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IN REVIEW – *ECOLOGICAL APPLICATIONS*

Evidence of Absence Regression: A Binomial N-Mixture Model for Estimating Bird and Bat Fatalities at Wind Power Facilities

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Abstract

Estimating bird and bat fatalities caused by wind-turbine facilities is challenging when fatalities are rare and the number of observed carcasses is either exactly zero or very near zero. The rarity of found carcasses is exacerbated when particular species are rare, when carcasses degrade quickly, when they are removed by scavengers, or when they are not detected by observers. With few observed fatalities, common statistical methods like logistic, Poisson, or negative binomial regression are biased and prone to fail due to complete or quasi-complete separation. Here, we propose a binomial N-mixture model to estimate fatality rates and totals that incorporates study covariates and separate information on probability of detection. Our model extends the 'evidence of absence' model (Huso et al., 2015) by relating carcass deposition rates to study covariates and by incorporating the number of turbines. Our model, which we call Evidence of Absence Regression (EoAR), can retrospectively and prospectively estimate the total number of birds or bats killed at a single wind-power facility or a fleet of wind-power facilities given covariates in the relation. Furthermore, with accurate prior distributions the model's results are extremely robust to complete or quasi-complete separation. In this paper, we describe the model, show its low bias and high precision via computer simulation, and apply it to bat fatalities observed on 21 wind power facilities in Iowa.

KEYWORDS:
Rare Events, Carcasses, Wind-turbines, Evidence of Absence, N-mixture

1 | INTRODUCTION

The number of birds and bats killed by wind turbine blade strikes in North America and Europe each year varies wildly (Huso et al., 2015). For example, Smallwood and Thelander (2008) estimated 1,127 raptor fatalities per year (1998 to 2003) at the Altamont Pass Wind Resource Area in California, while Chodachek et al. (2012) estimated 28 fatalities over all bird species during 2011-2012 at the Pioneer Prairie wind facility in Iowa. A review of 116 studies from across the United States found between 0.27 and 11.02 bird deaths per megawatt per year (Erickson et al., 2014, Table 2). Given the wide range and sometimes low number of fatalities, particularly when partitioned by into species groups or focused on endangered species, it is not surprising that the number of carcasses available to the analyst is often either zero or too few to make meaningful estimates. The difficulties of low

counts are even more pronounced because searchers find only a fraction of the fatalities at a site due to carcass decomposition, removal by scavengers, detection failure, or failure to search everywhere a carcass might land.

If fatalities were common at a wind-power facility, many statistical techniques could be used to estimate fatality rates, and hence totals. Commonly applied techniques include the Horvitz-Thompson-type (Horvitz and Thompson, 1952; Särndal, Swensson, and Wretman, 1992; Lohr, 2010) methods of Shoenfeld (2004), Jain (2005), and Huso (2011). These Horvitz-Thompson-type approaches generally estimate a single mean fatality rate (e.g., per turbine per year) and scale by the number of turbines in order to estimate annual totals. When fatalities are common, techniques like normal-theory regression, generalized linear regression models (McCullagh and Nelder, 1989), negative binomial regression, and occupancy methods (MacKenzie, Nichols, Hines, et al., 2003; MacKenzie, Nichols, Sutton, et al., 2005; Pavlacky et al., 2012) can be used to model fatality rates as a function of covariates and hence account for variation due to measurable factors. Many of these techniques also account for variation in detection probabilities, either by inclusion of an offset term or by including detection as an explicit parameter.

But, when fatalities are rare, estimation of mean fatality rates and explanation of variation via a regression-type relationship is challenging. All the regression models cited in the previous paragraph encounter difficulties when fatalities are rare. Difficulties arise because the increased prevalence of zeros causes complete or quasi-complete separation among covariate combinations (Albert and Anderson, 1984). Quasi-complete separation occurs if no fatalities are observed for one or more combinations of covariate values. For these unobserved covariate combinations, the data-only rate is exactly zero yet the true mean rate in these models must be greater than zero, and instability or near singularities in the estimation process results. In other words, support for estimation of one or more coefficients in the covariate space is weak or unstable. Instability in these cases is natural because variation (and information) is low when event rates are near zero.

Model instability is not the only problem when common regression-like techniques are applied to rare event data. Bias is also a problem. When applying logistic regression to rare event data, the low variation near zero induces many zeros and causes a sharp underestimate of rare event probabilities (King and Zeng, 2001). To combat logistic regression's bias when probabilities of a one ('success') are extremely low, analysts have used weighted logistic regression (King and Zeng, 2001; Maalouf and Trafalis, 2011) and modifications of machine learning algorithms (Maratea, Petrosino, and Manzo, 2014). Both of these techniques used separate estimates of rare event probabilities to adjust and stabilize coefficient estimates. Despite these modifications that combat bias, nothing resolves logistic regression's inability to estimate rates when data are completely or quasi-completely separated.

When monitoring for wildlife fatalities, detecting zero fatalities is a particularly problematic case. All statistical techniques cited above require at least one fatality to produce positive (non-zero) estimates. In most studies, this zero-carcass-zero-estimate situation is not satisfactory because fatalities are known to occur with non-zero probability. The problems inherent in this case are compounded by the fact that the probability of discovering a fatality is almost always significantly less than 100%.

To combat problems associated with rarity when monitoring wind power facilities after construction, Huso et al. (2015) introduced the probability of detection (here, denoted g; other papers, denoted p or π) into a binomial N-mixture model (Kéry and Royle, 2016, Ch. 7) and used this model to estimate total fatalities. The key feature of Huso et al.'s (2015) model is that detection probabilities are estimated from the study design (e.g., survey frequency, carcass persistence, detection probability, etc.) and not from observed carcass counts in the binomial portion of the model. This feature allows the model to avoid the problems and controversies surrounding use of binomial N-mixture models in other parts of ecology (Barker et al., 2018; Kéry, 2018).

The method of Huso et al. (2015) has become known as the Evidence of Absence (EoA) approach, and it represents a major step forward in analysis of bird and bat fatalities at wind facilities. Current parameterizations of the EoA mixture model focus on estimating the total number of fatalities that occurred during a particular year. Current EoA models do not estimate the relationship between rates and other study factors (covariates), nor do they take into account the facility's size or number of years monitored. Standard EoA estimates are exactly the same for 20-turbine and 2000-turbine facilities. Assuming the same number of carcasses are found and g is constant, current EoA rate estimates are also the same whether the study covered one or five field seasons. The utility of EoA would be greatly enhanced if it were extended to include covariates and if rate estimates were inherently scaled to a useful basis (such as number of turbines or total megawatts).

In this paper, we extend EoA to allow estimation of covariate relationships and show that inclusion of an offset term allows scaling to occur naturally during estimation. Our binomial N-mixture model includes an additional level of hierarchy, relative to EoA, that parameterizes a log-linear relationship between study covariates and sample-unit fatality rates. This extension allows factor and continuous effects to act upon rate estimates, thereby reducing unmodeled heterogeneity and improving both accuracy and precision. While not necessary, we also advocate inclusion of an offset in the log-linear model to scale retrospective rate estimates to natural units (e.g., per turbine, per hectare, per year, etc.). It is then possible to derive prospective estimates of

future totals by simple scaling operations. If zero mortalities are observed, it is possible to inform the prior distribution of our model's rate parameter using previous estimates or separate information and thereby ameliorate or eliminate the separation issues that plague other techniques. If zero mortalities are observed and it is not possible to inform the rate parameter's prior, the method still provides reasonable fatality estimates because the information provided by detection probabilities (i.e., g) is being considered and this stabilizes the estimation process greatly. While our focus is primarily on bird and bat fatality rates, the model is useful in any situation where detection probabilities can be quantified. Our model is particularly useful when events are rare and covariate relationships are sought. We call our model Evidence of Absence Regression, or EoAR, to reflect the fact that this model is essentially EoA with a regression relationship tacked on.

In the next section, we define the EoAR hierarchical model. We then describe two example applications of the model to rare and non-rare fatalities observed during three years of bat monitoring at 21 wind power facilities in Iowa. We use uninformed prior distributions during analysis of the non-rare species, and illustrate use of informed priors during analysis of the rare species. Following these examples, we describe simulations designed to investigate EoAR's statistical properties. In Results, we provide results of the two example applications and summarize output from the simulation. Finally, in Discussion, we summarize EoAR's features and compare them to the features of alternative methods.

