Min	Max
Proposed	Simple	½ Normal	0.91	1.00	0.91	1.00	0.91	1.01	0.92	1.02	0.95	1.03	0.99	1.05
		Exponential	1.00	1.04	1.00	1.04	1.01	1.05	1.01	1.04	1.02	1.06	1.02	1.12
		Gamma	1.07	1.32	1.09	1.27	1.13	1.25	1.06	1.26	1.04	1.29	1.04	1.37
	Conditional	½ Normal	0.79	0.94	0.80	0.94	0.80	1.01	0.82	1.02	0.86	1.03	0.94	1.03
		Exponential	0.80	1.02	0.80	1.04	0.82	1.05	0.84	1.04	0.90	1.04	1.00	1.09
		Gamma	0.86	1.29	0.89	1.23	0.93	1.22	0.98	1.22	1.03	1.26	1.04	1.34
Naïve	Simple	½ Normal	1.45	64.36	1.09	32.40	1.03	16.48	1.01	9.67	1.02	5.19	1.02	2.98
		Exponential	1.64	64.52	1.21	32.57	1.06	16.67	1.04	9.87	1.03	5.37	1.03	3.16
		Gamma	2.09	68.89	1.43	35.56	1.15	18.62	1.07	11.33	1.05	6.37	1.05	3.85
	Conditional	½ Normal	1.35	60.58	1.08	30.56	1.03	15.58	1.01	9.18	1.02	4.97	1.02	2.89
		Exponential	1.52	60.75	1.18	30.74	1.06	15.76	1.04	9.38	1.03	5.16	1.03	3.07
		Gamma	1.89	65.08	1.36	33.61	1.13	17.69	1.07	10.80	1.05	6.13	1.05	3.75
Modified	Simple	½ Normal	0.99	4.68	0.97	4.44	0.95	4.01	0.96	3.54	0.98	2.86	1.02	2.18
		Exponential	1.12	4.70	1.07	4.46	1.05	4.06	1.02	3.62	1.03	2.95	1.03	2.31
		Gamma	1.33	5.02	1.27	4.87	1.13	4.54	1.07	4.15	1.05	3.51	1.05	2.81
	Conditional	½ Normal	0.97	3.72	0.96	3.54	0.95	3.24	0.95	2.91	0.98	2.41	1.02	1.94
		Exponential	1.10	3.75	1.06	3.58	1.04	3.29	1.02	2.98	1.03	2.52	1.03	2.08
		Gamma	1.26	4.04	1.26	3.95	1.13	3.73	1.06	3.48	1.05	3.02	1.05	2.53

conditional model are shown (Figure 4). The modified estimator was also sensitive to average persistence time, with bias increasing as average persistence time increased, but this effect was strongest when p was small. Increasing the interval reduced the bias, particularly when average persistence time was short. When searches were conducted daily and p was small, the maximum MF was ~ 5 (Table 2). Absolute bias was reduced as $p \rightarrow 1$, but the modified estimator still underestimated fatality under almost all conditions.

4.2. Root mean squared error (RMSE)

The MSE is useful as a measure of goodness or closeness of an estimator only if the estimator is unbiased (Mood *et al.*, 1974). Biased estimators can have smaller MSE than unbiased, and no estimators have uniformly minimum MSE, which is why the search for estimators with minimum MSE is generally restricted to unbiased estimators. In this simulation, the estimators were often biased, obviating comparison of MSEs. However, when estimates were relatively unbiased (absolute bias $< 10\%$) the MSE for the proposed estimator was always larger than that of the other two, usually by $< 2\%$ (median), and never by more than 50%.

