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Research Article

Estimating Wind Turbine Fatalities Using Integrated Detection Trials

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ABSTRACT Fatality monitoring at wind projects requires carcass detection trials to adjust fatality estimates for the proportion of fatalities not found. However, detection trials vary greatly in metric, duration, carcass monitoring schedule, species, number placed, state of decomposition, whether placed within or outside search areas, and other factors. We introduce a new approach for estimating fatalities by quantifying overall detection rates rather than separate rates for searcher detection error and carcass persistence, and by leaving placed and found fatality carcasses undisturbed throughout monitoring. We placed 2 fresh-frozen bird carcasses weekly at random sites within fatality search areas and on randomized days Monday–Friday at Sand Hill and Santa Clara wind projects, Altamont Pass Wind Resource Area, California, USA. To estimate detection rates, we used logit regression to fit detection outcomes on body mass, which served as an axis of similitude between placed trial carcasses and fatality finds. Adjusted carcass placement rates among species detected by searchers regressed on true placement rates with a slope of 1.0 so long as sufficient numbers of trial carcasses were placed, thus validating our approach as an unbiased estimator. Our approach generally estimated lower fatality rates than did conventional approaches, the latter of which demonstrated biases in searcher detection rates and carcass persistence rates whether based on proportion of carcasses remaining or mean days to removal. Our approach also revealed detection errors that highlight the difficulty of finding and identifying the remains of dead animals, and which warrant routine reporting. Despite averaging only a 5-day search interval on intensively grazed annual grasslands where ground visibility was usually high, our experienced fatality monitors averaged 4.3 searches/first carcass detection, failed to detect 25% of 75 species represented by placed carcasses, and misidentified carcasses to species among 44% of species detected. Estimates of time since death also suffered bias and large error. Our approach more realistically simulates carcass detection probabilities associated with fatality monitoring, is less costly, facilitates hypothesis testing, eliminates multiple sources of error and bias suspected of conventional methods, and enables quantification of errors in estimated time since death, species identifications, and false negative findings.

KEY WORDS bird fatalities, carcass persistence, fatality estimates, integrated detection trials, searcher detection, wind turbine collisions.

Fatality monitoring at wind energy projects typically involves trials to estimate rates of searcher detection and carcass persistence. Carcass placement trials are needed to adjust fatality estimates for the portion of fatalities not detected during monitoring. Rogers et al. (1977), Winkelman (1989), and Orloff and Flannery (1992) were among the first to place bird carcasses in fatality search areas around wind turbines to estimate searcher detection and carcass persistence rates. Gauthreaux (1995) suggested standardizing fatality monitoring so that results would be comparable among wind projects or through time. However, other than Morrison's (1998) suggested standard practice of removing found carcasses to prevent double counting, little standardization of field methods exists.

In fact, field methods vary and this variation diminishes comparability of fatality estimates (Smallwood 2013). Fatality search intervals range from 1 to 90 days among North American studies, greatly affecting rates of false negatives in fatality reporting. A study comparing fatality

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rates at turbines searched concurrently by one team at 5-day intervals and another at 39-day intervals reported that the team searching the longer interval detected only 37% of the species and 28% of the total fatalities detected during the shorter interval, resulting in over- and under-estimates and a shorter list of species affected (Smallwood 2017). Maximum search radii vary among studies. For a given turbine size, a search radius of 50 m covers only 17.4% of the area relative to a search radius of 120 m, potentially resulting in a nearly 6-fold range of fatality finds if in fact carcasses are uniformly deposited up to 120 m from the turbine. Transect spacing within search areas range 4 m to 20 m, so if the effective detection distance for a small bird or bat carcass is 2 m from the transect, then the effective search coverage, assuming similar ground cover, would range 20% to 100% for a 5-fold range in fatality finds. The numbers, species, and conditions of carcasses placed in searcher detection and carcass persistence trials also vary greatly. For example, using house sparrows (*Passer domesticus*) as surrogates for bats can inflate detection rates 4-fold for bats of similar body size (K. S. Smallwood, independent researcher, unpublished data). Choices in field methods can lead to differences in fatality rate estimates, and these differences exceed those attributed to mathematical structure and stated assumptions among available estimators (Korner-Nievergelt et al. 2011).