2 | METHODS

2.1 | EoAR Model Definition

We define a site to be the temporal and spatial units over which observed counts are summarized. For example, wind power studies typically summarize fatalities by facility and year (i.e., site = annual facility). Alternatively, wind power fatalities could be summarized by turbine and field season (i.e., site = annual turbine) or by turbine over all years of the study (i.e., site = turbine).

We assume m sites are included in the study and that each site is searched a variable number of times over a variable length of time. During each search, field personnel record c_{ij}, the number of observed carcasses detected on search j of site i. The count summary we analyze is the total number of observed events at site i summed over searches that occurred during the monitoring period, $C_i = \sum_j c_{ij}$. To accommodate sites of different sizes and monitoring periods of differing lengths, we define A_i to be the number of natural units (e.g., days, years, turbines, facilities, facility-years, etc.) over which past data was collected at site i. Typically, C_i is the total number of carcasses observed at facility i during a particular year and A_i is the number of turbines monitored at facility i.

The probability of detection (g) is a key parameter, both here and elsewhere (Huso et al., 2015), and its estimation for wind facility monitoring studies is too complicated to detail here. Wind facility carcass search detection probabilities are complicated because they depend on the timing of seasonal carcass arrivals, search start date, search end date, search interval, number of searched turbines, searcher efficiency (detection given presence), carcass removal rates, and the proportion of the carcasses that fall in the searched area. Here, we rely on established formulas implemented in the `genest` R package (`west-inc.shinyapps.io/GenEst/`; `cran.r-project.org/GenEst`) to compute detection probabilities and variation given study design elements (Dalthorp and Huso, 2015; Reyes et al., 2016). Whether using `genest` or not, we assume field data collection elements have been evaluated and that site-specific g_i and have been computed. We then compute a weighted average g across sites as,

$$\hat{\mu}_g = \frac{\sum_{i=1}^{m} A_i g_i}{\sum_{i=1}^{m} A_i}$$

where weights A_i are typically the number of turbines at facility i. We also compute a weighted standard deviation for the overall g as,

$$\hat{\sigma}_g = \sqrt{\frac{\sum_{i=1}^{m} A_i (g_i - \hat{\mu}_g)^2}{\frac{m-1}{m} \sum_{i=1}^{m} A_i}}$$

We then model site-specific detection probabilities as random effects following a single Beta distribution. Given $\hat{\mu}_g$ and $\hat{\sigma}_g$, we estimate the the α and ψ parameters of g's Beta distribution using method of moments,

$$\hat{\alpha} = \hat{\mu}_g \left(\frac{\hat{\mu}_g(1-\hat{\mu}_g)}{\hat{\sigma}_g^2} - 1 \right) \tag{1}$$

$$\hat{\psi} = (1-\hat{\mu}_g) \left(\frac{\hat{\mu}_g(1-\hat{\mu}_g)}{\hat{\sigma}_g^2} - 1 \right), \tag{2}$$

which assumes $\hat{\mu}_g(1-\hat{\mu}_g) > \hat{\sigma}_g^2$. If $\hat{\mu}_g(1-\hat{\mu}_g) < \sigma_g^2$, we evaluate the histogram of g_i and either reduce σ_g^2 slightly and continue or discard the Beta distribution in favor of a truncated Gamma or a smoothed histogram density estimate (Silverman, 1998).

Our interest lies primarily in the site-specific rate, λ_i, defined as the number of events per units of A_i. Again, λ_i in wind facility monitoring studies typically estimates fatalities per turbine per year, but could be fatalities per facility-year, per turbine over multiple years, per turbine per month, etc. The most useful basis for λ_i depends largely on the utility and availability of appropriately covariates.

If carcass detection were perfect (i.e., $g = 1$) and if we observed $C_i > 0$ for at least some sites, we could potentially model λ_i as a function of study covariates using the weighted logistic analysis of King and Zeng (2001), which should include A_i as an offset. If $g = 1$ and events tallied by C_i are sufficiently common to prevent incomplete separation issues, we could also model variation in λ_i using well known models for binomial or count responses (e.g., logistic and negative-binomial regression), again using A_i as an offset term.

When $g < 1$, we do not know the true number of events, only that it is greater than or equal to C_i. Associated with λ_i in this case is the true number of events at site i over A_i units, which we define to be M_i ($M_i \geq C_i$). To estimate M_i, we employ a Bayesian approach which views both λ_i and M_i as random variables, and in particular assumes that M_i follows a Poisson distribution with mean $\lambda_i A_i$. Given values of M_i, the total number of past fatalities over all sites is $M_T = \sum_{i=1}^{m} M_i$. Assuming independence of fatalities among sites, M_T is Poisson with mean $E[M_T] = \sum_{i=1}^{m} \lambda_i A_i$.

In many cases, the size of data collection units (i.e., A_i) differs from the size of future prediction units. To accommodate this difference, we define B_i to be the number of future units of interest. For example, if the lifetime of a site is 30 years and λ_i is expressed as fatalities per year, we might be interested in the number of fatalities over the entire lifetime of the site and hence set $B_i = 30A_i$. Or, A_i could be the number of monitored turbines while we set B_i to the total number of turbines at facility i (monitored + un-monitored). Given B_i, future fatalities also follow a Poisson distribution with mean $\lambda_i B_i$. Total future mortalities over all sites has expectation $E[M_F] = \sum_{i=1}^{m} \lambda_i B_i$, assuming independence of sites.

The key feature of EoAR is the regression relationship between λ_i and exogenous site-specific covariates, like season, habitat, year, distance, etc. By estimating such a relationship, we allow heterogeneity in rates across sites and predictions of future fatalities are correspondingly improved. The hierarchical binomial N-mixture EoAR model is,

$$\begin{aligned} \beta_k &\sim \text{Normal}(\mu_k, \sigma_k) \quad \text{for } k = 0, 1, \ldots, p \\ log(\lambda_i) &= \beta_0 + \beta_1 x_{i1} + \ldots + \beta_p x_{ip} \\ g &\sim \text{Beta}(\alpha, \psi) \\ M_i &\sim \text{Poisson}(\lambda_i A_i) \\ C_i &\sim \text{Binomial}(M_i, g) \end{aligned} \tag{3}$$

In this model, the β_k's are coefficients to be estimated, while α and ψ are constants. To illustrate the model visually, Appendix S1: Figure S1.1 contains a directed acyclic graph depicting the structure of EoAR, drawn for Bayesian analysis along the lines suggested by Kruschke (2011). We choose to estimate coefficients in Equation 3 using MCMC sampling implemented in JAGS (Plummer, 2003), but other estimation techniques are possible (e.g., Gibbs sampling or direct evaluation). We generally use the median of a parameter's posterior distribution as that parameter's point estimate and quantiles of the posterior as the parameter's credible interval, but other summaries of the posteriors are possible (e.g., means and highest-density intervals). An R package for estimating EoAR models using formula-based specification is available from the authors and via links provided Appendix S1.

The minimum amount of information needed to estimate an EoAR model consists of detection probabilities (α, ψ) and the number of observed carcasses (i.e., C_i for all i). If we have prior estimates or independent information about the location and width the distribution for certain β coefficients, it can be incorporated into the EoAR model by setting μ_k and σ_k accordingly. A special case arises when only an intercept is present in Equation 3 and independent information exists on the magnitude and variation of the average number of fatalities. Suppose we obtain independent information regarding the mean rate, $\bar{\lambda}$ and its

variation over all m sites. For example, perhaps we obtain an estimate of total mortalities, $\hat{M}_T$, and its standard deviation, $\hat{\sigma}_{M_T}$, during the previous year or using a separate technique. A reasonable prior estimate of $\bar{\lambda}$ is then $\hat{M}_T / \sum_i A_i$, and a prior estimate of $\bar{\lambda}$'s standard error is $\hat{\sigma}_{\bar{\lambda}} = \hat{\sigma}_{M_T} / \sum_i A_i$. Assuming $\bar{\lambda}$ follows a log-normal distribution, the intercept parameter in Equation 3 will follow a normal distribution, and we can set the prior mean and standard deviation of the intercept using the following equations,

$$\mu_{\beta_0} = 2log(\bar{\lambda}) - 0.5log(\hat{\sigma}^2_{\bar{\lambda}} + \bar{\lambda}^2) \tag{4}$$

$$\sigma_{\beta_0} = \sqrt{-2log(\bar{\lambda}) + log(\hat{\sigma}^2_{\bar{\lambda}} + \bar{\lambda}^2)}. \tag{5}$$

When prior information on coefficients is not available, we choose to use vague prior distributions for the β_k's. To implement vague priors, we set μ_k=0 and σ_k equal to 100 times the upper limit of a (Wald) 95% confidence limit for β_k obtained from a Poisson regression of C_i/g_i on x_{i1} through x_{ip}. Specifically, our vague priors set $\sigma_k = 100(|b_k + 2sd_k|)$ where b_k and sd_k are the estimated coefficient and standard error for β_k in a Poisson regression containing covariates and offset term $log(g_i) + log(A_i)$. If the regression does not converge due to incomplete separation, we remove effects without variation in C_i until the regression converges and set prior standard deviations for the coefficients of removed effects equal to 100.