5. DISCUSSION

5.1. Evaluation of estimators in the absence of detection bias

A basic property of a good estimator in the current context is one that, in the absence of detection bias, will return the simple count of the carcasses as an estimate. In the absence of detection bias, adjustments to simple carcass counts would be unnecessary and abundance estimates could be calculated using well-established finite sample size methods, adjusting only for unequal probability sample, if necessary. An evaluation of the estimators under these conditions provides insight into their inherent bias. When $p = 1$, the naïve estimator reduces to $\hat{f} = \frac{c}{\bar{t}/I}$, with the estimate of fatality inversely proportional to the ratio of the average CP time to the length of the search interval. This ratio does not reflect the probability that a carcass will persist through the interval, and is not bounded between 0 and 1. When the average CP time is greater than the search interval, this ratio will be greater than 1, causing the estimate of fatality to be biased low and in some cases be less than the actual count (see Fiedler *et al.*, 2007, Table 10). In other cases, the reported lower confidence limit will be less than the actual number of carcasses found (*e.g.*, Johnson *et al.*, 2003b), violating one of the fundamental tenets of statistical analysis, that the estimate itself and its confidence limits be reasonable (Kempthorne, 1989).

The modified estimator is simply an extension of the naïve estimator, *i.e.*, it is the naïve estimator multiplied by an additional factor, $\left(\frac{e^{I/\bar{t}} - 1 + \hat{p}}{e^{I/\bar{t}} - 1}\right)$. The ability of this modification to mitigate the effect described above is highly dependent on the magnitude of p . As p becomes small this factor approaches 1 and the modified estimator reduces to the naïve estimator. Alternatively, as estimated searcher efficiency approaches 1 this factor approaches the value of the multiplier in the proposed estimator, as long as the average persistence time is less than the search interval. This modification to the naïve estimator reduced but did not eliminate the influence of persistence time on bias of the estimate, and was most effective when p was high and the search interval was long.