Much debate over methodology has centered on fatality estimators derived from Horvitz and Thompson (1952), mostly focused on how to express the proportions of fatalities not found because of searcher error and carcass disappearance. In one type of estimator (Fig. 1A), Winkelman (1989) and Orloff and Flannery (1992) divided the number of found fatalities (f) by the proportion of placed and available-to-be-found carcasses actually found by searchers (S) and the proportion of placed carcasses persisting to the next search (R). Smallwood (2007) modified R by adding a daily averaging function (R_C) to account for an assumed constant deposition rate of carcasses through a trial period intended to equal the average fatality search interval (I). In another type of estimator (Fig. 1B), Johnson et al. (2002), P. S. Shoenfeld (West Virginia Highlands Conservancy, unpublished report), and Huso (2010) used mean days to carcass removal to estimate the probability that a trial carcass would remain to be found (r). Estimators based on proportion of carcasses remaining by the end of a search interval tacitly assume that no carcasses persist beyond the search interval to be detected during a subsequent search. Estimators based on mean days to carcass removal ($\hat{\bar{t}}$) often assume exponential carcass persistence through the search interval (I): $r = e^{\frac{-I}{\hat{\bar{t}}}}$ (Huso 2010, Warren-Hicks et al. 2013), but this assumption is unrealistic for carcasses persisting >8 days because vertebrate scavengers remove fewer older carcasses (Smallwood et al. 2013). For both types of estimators, an assumed steady rate of carcass deposition is probably unrealistic because of seasonal and annual variation in fatalities.

The estimators described thus far rely on trial carcass checks, which are tacitly assumed to accurately characterize carcass persistence and availability to searchers. However, trial administrators search relatively small areas around placement sites because of awareness of the sites, and scavengers sometimes relocate carcass remains elsewhere within the fatality search area unbeknownst to the trial administrator. Falsely concluding trial carcasses were removed will bias searcher detection rates high by reducing the number of available carcasses to be found, and it will bias carcass persistence rates low or high depending on which carcass was falsely determined as removed. Conventional detection trials are vulnerable to errors and bias, such as trial administrators and fatality searchers applying different fatality determinations to carcass remains.

As potential errors and bias emerged in discussions around fatality estimation (Smallwood 2007; Smallwood et al. 2010, 2013; Huso and Erickson 2013), investigators sought to rectify the problems of bias using statistical methods (Bispo et al. 2010, Huso 2010, Korner-Nievergelt et al. 2011, Péron et al. 2013, Huso et al. 2015), whereas field methods remained largely unaddressed. However, if the data used for fatality estimators are poorly founded, then the estimates will be poorly founded regardless of the estimator. Smallwood et al. (2010) introduced field methods to improve understanding of carcass persistence, and Warren-Hicks et al. (2013) introduced an integration of detection trials into routine monitoring. Smallwood (2013) and Smallwood et al. (2013) suggested estimating an overall detection rate rather than separate rates of searcher detection and carcass persistence, an approach we implemented here.

We introduce an integrated carcass detection trial for estimating overall detection rates needed to adjust fatality rate estimates for the proportion of carcasses not found during monitoring. We hypothesized that body mass would explain most of the variation in detection trial outcomes in routine fatality monitoring relying on human searchers, and that body mass could predict most of the adjustment needed for fatality rate estimates. We tested whether fatality rates estimated from our new approach differed from those of the conventional approaches. To help explain differences in fatality rates between the approaches, we compared conventional fatality adjustment factors to aspects of field methods used to derive them. We compared searcher detection rates of carcasses placed 1 day and >1 day prior to the fatality search. We also compared carcass persistence rates derived from proportion of carcasses remaining versus mean days to carcass removal, and we related results from both of these conventional approaches to trial duration and body mass to reveal potential biases. We hypothesized that our new approach would facilitate validation of estimation accuracy by treating the placement data as faux fatality data and relating estimated placement rates to known placement rates among species. We further hypothesized that our new approach would improve quantification of error rates related to species identification and estimated time since death. Finally, we explored whether residual variation in detection outcomes logit-regressed on body mass could reveal patterns potentially useful for improving the efficacy of fatality monitoring.