2.2 | Simulations

To investigate bias and precision of the EoAR λ and M estimators, we ran two set of simulations, one set without covariates and another set with a simple factor covariate.

During the first set of simulations, we set $\beta_0 = 2.718$ to simulate a true fatality rate of $\lambda = 1$. We set the number of sites, m, to 20 and 50 and the true mean detection probability to $\mu_g = 0.1$ and 0.3. For both levels of mean detection probability, the simulation assumed a variance of $\sigma^2_g = 0.005$ and computed α and ψ using Equations 1 and 2. At all sites ($i = 1, 2, \ldots, m$), the simulation generated actual fatalities using a Poisson distribution, i.e.,

$$M_i \sim \text{Poisson}(\lambda).$$

Finally, the simulation generated observed fatalities using the binomial distribution, i.e.,

$$C_i \sim \text{Binomial}(M_i, g_i),$$

where $g_i \sim \text{Beta}(\alpha, \psi)$. Given C_i, α, and ψ, we estimated λ, M_i and $M_T = \sum_i M_i$ using the EoAR model outlined in Section 2.1. We computed point estimates as the median of each posterior distribution and reported histograms of the rate parameter's point estimates, $\hat{\lambda}$, and fatality estimation errors, $\hat{M}_T - M_T$, over 500 iterations of the simulation. We estimated bias of both $\hat{\lambda}$ and $\hat{M}_T$ as the numerical average of each estimator over iterations minus the true value.

Our second set of simulations was similar to the first except that we added a single discrete factor with two levels to the equation for true λ. This simulation investigated whether EoAR could recover rate information when fatalities were rare at some sites and common at others. We set the factor covariate, x_{i1}, to 0 for half the sites in the simulation (i.e., $x_{i1} = 0$ for $i = 1, 2, \ldots, m/2$) and 1 for the other half. To simulate high and low rates among sites, we set $\beta_0 = 22{,}000$ and $\beta_1 = -21{,}997.28172$. These β values translated into rates of $\lambda_i = 10$ for the first half of the simulated sites and $\lambda_i = 1$ for the second half. We set the remaining parameters to make the second simulations comaprable to the first. That is, we set m to 20 and 50, $\mu_g = 0.1$ and 0.3, simulated $M_i \sim \text{Poisson}(\lambda)$, and $C_i \sim \text{Binomial}(M_i, \mu_g)$.

R code to carry out the simulations appears in Appendix S1.

3 | BAT FATALITY MONITORING IN IOWA

To illustrate EoAR numerically, we apply it to Indiana bats (INBA) (*Myotis sodalis*) and little brown bats (LBBA) (*Myotis lucifugus*) bat fatality data collected during 2015, 2016, and 2017 at twenty-one operating wind power facilities located in Iowa (Figure 1). We conducted these studies in part because INBA are listed as endangered under the Endangered Species Act and LBBA are considered rare and of concern. LBBA are considered to occur statewide in Iowa, while INBA officially occur only in its range that encompasses the southeast quarter (approximately) of the state.

During data collection, field personnel regularly searched for bat carcasses beneath turbines on plots of varying size and shape. In 2015 and 2017, personnel walked the perimeter of each turbine's pad and along the access road to a distance of 100m from

the turbine. In 2016, technicians mowed square plots centered on the turbine at a random sample of 20% of the turbines. The size of mowed plots were 60m X 60m, 100m X 100m, or 200m X 200m. We call the mowed plots 'full' plots and technicians searched them by walking straight transects placed 10m apart. At the other 80% of turbines in 2016, technicians walked the perimeter of the turbine pad and along the access road out to a distance of 100m from the turbine. Data collection occurred at nine facilities in 2015, thirteen facilities in 2016, and two facilities in 2017. Three facilities received survey effort in both 2015 and 2016 (Table 1).

In the remainder of this section, we describe two example EoAR applications. In the first, we relate annual counts of LBBA to study covariates and identify factors effecting fatalities per turbine per year. We illustrate the model using LBBA counts because we found this species with enough frequency to support inclusion of covariates. In the second example, we analyze INBA counts, which were less frequently found than LBBA, to illustrate estimation of the model without covariates. In the second example, we fit an intercept-only model with and without informed prior distributions to illustrate the consequences of informing the intercept's distribution.

3.1 | Example 1: Little Brown Bats

The covariates we considered in this example included ecological sub-region of the facility (Figure 1a), an east-west grouping of facilities we termed 'stratum' (Figure 1b), and distance to the nearest river greater than class 4 (Figure 1c) (Table 1, columns 8 through 11). Based on visit timing, visit frequency, searcher efficiency, carcass persistence, and the proportion of the carcass distribution searched, we computed facility and year-specific probabilities of detection (g) using the functions and interface in the `GenEst` R package. (columns *gAlpha* and *gBeta*, Table 1).

For this example, we fitted all possible combinations of study covariates up to a maximum of three variables in the log-linear model for λ (Equation 3). All combinations of covariates *DistToWater*, *EastWestStratum*, *EcoRegion*, and *SiteAge* with three or fewer variables yielded 15 possible models (i.e., intercept only, intercept + *DistToWater*, intercept + *EastWestStratum*, …, intercept + *EastWestStratum* + *EcoRegion* + *SiteAge*). We assessed the fit of each model using the deviance information criterion (DIC) (Gelman et al., 2004; Spiegelhalter et al., 2014) and considered models within approximately 2 DIC units to be equivalent. We included the logarithm of the number of turbines at each facility (*Turbines*, Table 1) as an offset in all models.

3.2 | Example 2: Indiana Bats

A single INBA carcass was found during monitoring at the eight facilities in the official INBA range, and this low number of carcasses could not support inclusion of a covariate in the EoAR model without drastically informing all coefficient priors. Instead, we first estimate the mean and upper bound of the number of INBA fatalities from an intercept-only EoAR model with vague priors. We also illustrate the informed case by including estimates of the INBA fatality rate from a separate analysis. The informed analysis used rate estimates obtained from a species composition approach applied to 2015 and 2016 data. The species composition approach uses the Huso (2011) Horvitz-Thompson-type estimator to estimate fatalities of all bat species combined, then multiplies all-species mortality by a species composition ratio obtained from a larger statewide data set of observed fatalities. We computed variance of the species composition estimate using formulas for the product of random variables found in Casella and Berger (2002).

In 2015 and 2016, the species composition approach estimated $\hat{\lambda} = 0.055038$ INBA fatalities per turbine per year at facilities in the Iowa portion of INBA's official range. The estimated standard deviation of the species composition estimator was $\hat{\sigma}_{\hat{\lambda}} = 0.03664359$. Using these values in Equations 4 and 5, we obtained prior estimates of $\mu_{\beta_0} = -3.0831799$ and $\sigma_{\beta_0} = 0.6057304$.