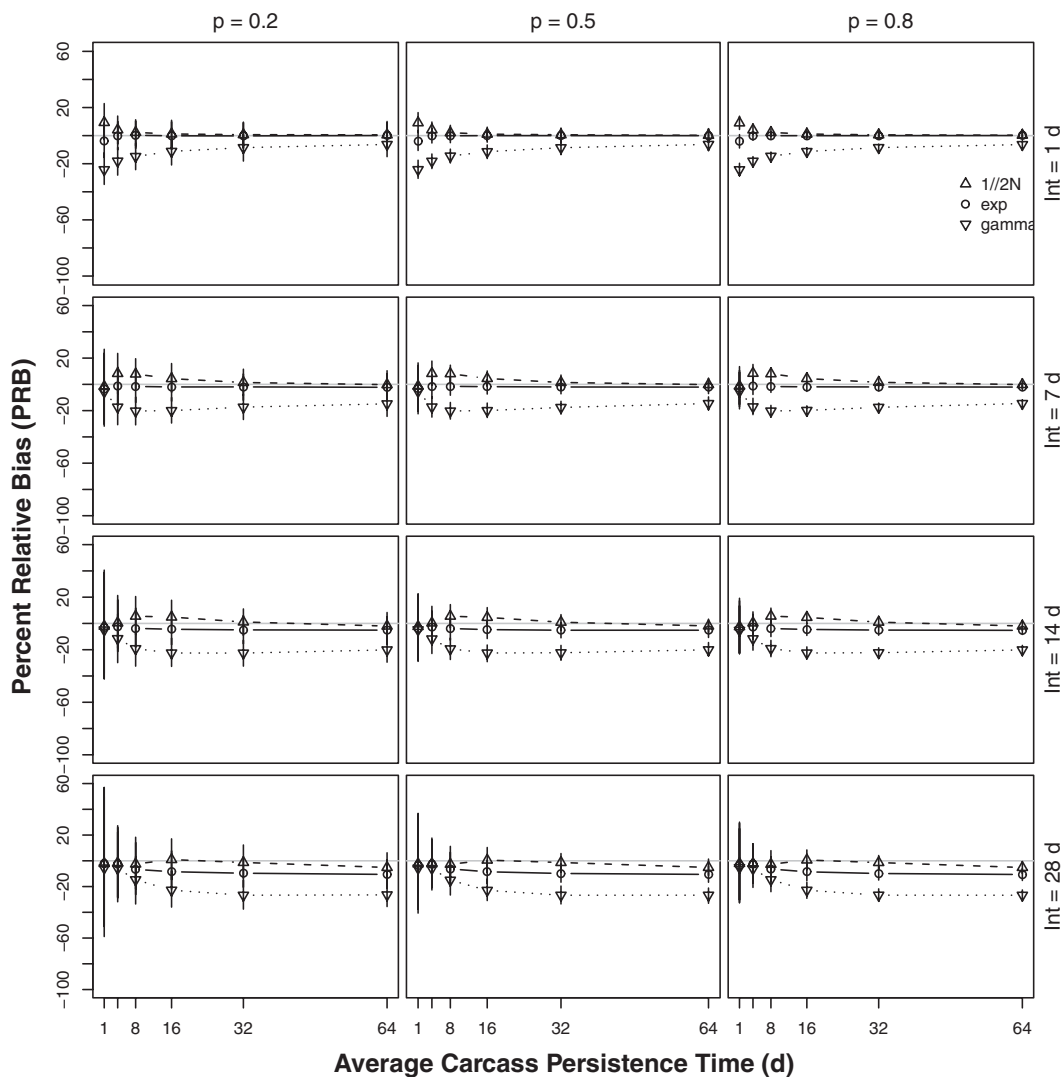


Figure 1. PRB of the proposed estimator as a function of average CP time for each of three CP models when the probability of seeing a carcass was modeled using a simple binomial model, plotted for four search intervals (plot rows) and three magnitudes of searcher efficiency (plot columns)

Only the proposed estimator is unbiased as the detection parameters approach their limits, and estimates fatality to be the carcass count when detectability = 1.

5.2. Percent relative bias

In order to compare fatality rates among species and across sites, and to experimentally evaluate mitigation efforts, estimates of fatality need to be unbiased or the bias must be constant. This simulation demonstrates that bias of two estimators in current use is strongly dependent on detectability parameters, precluding comparisons of rates among sites, and even among species groups within sites.

The bias in the proposed estimator was primarily a function of assumed models of CP and observability rather than of uncontrollable factors. Assuming that carcass removal rate was constant (exponential model) resulted in higher estimates of fatality when the actual process was $\frac{1}{2}N$ and lower estimates of fatality when the actual process was gamma. This was expected. If carcasses are less likely to be removed early on ($\frac{1}{2}N$), then assuming a constant removal rate (exponential) will lead to lower estimates of r and higher estimates of fatality. On the other hand, if carcasses are removed more rapidly early on (gamma), just the opposite occurs, and assuming a constant removal rate will lead to higher estimates of r , and lower estimates of fatality. While the direction of the effect was predictable, the magnitude of the sensitivity of the estimators to assuming an incorrect persistence model was not.

In general, there was little effect of assuming a constant removal process (exponential) when the $\frac{1}{2}N$ was the true model. The proposed estimator was more sensitive to assuming a constant removal rate when the gamma was the true model. To correct this, the proposed estimator could be modified so that r is calculated based on the best fit persistence model rather than assuming a constant removal rate (exponential model). However, knowing the persistence distribution does not guarantee unbiasedness, because time of death will still be unknown and the actual probability of a carcass persisting until the next search can only be determined if time of death is known. Any estimator will need to make assumptions about the rate of entry of carcasses into the morbid population in order to accurately model the probability of persisting. If we had precise models of rate of entry (perhaps based on measured covariates), then searches would be completely obviated. But so far, none