STUDY AREA

We conducted this study at 2 wind projects in the Altamont Pass Wind Resource Area (APWRA) in Alameda County,

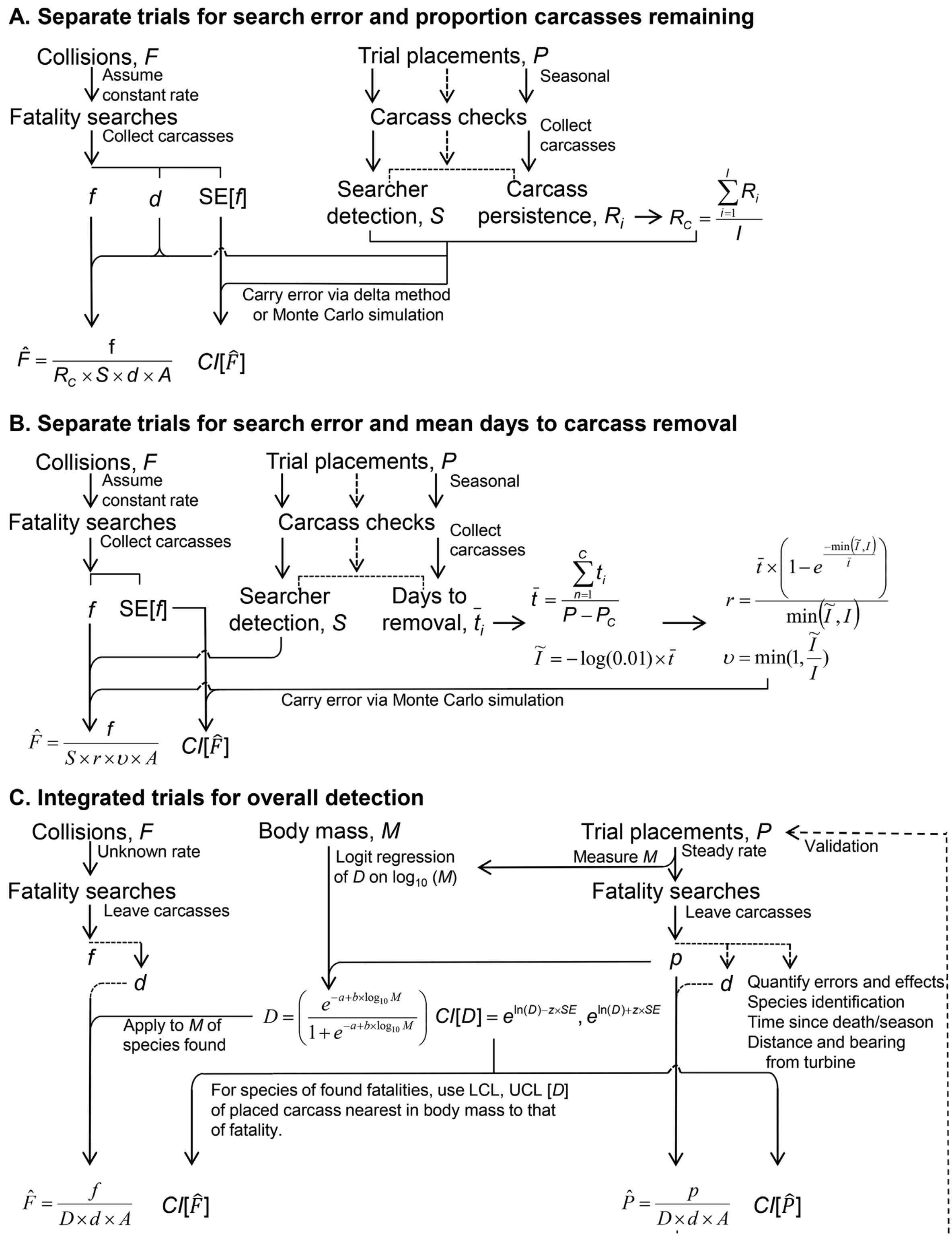

Figure 1. Comparison of conventional carcass detection trials (A and B) and the new integrated trials for estimating overall detection rates (C). Estimator A typifies Horvitz and Thompson (1952) as adopted by Smallwood (2007), B typifies Johnson et al. (2002) and later modifications by P. S. Shoenfeld (West Virginia Highlands Conservancy, unpublished report) and Huso (2010), but note that although the exponential persistence function in Huso (2010) is the most commonly depicted form of the estimator, other functions are possible. F = true number of fatalities, f = number of fatalities found by searchers, $\hat{F}$ = fatalities adjusted for the portion of undetected fatalities, P = number of placed trial carcasses, p = number of trial carcasses found by searchers, $\hat{P}$ = number of trial carcasses adjusted for the portion of undetected trial carcasses, P_C = number of trial carcasses remaining at end of trial period (curtailed trials), S = proportion of available trial carcasses found by searchers upon first search since placement, R_i = proportion of trial carcasses remaining on the ith day into a trial (carcass persistence rate), R_C = average proportion of trial carcasses remaining on the ith day into a trial corresponding with mean interval between fatality searches I and assuming a constant daily deposition rate, t_i = days to trial carcass removal, $\bar{t}$ = mean days to trial carcass removal, r = probability that a trial carcass would remain to be found, $\tilde{I}$ = effective search interval (applied to bat and small bird carcasses when such carcasses are not expected to last as long as the search interval), d = average proportion of carcasses found beyond the maximum search radius, A = proportion of area within maximum search radius that was actually searchable, D = proportion of carcasses predicted to be found based on proportion of trial carcasses found as a function of species' typical body mass M (a and b are fitted coefficients), SE = standard error.

California: Sand Hill, which occupied 3 areas separated by 2.6–3.4 km and totaled 354 ha at the eastern edge of the APWRA, and Santa Clara, consisting of 190 ha located about 3.4 km west of the Sand Hill site. Elevations at Sand Hill's rolling terrain ranged 61 m to 179 m, and at Santa Clara's steeper terrain ranged 252 m to 356 m. The climate was Mediterranean consisting of dry, hot summers with daily temperatures sometimes exceeding 37°C during July–August and mild, cool winters with extremely variable precipitation from October–April (https://www.usclimatedata.com). Ground cover at both projects consisted mostly of exotic annual grasses, which were grazed intensively by cattle. Grass height was short most of the year but sometimes grew to 75 cm in April. Where grazing was less intense, grasses tended to fall over and matt the ground by June. Common vertebrate scavengers of bird and bat carcasses included common raven (*Corvus corax*), turkey vulture (*Cathartes aura*), coyote (*Canis latrans*), red fox (*Vulpes vulpes*), gray fox (*Urocyon cinereoargenteus*), striped skunk (*Mephitis mephitis*), American badger (*Taxidea taxus*), red-tailed hawk (*Buteo jamaicensis*), great-horned owl (*Bubo virginianus*), and California ground squirrel (*Spermophilus beecheyi*; Smallwood et al. 2010).