4 | RESULTS

4.1 | Example 1: Little Brown Bats

The DIC statistic ranked two of the fifteen fitted models as equivalent (Table 2). The difference in DIC between the top two models was 0.65, while DIC differed by four units between the highest and third-ranked model. The top two models both contained *EastWestStratum* and *SiteAge*, while the top model contained *EcoRegion* and the second model contained *DistToWater*. Close inspection of the data in Table 1 reveals zero fatalities in *EcoRegion* 'RP'. The lack of a single fatality in one level of *EcoRegion* caused an abnormally large negative coefficient for *EcoRegion* = 'RP' relative to *EcoRegion* = 'DML' (the reference

level; coefficient = -80.8). Because *EcoRegion* is a factor, this type of quasi-complete separation in one dimension did not cause bias in the other coefficient estimates, but it may have biased DIC downward. In addition, we felt the second ranked model was more easily applied to study areas outside Iowa because we felt *DistToWater* effects were more likely to be accurate on other study areas in the Mid West than the *EcoRegion* effects estimated here. For these reasons, we chose to report results derived from the second-ranked model.

Coefficients in the final model (Table 3) estimated a 9.8% drop in mortalities per turbine per year for every kilometer increase in distance to the nearest water (0.098 = 1 - exp(-0.1036), 95% CI = 2.8% to 17.1%). To illustrate the size of this effect, consider Facility A located 5 kilometers from water and Facility B located 15 kilometers from water. Assume that apart from the difference in distance to water, Facility A and Facility B are identical. The final LBBA EoAR model estimates fatalities per turbine per year at Facility B to be 64.5% lower than at Facility A (0.645 = 1 - exp(-0.1036(10))). In other words, if the fatality rate at Facility A is X per turbine per year, this model predicts fatalities at Facility B will be $exp(-0.1036(10))X = 0.355X$.

We note that only 2 LBBA fatalities occurred in the western stratum of *EastWestStratum*, while 26 and 41 mortalities occurred in the eastern and middle stratum, respectively. After factoring in probability of detection and the fact that the middle stratum contained more turbines, the final model estimated that annual LBBA fatalities per turbine per year decreased by 30% (95% CI = -8% to 53%) in the middle stratum relative to the eastern stratum, and by another 98% (95% CI = 95% to 99%) in the western stratum relative to the middle stratum.

The final model also estimates that fatalities of LBBA decrease as the facility ages. The final model estimates that LBBA fatalities per turbine decrease by an average of 12.5% every year the facility operates (95% CI = 6.6% to 19.3%). To illustrate, fatalities during the first year of operation for a facility in the eastern stratum located 10 km from the nearest water were estimated to be 1.6484 individuals per turbine per year. By year 10, the estimated annual fatalities at this same facility drop to 0.4948 individuals per turbine. By year 30, the model estimates 0.0341 annual fatalities per turbine for this same facility.

4.2 | Example 2: Indiana Bats

Using vague priors, the EoAR model applied to INBA fatalities yielded an intercept of -4.590433 with standard deviation of 1.278849. This corresponds to a rate parameter estimate of $\lambda = 0.01015$ INBA per turbine per year with standard deviation 0.0156437. The model estimated a total of 9 INBA fatalities over the subset of facilities in INBA range that were monitored during 2015 and 2016 (10 facility-seasons). The model with vague priors estimated a 95% upper bound for the total number of INBA fatalities to be 37 individuals. Using EoAR, we can be 95% sure that the monitored turbines killed fewer than 37 INBA total even though field efforts only produced a single INBA carcass, .

The species composition approach produced a higher estimate of INBA mortality than the vague EoAR model, but lower standard deviation. Using Equations 4 and 5, we set the mean and standard deviation of the INBA intercept's normal prior distribution to -3.0831799 and 0.6057304, respectively, and re-estimated the intercept's posterior distribution. The informed INBA model's estimate of mortality increased slightly relative to the vague model, but not appreciably. The informed model yielded an intercept of -4.537222 with standard deviation of 1.278384, corresponding to $\lambda = 0.01070$ INBA per turbine per year and standard deviation 0.01453235. The informed model's estimate of total mortalities remained 9 INBA fatalities, but the 95% upper bound dropped slightly to 35 individuals.

In this case, the models using vague and informed priors produced practically equivalent estimates. This was expected due to the relatively high standard deviation of the species composition approach. In other words, the species composition method did not provide much information to the EoAR model.

4.3 | Simulation Results

Simulations containing a single low event rate produced uni-modal distributions for both $\hat{\lambda}$ and estimation errors that surround the total number (Figure 2). The distributions of $\hat{\lambda}$ were discrete for $m = 20$ or $\mu_g = 0.1$, but otherwise showed decreasing bias and increasing precision as either m or μ_g increased (Figure 2, Table 4). The distribution of estimation errors, $\hat{M} - M$, was centered on 0 and only slightly skewed right (slight underestimation) for the low level of m combined with the low level of μ_g.

Simulations that contained one low and one high rate (λ_L and λ_H) also showed uni-modal distributions centered over the correct values of 1.0 and 10.0 (Figure 3). In fact, the distribution of the estimator for λ_L only overlapped that of the estimator for λ_H when m was low (20 sites) and $\mu_g = 0.1$. The distribution of estimation errors surrounding the true value of M was

uni-modal and symmetric about zero in all cases despite the fact that true M was a mixture of Poisson distributions with means equal to $1(m/2)$ and $10(m/2)$. Bias of both $\hat{\lambda}$ and $\hat{M}$ decreased as m or μ_g increased.

5 | DISCUSSION

The regression-type approach of EoAR provides increased flexibility relative to techniques that do not allow covariates, such as the Horvitz-Thompson (Huso, 2011) and straight EoA methods (Huso et al., 2015). Typically, analyses of wind-facility post-construction carcass counts estimate a mean rate and apply it to an entire facility or set of facilities. This approach is acceptable when all habitats and covariate combinations within or across facilities have been observed. When all turbines in a facility, or all turbines in all facilities, receive equal search effort, analysts can be reasonably comfortable that all covariate combinations were observed and that adjustment of a mean rate into the number of missed carcasses will yield accurate aggregate estimates. However, this mean-only approach is not adequate if important covariate combinations are not observed or if prospective prediction is required at facilities that are operated differently or that reside in substantially different environments. When fatalities differ, or search effort is not uniform within facilities, or across facilities, extrapolation of a constant rate to additional sites can seriously bias overall mortality estimates. In addition, constant rates do not apply when future operations or habitat are known to change. Selecting an *a priori* probability sample of sites certainly diminishes the possibility of failing to observe important covariate combinations, but does not eliminate it. A better approach, and one that is consistent across multiple disciplines of ecology, is to identify factors causing variation in counts and to use those factors to improve the accuracy and precision of estimates. EoAR facilitates this approach by estimating direct relationships between covariate values and carcass counts.

EoAR is not the only analytical technique that estimates direct covariate relationships. At first glance, logistic and Poisson regression are viable alternative analysis techniques for wind facility monitoring data. If fatalities are common, logistic and Poisson regression can certainly be applied. Fortunately, wind-facility fatalities are often very rare and regular logistic regression is seriously biased in these cases (King and Zeng, 2001). Moreover, logistic regression cannot be applied when researchers find no carcasses (Huso, 2011). Assuming no complete or quasi-complete separation issues (Albert and Anderson, 1984), weighted logistic regression (King and Zeng, 2001) can produce unbiased estimates of rare event probabilities after exogenous information on the background proportion of successes in the population is incorporated into weights. That is, extra information not found in the count data is required.

Like weighted logistic regression, EoAR also requires extra information in the form of detection probabilities (g). However, unlike weighted logistic regression, EoAR's extra information does not require abundance or a population-level proportion of success. EoAR only requires outside information on g, and g is usually easy to compute from the study design even if searcher efficiency and carcass removal trials are required.

Aside from being easy to compute because abundance is not required, the amount of information injected into an EoAR model by exogenous information on detection probabilities (g) is substantial and solves a number of problems associated with other techniques. First, outside information on detection makes a variable total (in this paper denoted M_T) estimable in N-mixture models. Without an outside estimate of detection rate, information about variable totals and detection probabilities are confounded in the N-mixture model and cannot be separated (Barker et al., 2018). If true counts and detection probabilities are constant, an N-mixture model can estimate both M_T and g from a data set containing multiple observations because the first two sample moments are sufficient to estimate the two parameters of the binomial distribution. Second, information provided by detection probabilities allows an EoAR model to be fitted when the data is completely or quasi-completely separated among covariate combinations. With information on detection, zero percent success in one or more covariate combinations is a valid observation and it can be adjusted correctly. Complete and quasi-complete separation issues stymie logistic and Poisson regression models. EoAR is then a viable method for resolving complete and quasi-complete separation problems in binomial models if detection probabilities (i.e., g) are available from some outside source. A couple methods for resolving complete or quasi-complete separation that do not require detection probabilities are reviewed in (Heinze, 2006).