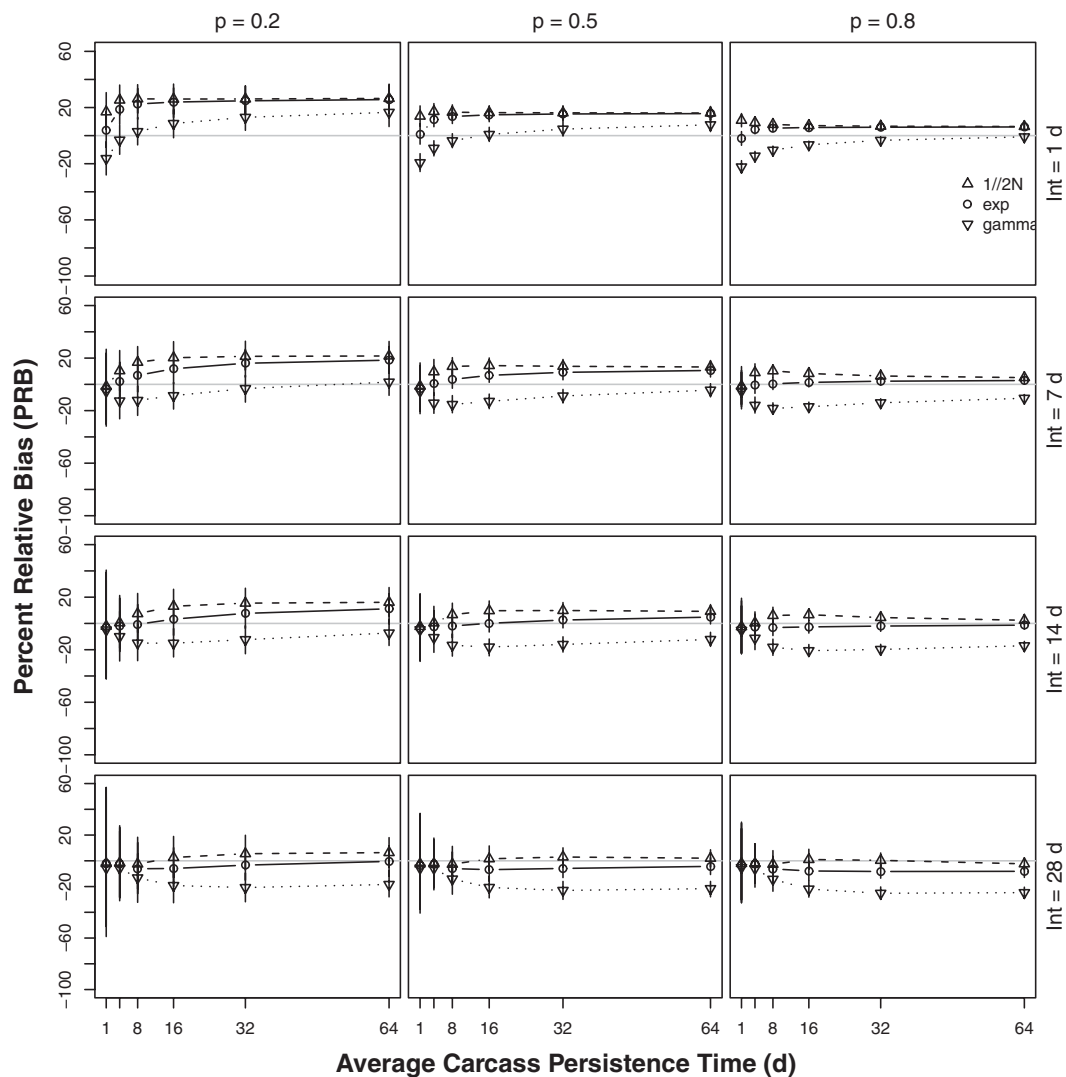


Figure 2. PRB of the proposed estimator as a function of average CP time for each of three CP models when the probability of seeing a carcass was modeled using a conditional binomial model, plotted for four search intervals (plot rows) and three magnitudes of searcher efficiency (plot columns)

exist. In this simulation, the rate of entry of carcasses into the morbid population was not constant, yet the assumption of constant rate in the proposed estimator did not produce large bias.

As the probability of persisting for more than one interval increases (CP long or I short), then how we model the conditional probability of observing a carcass becomes more important. In this study, the SE model had very little effect on bias when p was high, but had a much stronger effect when p was low. Maximizing p is the most direct way to improve the accuracy of the proposed estimator, both by reducing its sensitivity to the SE model and by reducing its sensitivity to changes in magnitude of p . If the area searched can be restricted to open, easily searched habitat in which we can assume that the probability that a carcass is observed is independent of whether it was missed on a previous search, *i.e.*, $p \sim 1$, then the probability of detection will only be a function of persistence. Of course, if the searched area does not comprise the total area of inference, this will require an accurate estimate of π_i , the unequal probability sample weight defined above.