The Sand Hill project included 403 wind turbines (23.123 MW), including 144 40-KW Enertech turbines in the original Altech I project, 12 65-KW Micon turbines in the original Swamp project, 183 65-KW Micon turbines in the original Taxvest project, 26 65-KW Micon turbines in the original Viking project, 26 65-KW Windmatic turbines in the original Venture Winds project, and 12 109-KW Polenko turbines at Venture Winds. On 21 April 2014 we added to our search rotation 36 95-KW Vestas wind turbines (3.42 MW) in the 200-turbine (19 MW) Santa Clara project to replace an equivalent capacity of Sand Hill turbines that we lost to attrition. All wind turbines in our study were shut down from 1 November through February as a measure to reduce collision fatalities, but we monitored the turbines for fatalities year-round.

METHODS

Estimating the number of fatalities caused by a wind energy project begins with a true number of fatalities (*F*) caused by wind turbines (Fig. 1). Investigators cannot know *F* because fatality monitoring will not detect all fatalities. Efforts are therefore made to estimate $\hat{F}$ from the number of found fatalities adjusted by the proportion of carcasses found in detection trials for which the number of trial placements (*P*) is known. Carcass detection trials are conducted in parallel to the fatality monitoring to simulate the detection probabilities associated with fatalities caused by wind turbines. Because the results of detection trials can substantially affect $\hat{F}$, it is critical that detection trials be designed and implemented to realistically simulate the detection probabilities of wind turbine fatalities. Our new approach involves 3 parallel series of steps that are integrated as part of the monitoring program (Fig. 1C).

Our integrated trials for overall detection start from the same true number of collision fatalities but diverge significantly from the conventional trials in design and execution of trial placements and carcass management. This divergence in design and execution of the trials aims to more realistically simulate the detection probabilities associated with fatality monitoring. All carcasses are left in the field indefinitely, whether found as routine fatalities or trial placements, because this is the normal fate of carcasses deposited by wind turbines in the absence of monitoring. Rather than performing separate trials for searcher detection and carcass persistence, only one set of trials is performed and trial carcasses are either found or not found by searchers. Also, other than any threshold time since death, there is no trial duration used to decide whether to include fatality finds in fatality estimates. To minimize the likelihood of scavenger swamping (Smallwood 2007, Smallwood et al. 2010), trial carcasses are placed in small groups every 7 to 14 days generally not exceeding 5 g body mass/ha/year on average.