Detection probability g is, in concept, similar to the detection probability parameters in other ecological field sampling techniques like distance sampling (Borchers, Zucchini, and Fewster, 1998; Buckland, Rexstad, et al., 2015; Buckland, Anderson, et al., 2004), occupancy analyses (MacKenzie and Kendall, 2002; MacKenzie, Nichols, Lachman, et al., 2002; MacKenzie, Nichols, Sutton, et al., 2005), and capture-recapture methods (Amstrup, McDonald, and Manly, 2005; Borchers and Efford, 2008; Efford, Borchers, and Byrom, 2009). Those methods, and EoAR, require probability of detection conditional on availability (i.e., P(*detect*|*available*)) to adjust observed counts for missed targets. The difference between distance, occupancy, and

capture-recapture analysis and EoAR is that the former techniques estimate detection probabilities from the same data available for abundance estimation. Indeed, one could argue that the key feature of these non-EoAR techniques is that they estimate probability of detection from a single data set so that observed counts can be adjusted into density, occupancy, or abundance. EoAR uses study design elements such as search timing, search frequency, and the proportion of carcasses sampled, along with measured quantities like average carcass lifetime and searcher efficiency, to estimate probability of detection given presence.

If components of g are unknown (e.g., carcass persistence rates), computation of EoAR's detection probability can be difficult. EoAR requires at least that detection probabilities be bracketed by lows and highs prior to analysis. If low and high values are identified, the variation in g between the lows and high can be built into g's Beta distribution by setting α and ψ accordingly. If absolutely no information is available on the range of detection probabilities, it is tempting to hypothesize a uniform distribution for g and set both α and ψ to 1.0. However, in this special case no additional information is injected into the model and the convergence of λ (and M_T) depends solely on variation in the observed counts across covariate combinations. In the case when absolutely no information on g is available, practitioners should probably reject our model in favor of standard logistic or Poisson regression and pool counts until $C_i > 0$ for all covariate combinations.

In addition to its other features, EoAR is useful for planning and study design. Studies can generally control probability of detection, at least absent budgetary constraints, and this allows planners to limit or control uncertainty in the form of credible interval widths. Studies that can accept large amounts of uncertainty can design data collection efforts that result in low g values because low g values generally produce wide credible intervals. Studies that require high precision surrounding the total number of fatalities must allocate effort wisely and design data collection efforts to achieve high g values. At a minimum, study designers can hypothesize various g values, compute credible interval widths and budgetary requirements associated with each, and weigh the associated field costs against precision of the final estimates. For planning purposes, we emphasize that EoAR's coefficient distributions can become more informative over time, and this feature, when implemented correctly, will maximize precision levels.

In summary, EoAR provides viable estimates and upper-bounds for the number of fatalities in all situations but most notably when no or few fatalities are observed. EoAR has the desirable feature of relating fatality rates to study covariates, which in turn increases precision. An EoAR model is robust to quasi-complete separation issues and is relatively easy to compute using the MCMC routines provided in Appendix S1. The only drawback of EoAR is that separate estimates of detection probabilities are required, and for wind facility monitoring these require additional field trials to establish searcher efficiency and carcass removal. This addition information on detection probabilities is worth acquiring because doing so generally requires less effort than acquiring separate information required by alternative techniques.

6 | ACKNOWLEDGEMENTS

The authors wish to thank Kraig McPeek, U.S. Fish and Wildlife Service, for his encouragement and insightful comments. We also wish to thank the dozens of Western EcoSystems Technology and MidAmerican field personnel who diligently collected the field data used in the examples.

7 | CONFLICT OF INTEREST

Western EcoSystems Technology, Inc., the firm employing T. McDonald, K. Bay, and J. Studyvin, has been under contract with MidAmerican Energy, the employer of J. Leckband, since 2015 to collect post-construction monitoring data at the 19 wind power facilities used in the examples. Berkshire Hathaway Energy, the employer of J. McIvor, is the parent company of MidAmerican. Western EcoSystems Technology, Inc., was also contracted to assist MidAmerican with drafting and submitting a Habitat Conservation Plan (HCP) covering take of endangered birds and bats at their facilities in Iowa. J. McIvor and J. Leckband are project managers charged with overseeing much of the HCP submission. Beyond J. McIvor and J. Leckband, no one at Berkshire Hathaway or MidAmerican received an advance copy of the manuscript nor exerted editorial or content control. Author A. Schorg is employed by the U.S. Fish and Wildlife Service, Region 3, which has regulatory responsibility for endangered bird and bat management in Iowa. Beyond A. Schorg and Kraig McPeek, no one employed by the U.S. Fish and Wildlife Service or the U.S. government received an advance copy of the manuscript nor exerted editorial or content control. The findings and conclusions in this article are those of the author(s) and do not necessarily represent the views of Western

EcoSystems Technology, Inc., Berkshire Hathaway Energy Company, MidAmerican Energy Company, or the U.S. Fish and Wildlife Service.

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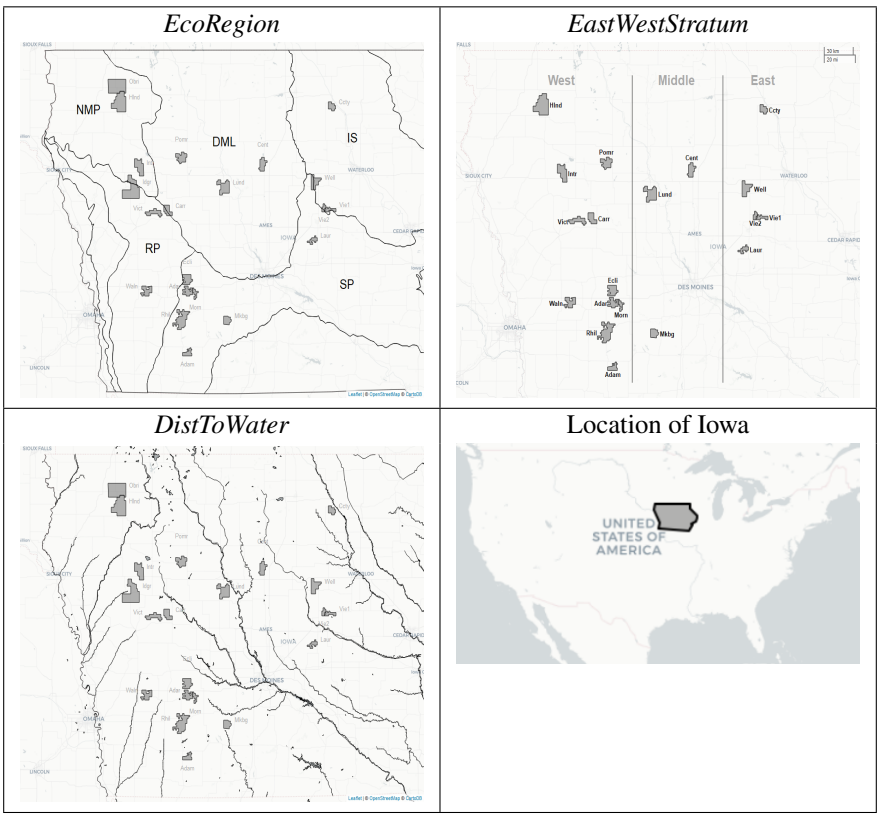

FIGURE 1 Geographic covariates used in Example 1. Upper-left panel shows ecological sub-regions. Upper-right panel shows the east-west stratum covariate boundaries. Lower left panel shows rivers and lakes in Iowa used to derive the distance to water covariate. Grey blocks are wind facilities. Site abbreviations and corresponding covariate values appear in Table 1.