Evaluation of estimators through simulation must be conducted carefully (Cherry *et al.*, 2007). In this simulation I used real activity data with its inherent temporal variance and autocorrelation as a basis for fatality, to remove dependence of simulation results on assumed models of fatality. I varied factors beyond the control of researchers (searcher efficiency and CP) as well as those within control of researchers (search interval) over ranges reported in the fatality monitoring literature. I modeled searcher efficiency and CP using several generating functions, to explore the effect of assumed models on simulation results. In practice, monitoring is carried out to estimate total or per turbine fatality at a site during a defined period of time, often a migratory season. In this simulation, I tried to mimic these conditions, keeping the period of inference constant, rather than keeping the search effort constant, allowing evaluation of the effect of changing the search interval on estimates of fatality. Holding the search effort constant would necessarily change the period during which fatalities were observed and consequently, the total number of fatalities, precluding direct comparisons of the estimators to accurately estimate fatality, and not reflecting the operational conditions under which the estimators are used.

The data used in this fatality simulation reflected actual activity pattern that was not assumed to occur at a constant rate. In monitoring studies, when carcass removal rates are high and searcher efficiency is low, we rely heavily on the assumptions inherent in our estimators of

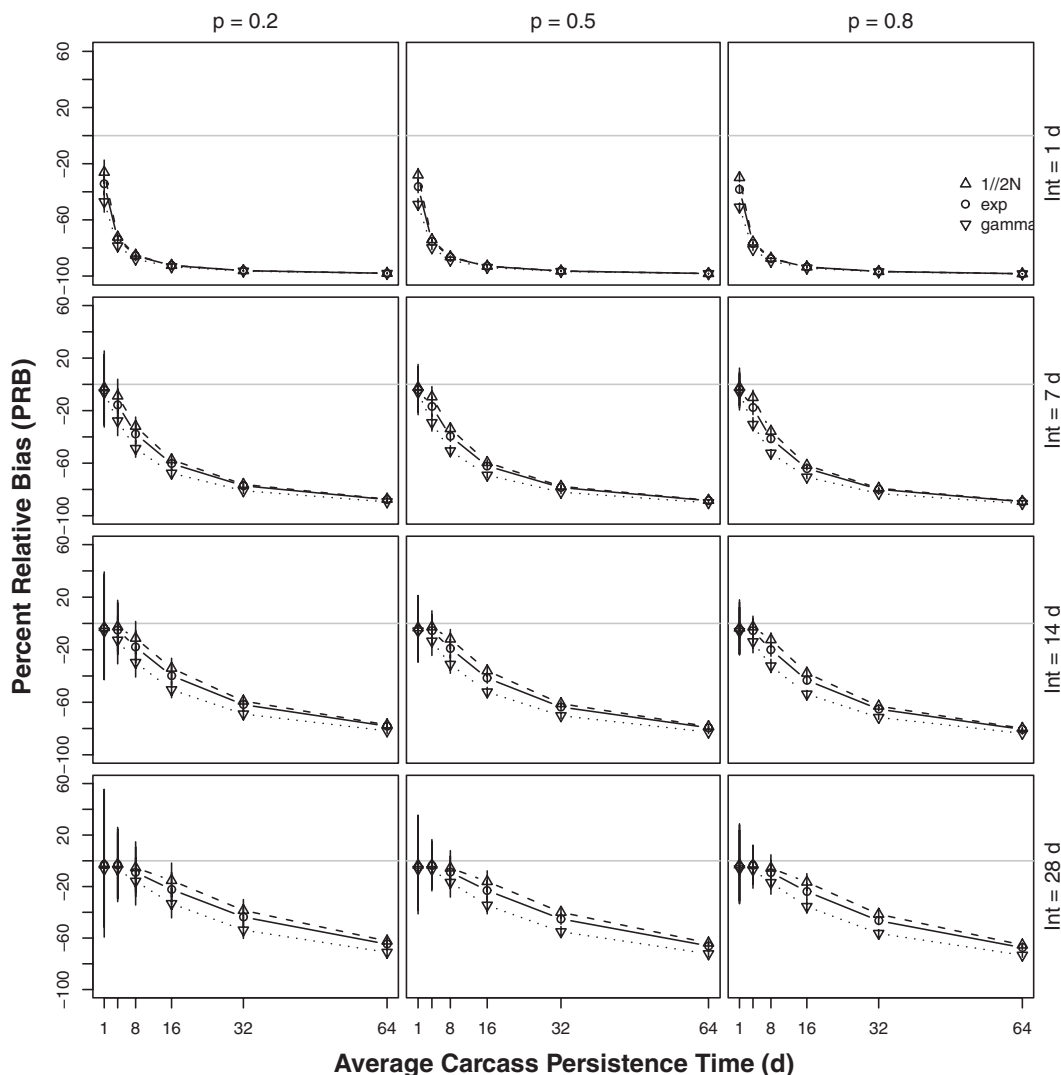


Figure 3. PRB of the naïve estimator as a function of average CP time for each of three CP models when the probability of seeing a carcass was modeled using a conditional binomial model, plotted for four search intervals (plot rows) and three magnitudes of searcher efficiency (plot columns)

these parameters to make accurate estimates of unobserved fatality. This simulation showed that for the proposed estimator, the shorter the search interval, the more robust the estimates were to violations of assumptions. As the search interval increased, the range of PRB increased reflecting the increased variation as the number of searches decreased.