Integrated detection trials include 2 design elements that are unrelated to realistically simulating detection probabilities but are necessary for informing the body mass model (central path of Fig. 1C). One need is to increase carcass placements as body size diminishes to provide searchers reasonable opportunities for detecting ≥1 member of a small-bodied species. Another need is to represent as many species as possible over a wide range of body sizes to better inform models of overall detection rates as functions of species' body mass. Prior to placement at randomized locations within search areas, carcasses should be weighed so that detection rates can be related to body mass. The resulting functions of overall detection rates on body mass are then projected to a database of body mass typical of each species occurring in the region. Overall detection rates can then be attributed to species found as fatalities by merging the data sets on species membership. This step is necessary because reliable measurements of body mass cannot be made from found fatalities, which often consist of decomposed or scavenged remains or feather piles. This use of body mass to adjust both *F* and *P* serves as an axis of similitude between the processing of collision fatalities and trial placements, similar to the axis of similitude in the allometry of animal density, in which body mass links variation in morphometric variables to variation in ecological patterns related to species' distribution and abundance (Smallwood 2001). Thus, body mass serves as a scaler of detection rates among species and as a proxy for species used in detection trials and those found as fatalities. A proxy is useful because the species found in fatality monitoring will unlikely match those used in detection trials, as one cannot have perfect, advanced knowledge of which species will be found as fatalities. In addition, carcasses of some species (e.g., golden eagle [*Aquila chysaetos*]) are unavailable for use in carcass trials because of regulatory restrictions.

Our study design relied on 4 years (2005–2009) of fatality data from earlier monitoring (ICF International, unpublished data) to identify 60 groups of wind turbines associated with the highest rates of avian fatality detections at Sand Hill. These groups averaged 4.5 times more native bird fatalities/MW/year than did the other turbines in the study

Table 1. Species placed in integrated detection trials at Sand Hill and Santa Clara wind energy projects, Altamont Pass Wind Resource Area, California, USA, July 2012 through March 2015. Body masses without footnotes were measured in the field.