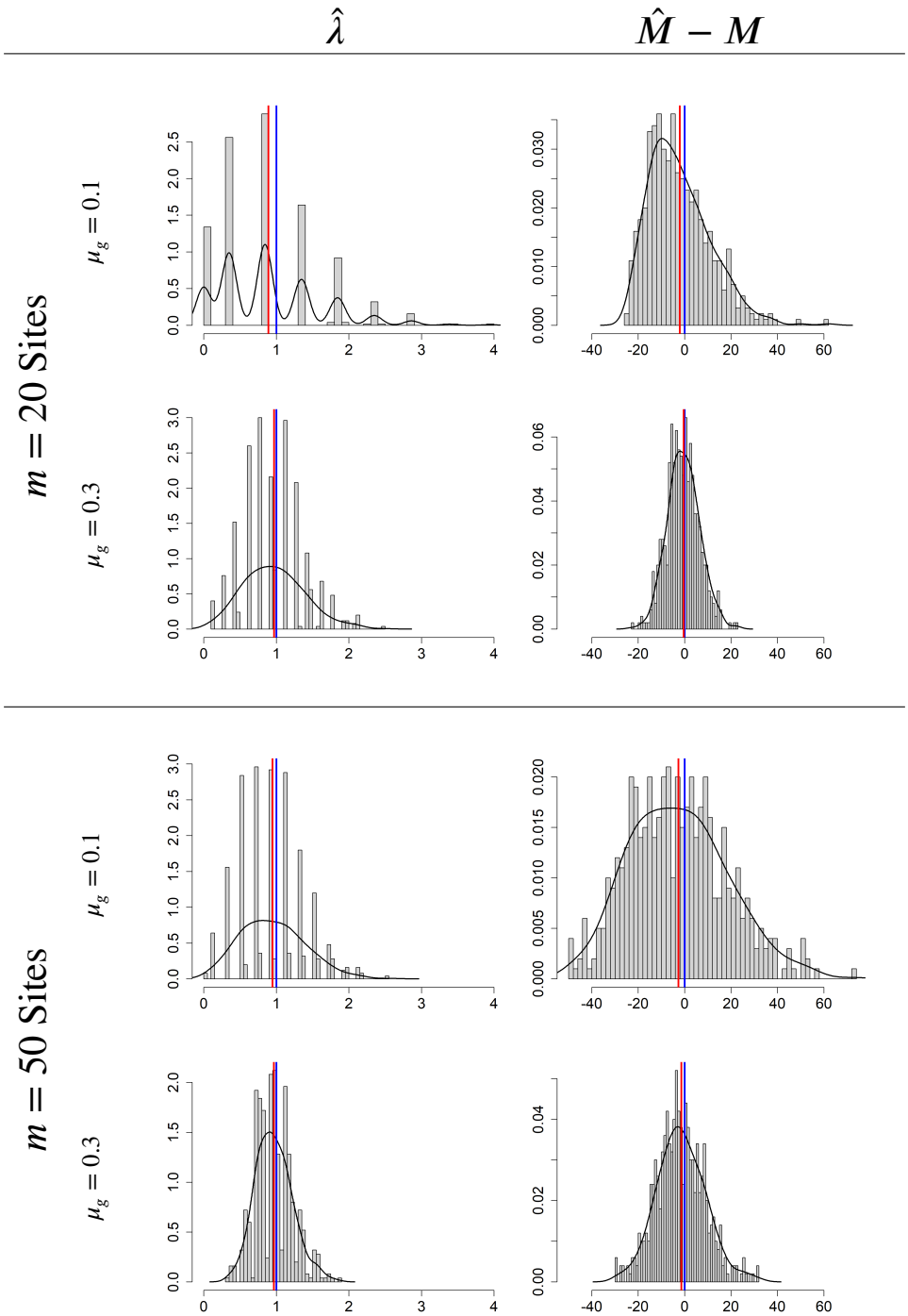

FIGURE 2 Histograms of estimated event rate ($\hat{\lambda}$) during simulations of a unimodal distribution of λ, and distribution of estimation errors for total number of events ($\hat{M} - M$). Vertical blue lines are the true parameter values. Vertical red lines mark the mean of simulation estimates. Black lines are a (Gaussian) kernel density estimate for the distribution.

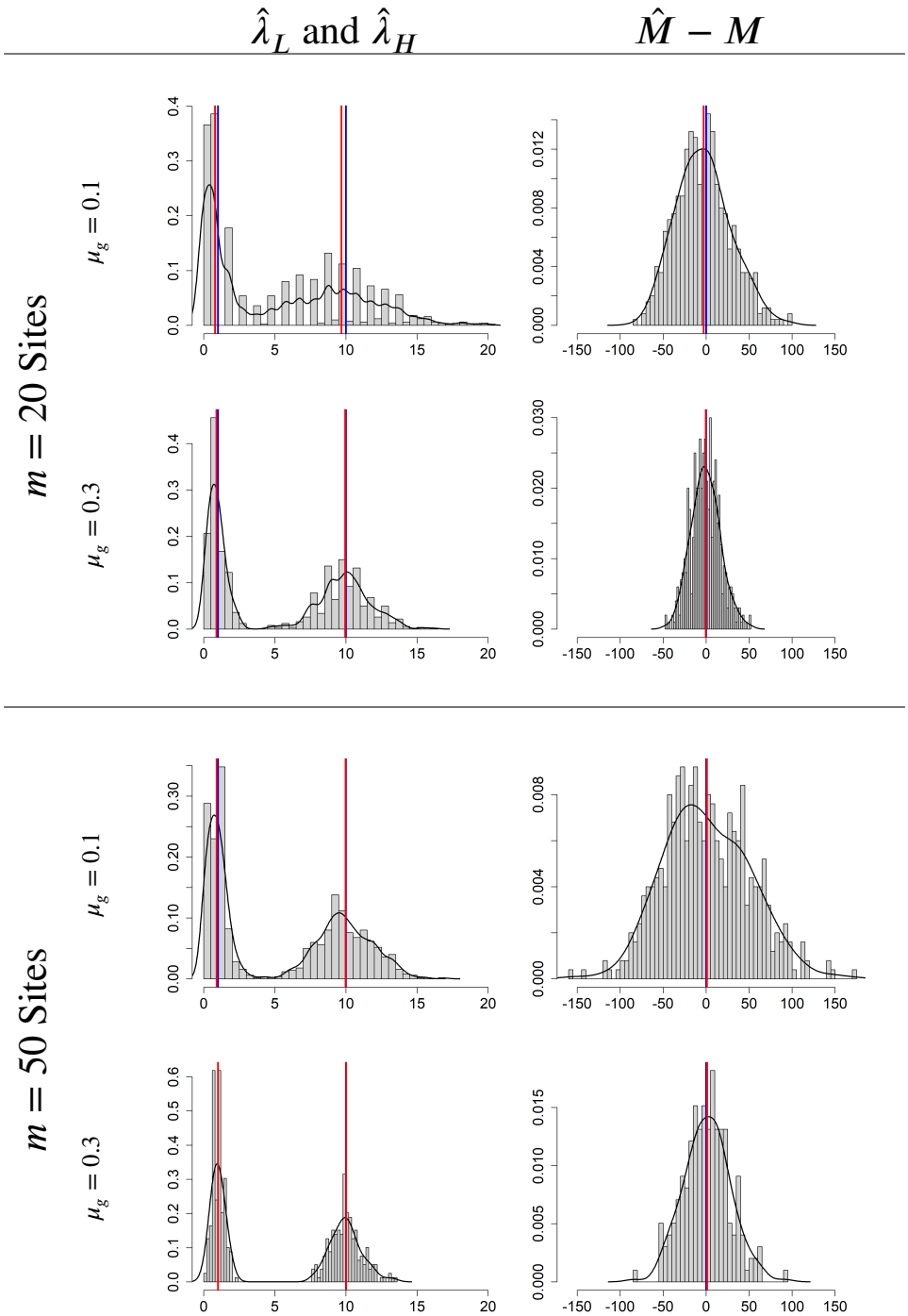

FIGURE 3 Histograms of estimated event rate ($\hat{\lambda}_L$ and $\hat{\lambda}_H$) during simulations of a bimodal distribution for λ, and distribution of estimation errors for total number of events ($\hat{M} - M$). Vertical blue lines are the true parameter values. Vertical red lines mark the mean of simulation estimates. Black lines are a (Gaussian) kernel density estimate for the distribution.