As the interval between searches at each turbine increases, our reliance on assumptions inherent in the estimators increases. We assume that our measure of the average proportion of carcasses persisting through the interval is applicable to all carcasses, that searcher efficiency is the same for a fresh animal as for one that has been lying out for the entire interval, and that the effective period is reflective of the entire interval and is appropriately measured. All of these argue for shorter intervals in which the assumptions are less influential and estimation of the effective period is obviated. But shorter intervals will incur higher costs of implementation. Daily searches may only be necessary for certain purposes, *e.g.*, relating fatality to wind, testing effectiveness of mitigation strategies, *etc.*, whereas monthly search intervals may be so long that the confidence intervals become too large to be useful. The appropriate search interval lies somewhere between these extremes, and the tradeoff between cost and precision of different search intervals should be formally investigated.

In my simulation, an average of ~1780 fatalities (~120/turbine) occurred in each simulation run, somewhat higher than the highest reported fatality rates in the literature (Arnett *et al.*, 2008) but perhaps not higher than actual rates, given the observed bias in current estimators. I believe that my results will apply regardless of fatality rate, but further study is needed to determine if bias changes when fatality rates are much lower.

This simulation was intended to evaluate the statistical properties of three estimators when parameters were known. In practice, searcher efficiency rates and average CP times and their models will have to be estimated. Undoubtedly, this will lead to reduced precision of estimated fatality, particularly when r and p are small, but additional bias may be introduced as well (Thompson, 1992). It is of practical importance to determine the sample size needed to estimate each of these with enough precision to result in confidence intervals of fatality of desired width. In addition, future work should determine the optimal sample size needed to determine the best model for persistence (exponential, $1/2N$, gamma, *etc.*).