Common name	Species name	Mass (g)
Black-crowned night-heron	*Nycticorax nycticorax*	600[ab]
Great egret	*Ardea alba*	1,000[ab]
Snowy egret	*Egretta thula*	205[b], 324[b], 370[ab]
Green-winged teal	*Anas crecca*	252[b]
Mallard	*Anas platyrhynchos*	756, 1,150[ab]
Northern shoveler	*Spatula clypeata*	593
American coot	*Fulicra Americana*	559[b]
Turkey vulture	*Cathartes aura*	2,000[ab]
White-tailed kite	*Elanus leucurus*	340[a]
Cooper's hawk	*Accipiter cooperii*	172[b], 276[b], 381[b], 400[ab], 400[ab], 400[ab], 400[ab], 400[ab], 400[a]
Sharp-shinned hawk	*Accipter striatus*	120[b], 140[ab], 162[b]
Red-shouldered hawk	*Buteo lineatus*	290[b], 614, 630[ab], 723[b]
Red-tailed hawk	*Buteo jamaicensis*	799[b], 894[b], 924[b], 948[b], 1,080[ab], 1,080[ab], 1,080[ab], 1,171[b], 1,215[b], 1,397[b], 1,561[b]
American kestrel	*Falco sparverius*	43[b], 82[b], 100[b], 122[ab], 122[ab]
Peregrine falcon	*Falco peregrinus*	381b
Barn owl	*Tyto alba*	200[b], 460[ab], 460[ab], 460[ab], 460[ab], 460[ab], 460[ab], 460[a]
Great-horned owl	*Bubo virginianus*	922[b], 1,705[ab], 1,705[ab]
Northern saw-whet owl	*Aegolius acadicus*	67[b]
Western screech owl	*Megascops kennicottii*	117[b], 202[ab]
Coturnix quail	*Coturnix coturnix*	160, 169, 179, 205[b], 216
California quail	*Callipepla californica*	90[a]
Wild turkey	*Melleagris gallopavo*	47, 6,650[ab], 6,650[ab]
Mourning dove	*Zenaida macroura*	62, 72, 99, 102, 103.5[b], 133[ab], 133[ab], 133[a], 133[a]
Rock pigeon	*Columba livia*	177[b], 255, 311.8[b], 322[ab], 380[b]
Eurasian collared-dove	*Streptopelia decaocto*	160[ab]
White-throated swift	*Aeronautes saxatalis*	20, 25, 26[b], 29[b]
Allen's hummingbird	*Selasphorus sasin*	2, 2, 3
Anna's hummingbird	*Calypte anna*	3, 4.2[b], 4.5[a], 4.5[a]
Downy woodpecker	*Picoides pubescens*	18[b]
Northern flicker	*Colaptes auratus*	93, 135[ab], 155[b]
Nuttall's woodpecker	*Picoides nuttallii*	18
Black phoebe	*Sayornis nigricans*	24[b]
Pacific-slope flycatcher	*Empidonax difficilis*	4, 4.2, 10[a], 10[a]
Barn swallow	*Hirundo rustica*	11
Cliff swallow	*Petrochelidon pyrrhonota*	12, 13[b], 14[b], 17[b]
Tree swallow	*Tachycineta bicolor*	6, 10[b], 11[b], 20.5[ab]
Violet-green swallow	*Tachycineta thalassina*	14[a]
American crow	*Corvus brachyrhynchos*	193[b], 214, 240[b], 247[b], 248[b], 249[b], 262, 264[b], 277[b], 303[b], 450[ab], 450[ab], 450[ab], 450[ab], 450[a]
Common raven	*Corvus corax*	580[b], 1,157[b], 1,157[b]
California scrub-jay	*Aphelocoma californica*	43, 45, 48[b], 63[b], 64, 78, 85[ab], 85[ab], 88
Yellow-billed magpie	*Pica nuttalli*	110[b], 160[ab]
Chestnut-backed chickadee	*Poecile rufescens*	7, 7, 7, 7, 9, 9
Oak titmouse	*Baeolophus inornatus*	12.6
Bushtit	*Psaltriparus minimus*	2, 3, 3, 4, 4.5, 5[a]
Red-breasted nuthatch	*Sitta canadensis*	10.5[a]
Bewick's wren	*Thryomanes bewickii*	7, 8, 10
House wren	*Troglodytes aedon*	11[a]
American robin	*Turdus migratorius*	40, 56[b], 63[b], 65[b], 73.7[b], 75[b], 81[ab], 81[ab]
Hermit thrush	*Catharus guttatus*	20, 21[b], 21[b], 21[b], 30[ab], 30[a]
Swainson's thrush	*Catharus ustulatus*	22[b]
Varied thrush	*Ixoreus naevius*	61[b]
Western bluebird	*Sialia mexicana*	21[b], 23, 23, 27[b], 27[ab]
Northern mockingbird	*Mimus polyglottos*	51[ab]
Cedar waxwing	*Bombycilla cedrorum*	26[b]
European starling	*Sturnus vulgaris*	40, 42[b], 42, 43, 47, 50, 51, 55.7, 71[b], 72, 75, 76, 76[b], 77[b], 78[ab], 78[ab], 78[ab], 78[ab], 78[a], 78[a], 78[a], 78[a], 78, 79, 80[b], 82[b], 85
Orange-crowned warbler	*Oreothlypis celata*	9[ab]
Wilson's warbler	*Cardellina pusilla*	3[b]
Townsend's warbler	*Setophaga townsendi*	9[a]
Yellow-rumped warbler	*Setophaga coronata*	9, 9
Black-headed grosbeak	*Pheucticus melanocephalus*	35[b], 42[b]
California towhee	*Melozone crissalis*	15[ab], 32[b], 32[b], 34[b]
Dark-eyed junco	*Junco hyemalis*	12.6[b], 14.3, 15
Fox sparrow	*Passerella iliaca*	32[ab]
Golden-crowned sparrow	*Zonotrichia atricapilla*	27.9[b], 28, 28
House sparrow	*Passer domesticus*	17, 17, 18, 19[b], 20, 20, 21, 21[b], 21[b], 22, 23, 23, 24, 25, 28[ab], 28[ab], 28[a], 28[a], 28[a]

(*Continued*)

Table 1. (*Continued*)