TABLE 1 Wind facility monitoring data collected in Iowa (USA). **LBBA** and **INBA** columns contain, respectively, the number of little brown bat (*Myotis lucifugus*) and Indiana bat (*Myotis sodalis*) carcasses found at the site during all visits that year. *gAlpha* and *gBeta* are parameters of the Beta distribution for *g* estimated by R package `GenEst`. ***Turbines*** is number of wind turbines at the site. ***DistToWater*** is the average distance of turbines to nearest water (river, lake, or swamp) in kilometers. ***EastWestStratum*** is east-west stratum membership. ***EcoRegion*** is ecologic sub-region of the site. ***SiteAge*** is number of years the facility has been operating.

Site[†]	Year	LBBA	INBA[⋆]	*gAlpha*	*gBeta*	*Turbines*	*DistToWater*	*EastWestStratum*[⋄]	*EcoRegion*[‡⋄]	*SiteAge*
Adam	2016	0	-	125.077	901.55	64	9.94 km	west	SP	1 yr
Adar	2015	0	0	56.092	1766.96	76	8.93	west	SP	7
Carr	2015	0	-	55.386	2131.74	100	12.94	west	DML	7
Ccty	2016	13	-	110.853	625.14	50	5.15	east	IS	9
Cent	2016	1	-	90.981	575.61	145	7.83	middle	DML	11
Ecli	2015	0	0	55.386	1355.14	87	15.82	west	SP	3
Hlnd	2016	1	-	138.798	1062.64	214	19.26	west	NWP	1
Idgr	2017	0	-	272.637	1265.42	134	8.82	west	RP	1
Intr	2016	0	-	125.917	1267.52	122	13.32	west	NWP	12
Laur	2016	1	0	68.099	722.81	52	16.26	east	SP	5
Lund	2015	22	-	54.179	1479.54	107	10.13	middle	DML	1
Lund	2016	4	-	128.993	751.04	107	10.13	middle	DML	2
Mkbg	2015	8	0	30.077	1136.03	51	5.68	middle	SP	1
Mkbg	2016	6	1	63.769	317.80	51	5.68	middle	SP	2
Morn	2015	0	0	50.989	1911.42	44	15.60	west	SP	3
Obri	2017	0	-	70.265	2229.99	104	25.58	west	NWP	1
Pomr	2016	0	-	70.535	715.56	184	11.47	west	DML	8
Rhil	2015	0	0	68.642	1552.22	193	14.62	west	SP	4
Rhil	2016	1	0	80.627	715.40	193	14.62	west	SP	5
Vict	2015	0	-	48.391	1513.95	66	16.34	west	RP	9
Vie1	2016	3	0	84.440	506.45	45	4.92	east	IS	4
Vie2	2016	2	0	82.614	485.38	19	9.45	east	IS	3
Waln	2015	0	-	59.233	1774.35	102	10.05	west	RP	7
Well	2016	7	-	105.654	689.03	60	7.73	east	IS	2

[†] Adar = Adair; Adam = Adams; Carr = Carroll; Cent = Century; Ccty = Charlescity; Ecli = Eclipse; Hlnd = Highland; Idgr = Idagrove; Intr = Intrepid; Laur = Laurel; Lund = Lundgren; Mkbg = Macksburg; Morn = Morninglight; Obri = Obrien; Pomr = Pomeroy; Rhil = Rollinghills; Vict = Victory; Vie1 = Viennai; Vie2 = Viennaii; Waln = Walnut; Well = Wellsburg [⋆] Counts are missing (-) at facilities outside the official INBA range. [⋄] see Figure 1
[‡] SP = Southern Iowa Rolling Loess Prairies; DML = Des Moines Lobe; RP = Loess Hills and Rolling Prairies; IS = Iowan Surface; NWP = Northwest Iowa Loess Prairies

TABLE 2 All possible three-variable models relating covariates (Table 1) to LBBA mortality rates after adjustment for detection and facility size. Fleet-wide mortality estimates and 95% confidence limits are $\hat{M}$, $\hat{M}_{low}$, and $\hat{M}_{high}$, respectively. Model 2 (bolded) was chosen as the final model for reasons given in the text.

Model	Structure	DIC	$\hat{M}$	$\hat{M}_{low}$	$\hat{M}_{high}$
1	$\log(\lambda) \sim$ *EastWestStratum* + *EcoRegion* + *SiteAge*	77.02	778	644	931
2	**$\log(\lambda) \sim$ *DistToWater* + *EastWestStratum* + *SiteAge***	**77.67**	**779**	**641**	**944**
3	$\log(\lambda) \sim$ *DistToWater* + *EastWestStratum* + *EcoRegion*	81.06	781	643	944
4	$\log(\lambda) \sim$ *EastWestStratum* + *SiteAge*	82.21	777	635	927
5	$\log(\lambda) \sim$ *EastWestStratum* + *EcoRegion*	82.88	777	636	938
6	$\log(\lambda) \sim$ *DistToWater* + *EastWestStratum*	89.03	780	639	932
7	$\log(\lambda) \sim$ *EastWestStratum*	91.78	781	640	940
8	$\log(\lambda) \sim$ *DistToWater* + *EcoRegion* + *SiteAge*	91.82	783	638	932
9	$\log(\lambda) \sim$ *DistToWater* + *EcoRegion*	124.25	781	642	928
10	$\log(\lambda) \sim$ *EcoRegion* + *SiteAge*	126.30	776	640	934
11	$\log(\lambda) \sim$ *DistToWater* + *SiteAge*	129.76	776	637	932
12	$\log(\lambda) \sim$ *DistToWater*	142.39	774	642	937
13	$\log(\lambda) \sim$ *EcoRegion*	150.68	773	638	936
14	$\log(\lambda) \sim$ *SiteAge*	204.01	775	627	936
15	$\log(\lambda) \sim$ (Intercept)	219.94	782	633	939

TABLE 3 Coefficients in the second-ranked (by DIC) EoAR model in Table 2. The model relates LBBA fatalities to *DistToWater*, the *EastWestStratum* factor, and *SiteAge*.

Effect	Estimate	SD	CI_5	CI_{95}
(Intercept)	1.6691	0.4617	0.9052	2.4124
DistToWater	-0.1036	0.0481	-0.1871	-0.0284
EastWestStratum.middle	-0.3502	0.2561	-0.7649	0.0780
EastWestStratum.west	-4.1858	0.8823	-5.7975	-2.9235
SiteAge	-0.1337	0.0437	-0.2139	-0.0681

TABLE 4 Percent bias in $\hat{\lambda}$ and $\hat{M}$ computed during both sets of simulations.

Simulation	True λ	m	$\hat{\lambda}$ %Bias	$\hat{M}$ % Bias
1	1	20	-11.03	-10.34
1	1	20	-3.18	-2.13
1	1	50	-5.15	-5.32
1	1	50	-3.30	-2.90
2	1	20	-20.77	-2.74
2	1	20	-10.79	-0.52
2	1	50	-8.73	0.35
2	1	50	-0.16	0.42
2	10	20	-3.16	-
2	10	20	-0.47	-
2	10	50	-0.31	-
2	10	50	0.15	-

APPENDIX S1

EVIDENCE OF ABSENCE REGRESSION: A BINOMIAL N-MIXTURE MODEL FOR ESTIMATING BIRD AND BAT FATALITIES AT WIND POWER FACILITIES

Trent McDonald, Kimberly Bay, Jared Studyvin, Jesse LeckBand, Amber Schrog, and Jennifer McIvor
January 22, 2020