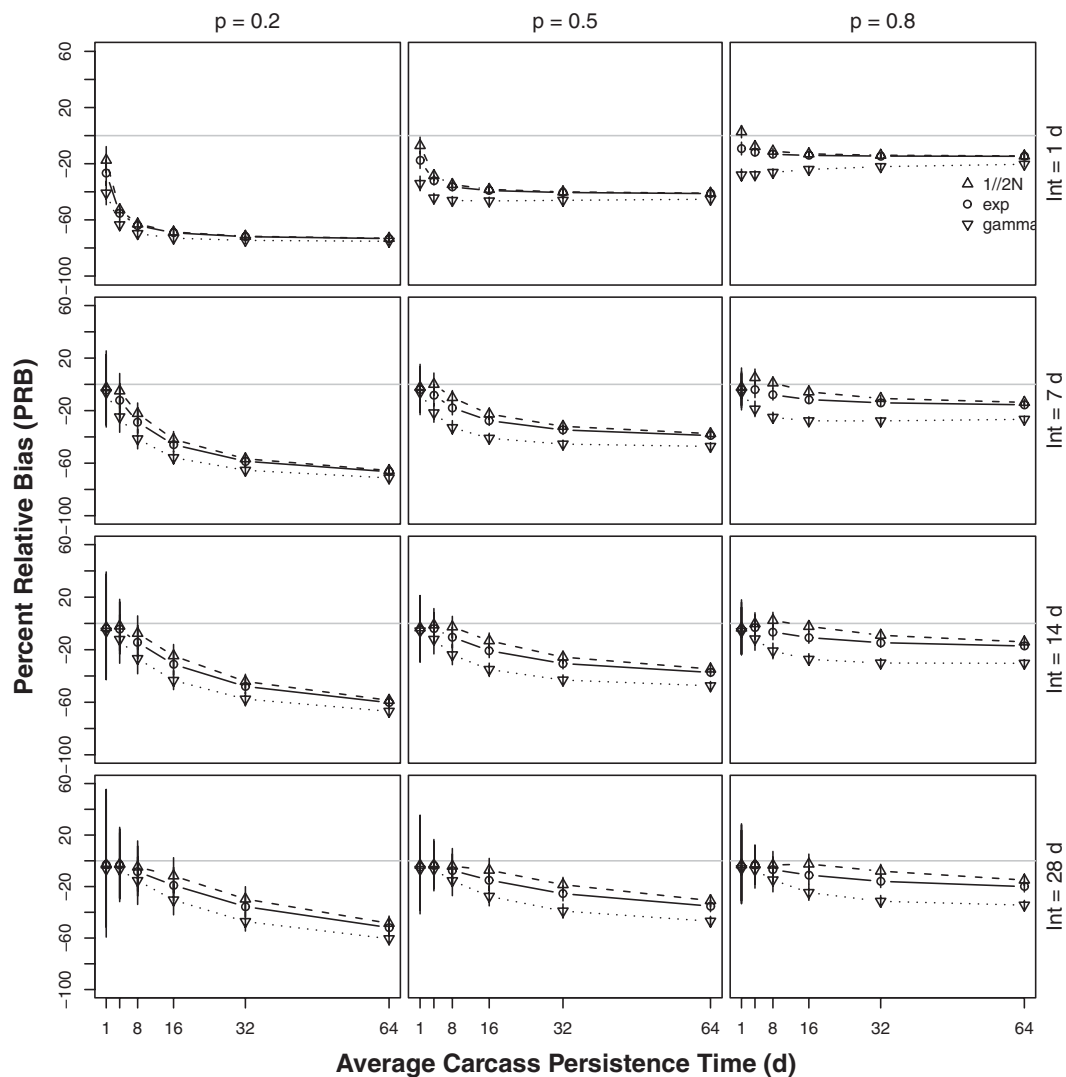


Figure 4. PRB of the modified estimator as a function of average CP time for each of three CP models when the probability of seeing a carcass was modeled using a conditional binomial model, plotted for four search intervals (plot rows) and three magnitudes of searcher efficiency (plot columns)

Abundance is one of the most difficult parameters to estimate without bias, even for a closed population (White, 2008). Several factors combine to place the problem of estimating abundance of a morbid population outside the reach of many currently available tools: (1) the population is not mobile; (2) the population often is not closed; (3) the parameter of interest is not the instantaneous size of a closed population, but the total number of animals that ever entered the population; (4) the probability of detection of members of the population < 1 ; (5) the probability of detection is not equal among members of the population and may be unique to each. The first three factors limit the use of mark-recapture models for estimating abundance of the morbid population. Assumptions of even the simplest Petersen estimator of abundance cannot be met when the population is not mobile (either geographically or temporally). If the cause of death is a particular event, such as an application of a pesticide or an oil spill, then the population of interest might reasonably be considered closed to immigration, but perhaps not to emigration. If the cause of death is on-going, as with power lines, automobile traffic or wind turbines, the population of interest is the total number of fatalities over an extended time period and the population is known not to be closed. The rates at which carcasses emigrate and immigrate can be highly variable and are in turn influenced by species behavior, season, density, and weather. Schwarz *et al.* (1993) developed a Jolly–Seber type estimator to estimate gross births in open populations. It defines group-specific capture and survival parameters, but to overcome identifiability problems relies on assumptions regarding the probability of capture and recapture among time periods and among groups that may not be realistic in the case of a morbid population. Nonetheless, further work to adapt the Jolly–Seber type estimator to the current problem might prove fruitful.