Common name	Species name	Mass (g)
Spotted towhee	*Pipilo maculatus*	41[b], 41[ab]
White-crowned sparrow	*Zonotrichia leucophrys*	23, 27
Brewer's blackbird	*Euphagus cyanocephalus*	40[b], 53, 66[b]
Brown-headed cowbird	*Molothrus ater*	19
Red-winged blackbird	*Agelaius phoeniceus*	43[b], 51
Western meadowlark	*Sturnella neglecta*	25[b]
American goldfinch	*Spinus tristis*	15.5[ab]
Lesser goldfinch	*Spinus psaltria*	4, 4, 6, 7.6, 15[ab], 15[a]
Pine siskin	*Spinus pinus*	10.7, 15[ab]
House finch	*Haemorhous mexicanus*	15[b], 16[b], 17, 19, 21, 24[b], 25[ab], 25[a]

[a] Body mass typical of species, rather than measured upon trial placement.
[b] Carcass detected by searchers.

area. We randomly selected groups of wind turbines from the 60 groups in this high-fatality sampling pool, and we rank-ordered our selections until our study turbines numbered 158 (8.345 MW). To these we added 36 wind turbines at the nearby Santa Clara project.

Fatality Monitoring

We monitored for fatalities at Sand Hill from 3 April 2012 through 31 March 2015. We walked parallel transects separated by 6–7 m and out to 50 m from wind turbine pads, which overlapped unselected adjacent turbines and raised the monitored capacity to 10.979 MW. We searched each wind turbine every 5 ± 1.5 days (SD) on average. We recorded dates of fatality searches, and initial and subsequent carcass detections. We mapped fatality locations using a Trimble GeoXT global positioning system (GPS; Trimble, Sunnyvale, CA, USA), photographed each carcass, and left all found fatalities in place indefinitely. We recorded fatalities found beyond the search radius during routine searches, but we recorded as incidental finds those fatalities outside search areas at times other than routine searches. We included 12 incidental finds in fatality estimation that we found at monitored wind turbines, but we excluded incidental finds found at non-monitored wind turbines.

Each time searchers found a carcass, they estimated low and high numbers of days since death. We used these estimates to test the accuracy and precision of time-since-death estimates that were applied to detection trial carcasses because time since death is often used in fatality monitoring programs to decide whether a given fatality find should be included in a season, year, mitigation treatment period, or the fatality monitoring program. Searchers also identified carcasses to species when possible, and when not possible, they identified the carcasses to the nearest possible taxonomic group or to small, medium, or large size class as described below. We quantified the accuracy of species identifications among detection trial carcasses. Because we left all carcasses in the field as found, we also monitored our ability to identify carcasses to species as remains aged, treating each successive detection as if anew. For example, had a carcass been identified as a red-shouldered hawk (*Buteo lineatus*) because the entire carcass was initially encountered only 1 day since death, but 2 months later only matted contour feathers remained, then the searchers were expected to identify the carcass based on what they saw on the ground at the moment and not 2 months earlier. Given matted contour feathers and nothing else to identify the remains, searchers would record the fatality as a large raptor as if detecting it for the first time. Often, however, the searcher would also record species identification based on memory in the notes section of the GPS data recorder.

We determined fatalities from remains including $\geq$2 flight feathers connected by tissue or $\geq$5 scattered flight feathers, $\geq$10 contour feathers, bones, body parts, or whole carcasses. Searchers recorded cause of death when confident in their determination. They took $\geq$4 photos of carcass remains, recorded visible injuries and carcass condition, and noted any other possible causes of death such as electric distribution lines, transmission lines, electric distribution poles, fences, and mammalian carnivore dens. We used all this information and distance from wind turbine or other hazardous structure to determine whether the fatality was unlikely, possibly, probably, or certainly caused by a wind turbine. We excluded fatalities determined unlikely caused by a wind turbine from wind turbine-caused fatality estimation.

Detection Trials

We placed 276 carcasses representing 79 bird species ranging in size from hummingbirds to wild turkeys (Table 1) obtained from licensed wildlife rehabilitation facilities or as serendipitous carcass finds. Carcasses had not been treated with pharmaceuticals, posing no chemical or physical risk of injury to scavengers. Possession and placement of avian carcasses were permitted by United States Geological Survey Bird Banding Laboratory Master Federal Bird Banding Permit 23599 (DAB), Federal Fish and Wildlife Permit – Scientific Collecting MB135520-0 (DAB) and California Department of Fish and Wildlife Scientific Collecting Permits SC-7313 (DAB) and SC-5242 (KSS). These carcasses were frozen immediately after death. To simulate an assumed steady rate of fatalities caused by the wind turbines, we placed 2 carcasses weekly on randomized days Monday through Friday and at random locations within the search areas around wind turbines. This placement rate averaged only 2.3 g/ha/year of bird mass, which probably avoided scavenger swamping. We thawed carcasses just before placement and weighed them. We assumed that carcass freezing did not affect carcass persistence, but there is no evidence available on whether our assumption was accurate. We marked carcasses by clipping wing and tail feathers and wrapping a small strip of black electrical tape or plastic zip-ties around each leg. We cut excess plastic from the zip-ties to minimize risk of injury to scavengers, and we wrapped only small strips of electrical tape around legs. The searchers were blind to these placements unless and until they found the carcasses.