S1.1 | EOAR'S DIRECTED ACYCLIC GRAPH

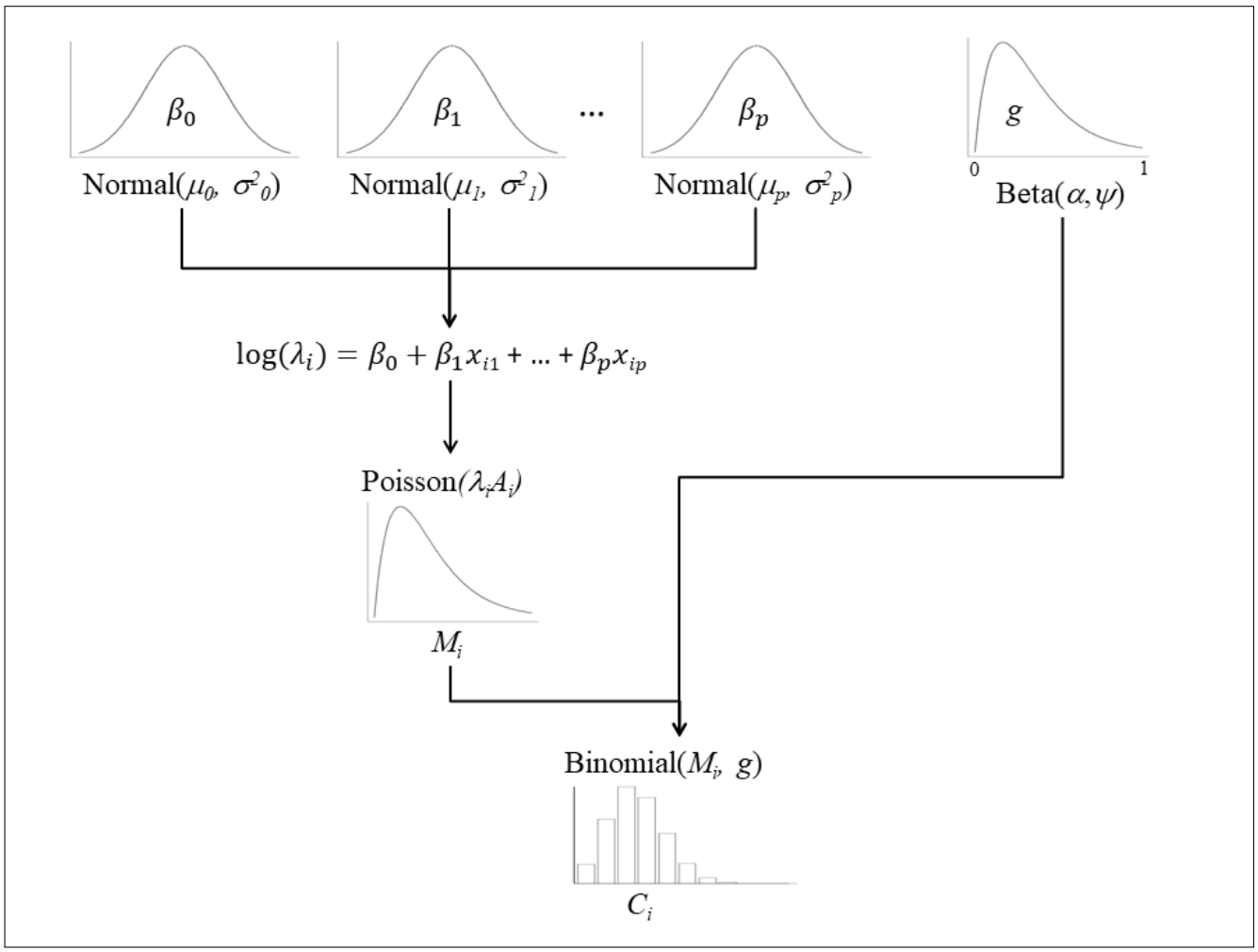

FIGURE S1.1 A directed acyclic graph of the Evidence of Absence Regression (EoAR) model loosely based on the directed acyclic graphs of Kruschke (2011). Top row of distributions represent the priors and can be either vague or informed. Data are carcass counts C_i. Scaling offsets are A_i. Beta parameters α and ψ must be estimated from the study design (e.g., with routines in R package `GenEst`).

S1.2 | EOAR CODE

R code to estimate an EoAR model is available from the author's GitHub site : `https://github.com/tmcd82070/evoab`

S1.3 | SIMULATION CODE

S1.3.1 Figure 2 Code

This section contains R code used to carry out the simulations associated with Figure 2 of the main text. Unmodified, this code carries out additional simulations for higher values of λ than those plotted in Figure 2. Plots of simulations for higher values of λ (i.e., $\lambda = 10$, 25, and 50) show similar results.

```
lambda <- c(1,10,25,50)
nFacilities <- c(20,50)
g <- c(.1,.3)
var.g <- c(.005,.005)
R <- 500

# ----------------------

library(evoab)
nl <- length(lambda)
nn <- length(nFacilities)
ng <- length(g)

M.true <- array(NA,c(nl,nn,ng,R))
M.ests <- array(NA,c(nl,nn,ng,R))
l.ests <- array(NA,c(nl,nn,ng,R))

dimnames(M.true) <- list(lambda, nFacilities, g, NULL)
dimnames(M.ests) <- list(lambda, nFacilities, g, NULL)
dimnames(l.ests) <- list(lambda, nFacilities, g, NULL)

for( ll in 1:nl){
  for( nn in 1:nn){
    for( gg in 1:ng){

      pBa <- g[gg]*(g[gg]*(1-g[gg])/var.g[gg] -1)
      pBb <- (1-g[gg])*(g[gg]*(1-g[gg])/var.g[gg] -1)
      b.parms <- data.frame(alpha=pBa, beta=pBb)

      for( iter in 1:R){
        M <- rpois(nFacilities[nn],lambda[ll])

        C <- rbinom(nFacilities[nn], M, g[gg])

        fit <- eoa(C~1,b.parms,nburns = 100000)

        M.ests[ll,nn,gg,iter] <- fit$estimates["Mtot",1]
        l.ests[ll,nn,gg,iter] <- fit$estimates["lambda[1]",1]
        M.true[ll,nn,gg,iter] <- sum(M)
      }
    }
  }
}
```

```
save(M.ests, l.ests, M.true, lambda, nFacilities, g,
     var.g, file="SimA.RData")
```

S1.3.2 Figure 3 Code

This section contains the R code for simulations associated with Figure 3 of the main text. Unmodified, this code carries out additional simulations for higher values of λ than those plotted in Figure 3. Plots of simulations for higher values of λ_L and λ_H (i.e., $\lambda_L = 10$ and $\lambda_H = 20$) show similar results.

```
lambda1 <- c( 1,10)
lambda2 <- c(10,20)
nFacilities <- c(20,50)
g <- c(.1,.3)
var.g <- c(.005,.005)
R <- 500

# ----------------------

library(evoab)
nl <- length(lambda1)
nn <- length(nFacilities)
ng <- length(g)

M.true <- array(NA,c(nl,nn,ng,R))
M.ests <- array(NA,c(nl,nn,ng,R))
l1.ests <- array(NA,c(nl,nn,ng,R))
l2.ests <- array(NA,c(nl,nn,ng,R))
betas <- array(NA,c(nl,nn,ng,R))

ldims <- paste0(lambda1,",",lambda2)
dimnames(M.true) <- list(ldims, nFacilities, g, NULL)
dimnames(M.ests) <- list(ldims, nFacilities, g, NULL)
dimnames(l1.ests) <- list(ldims, nFacilities, g, NULL)
dimnames(l2.ests) <- list(ldims, nFacilities, g, NULL)
dimnames(betas)  <- list(ldims, nFacilities, g, NULL)

for( ll in 1:nl){
  for( nn in 1:nn){
    for( gg in 1:ng){

      pBa <- g[gg]*(g[gg]*(1-g[gg])/var.g[gg] -1)
      pBb <- (1-g[gg])*(g[gg]*(1-g[gg])/var.g[gg] -1)
      b.parms <- data.frame(alpha=pBa, beta=pBb)

      for( iter in 1:R){
        nlow <- round(nFacilities[nn]/2)
        nhi <- nFacilities[nn] - nlow
        X <- factor(c(rep("low",nlow), rep("hi",nhi)))

        MLow <- rpois(nlow,lambda1[ll])
        MHi  <- rpois(nhi,lambda2[ll])
        M <- c(MLow, MHi)
```

```
        C <- rbinom(nFacilities[nn], M, g[gg])

        fit <- eoa(C~X,b.parms,nburns = 75000)

        ind.low <- paste0("lambda[",nhi+1,"]")
        M.ests[ll,nn,gg,iter] <- fit$estimates["Mtot",1]
        l1.ests[ll,nn,gg,iter] <- fit$estimates["lambda[1]",1]
        l2.ests[ll,nn,gg,iter] <- fit$estimates[ind.low,1]
        M.true[ll,nn,gg,iter] <- sum(M)
        betas[ll,nn,gg,iter] <- fit$estimates[fit$coef.labels[2],1]
      }
    }
  }
}

save(M.ests, l1.ests, l2.ests, betas, M.true,
  lambda1, lambda2,nFacilities, g, var.g, file="Sim02.RData")
```