5.3. Examples of application of the estimators at two wind farms

Fatality estimates at the Casselman Wind Project in south-central Pennsylvania, USA, were calculated using all three estimators (Arnett *et al.*, 2009b). Ten of 23 turbines at the site were searched daily for 7 months. Estimated searcher efficiency for bats varied from 100% in open gravel to 13% in dense vegetation and average CP time was estimated to be 31.9 day, resulting in an estimated probability of persisting

through the search interval of 0.9845. A total of 148 bats carcasses were found; 92, 45, and 11 in habitat in which SE was estimated to be 100, 71, and 13%, respectively. Estimated total fatality (and 95% bootstrap confidence intervals) at the site was 784.2 (95% CI: 504.0, 1248.8), 27.0 (95% CI: 12.2, 54.9), and 459.1 (95% CI: 370.7, 555.6) for the proposed, naïve, and modified estimators, respectively. These results are consistent with the simulation results; when average CP time is long (32 day) and the search interval is short (1 day) the naïve estimator strongly underestimates fatality, regardless of SE (Figure 3, top row). In this case, even the upper confidence limit of the naïve estimate was far below the number of carcasses actually observed, for reasons discussed above. Estimates from the modified estimator were intermediate between those of the naïve and the proposed. Its discrepancy with the proposed estimator was due primarily to underestimates of fatality by >80% for those bats whose SE was estimated to be very low (13%).

In contrast, fatality estimates of birds at Klondike III Wind Farm in Oregon, USA, were calculated for the proposed and modified estimators only (Gritsky *et al.*, 2009). Twenty-three of 80 turbines at the site were searched on intervals of ~14 or ~28 day for 1 year. Both searcher efficiency and average CP time were found to vary with size of the carcass and season. For small birds, searcher efficiency ranged from 42 to 55% and average CP ranged from 4.3 to 15.4 day across seasons. For large birds, searcher efficiency ranged from 45 to 82% and average CP time ranged from 8.4 to 29.8 day across seasons. A total of 49 small and 7 large birds were found, with estimated total fatality (and 95% bootstrap confidence intervals) at the site of 496.4 (95% CI: 378.3, 662.8) and 468.8 (95% CI: 350.6, 631.0) for the proposed and modified estimators, respectively. Again these results are consistent with the simulation results; when average CP time is short (<16 day) and the interval is long (14 or 28 day), the estimates from the modified estimator are similar to those of the proposed (Figure 4, bottom two rows, center panel). Its discrepancy with the proposed estimator was due primarily to underestimates of fatality for the large birds with longer average persistence times.

5.4. Management implications

A first step in understanding the impacts of wind energy facilities on wildlife is to estimate the number of animals being killed at these facilities. Unbiased estimates of fatalities combined with unbiased estimates of population sizes will contribute to evaluation of ultimate impacts of wind energy facilities on wildlife populations. Failure to use an unbiased estimator precludes direct comparability of results among studies and even within studies. Average CP time and searcher efficiency can differ significantly among species. Underestimating one species by a factor of 2 and another by a factor of 30 in the same study will lead to erroneous conclusions regarding the relative proportion of total fatality contributed by each species. These same differences in bias can occur across sites, again leading to misunderstanding of the relative fatality rates in different parts of the country and the world. Comparability of fatality estimates across groups, *e.g.*, turbine models, species, sites, *etc.*, can only be achieved if the estimator used is unbiased and insensitive to changes in conditions across groups, *i.e.*, detectability parameters and search interval. As these factors are known to vary widely among species and across sites, current estimates of fatality using biased estimators are not directly comparable.

Researchers have no control over the average CP time at a site and little control over searcher efficiency beyond training personnel or mowing vegetation, but they do have control over the search interval. Establishing a search protocol and using an estimator that will result in relatively unbiased estimates for all possible values of searcher efficiency and average CP time is critical to achieve comparability among sites and to our ability to evaluate the impacts of wind power generation facilities on wildlife. Further research is needed to refine the estimator and to understand the effects of estimation methods on the parameters used in the estimator.

Tukey (1979) argues “We should measure what is needed for policy guidance, even if it can only be measured poorly.” While the proposed estimator exhibited some bias under certain conditions, its relative stability across all conditions, particularly in comparison to two commonly used estimators, makes it useful for measuring what is needed to guide policy decisions.

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