Even though we predetermined placement locations, we dropped trial carcasses from shoulder height and mapped the locations using a GPS. At each carcass placement, we recorded date; time; turbine address; distance and bearing to turbine; species; body mass (g); age class; sex; whether slight, modest, or no signs of desiccation; carcass source; transporter; who placed the carcass; whether partial, high, or no occlusion due to vegetation, rocks, burrows, or other features; whether the aspect facing upwards from the placement site was ventral, dorsal, or lateral; distances (m) in 3 directions (toward or away from turbine, and tangential to turbine in opposite directions) from placement site before the carcass was no longer visible or recognizable as a carcass; whether placed in grassland, reclaimed turbine pad, gravel pad, gravel access road, cut bank, or other substrate; and any relevant notes.

Searchers left all trial carcasses and all fatality finds undisturbed where found, so all trials were indefinite until the study ended. Searchers recorded locations and attributes of all found carcasses, and they delivered the data to the lead investigator (Smallwood) weekly. Searchers described carcass remains and noted those marked as trial carcasses, and this information helped the trial administrator track all carcasses throughout the study.

Smallwood visited placed trial carcasses at least once to check on status. Status information included date; time; trial carcass identification number; wind turbine address; distance and bearing to turbine; species; whether flight feathers were edged or frayed; whether body feathers were fluffy or matted; whether feathers were original, faded, or bleached in color; whether the remains were being visited by maggots, beetles, ants, flies, or grasshoppers; whether the remains were intact (if not, specific body parts were identified); and any notes that were relevant or that would help locate the carcass for an additional status check.

Fatality Rate Adjustments

Prior to this study, the standard fatality rate estimator used in the APWRA was a variation of the Horvitz and Thompson (1952) estimator based on carcass persistence curves (Fig. 1A). We used this estimator again, and the estimator based on mean days to carcass removal (Fig. 1B), to compare the resulting fatality estimates to those estimated from overall detection rates (Fig. 1C). We tabulated searcher detection outcomes, and hence searcher detection rates (S), from those trial carcasses confirmed to have been available for detection upon the first search following placements, where the trial administrator confirmed availability via carcass checks. We used administrator carcass checks and repeat searcher visits to also calculate carcass persistence rates and mean days to removal. Because carcasses were left *in situ* for the duration of the study, we could perform unusually long persistence trials while also curtailing trials to compare results to conventional trials. For the proportion of carcasses found within the maximum search radius of 50 m among wind turbines on tower heights ranging 18.5 m to 24.6 m, we used the national average $d = 0.92$, which was largely influenced by fatality monitoring in the APWRA (Smallwood 2013).

To estimate overall detection rates (our new approach), we used logit regression to fit detection outcomes (not found $= 0$, found $= 1$) of 276 trial carcasses as a function of $\log_{10}$-transformed body mass (M), from which the resulting odds ratio represented the proportion of carcasses found:

$$D = \frac{e^{-a+b\times\log_{10}M}}{1+e^{-a+b\times\log_{10}M}}$$

where a and b were best-fit coefficients. Logit regression estimates standard error for each odds ratio, which can be multiplied by standard normal z to obtain a desired confidence interval for application to fatality finds:

$$CI[D] = e^{\ln(D)-z\times SE}, e^{\ln(D)+z\times SE}$$

Whereas the function for D could be applied directly to M representing species found as fatalities, the same was not true for lower confidence limits (LCLs) and upper confidence limits (UCLs) because LCL and UCL were estimated specifically for trial carcasses. However, our placement of 276 trial carcasses meant that typical body masses of found fatalities were often equal or nearly equal to the mass of trial carcasses. Except for golden eagle, the average difference between the typical body mass of a found fatality and the closest measured body mass of a placed carcass was 0.74% of the mass of the fatality find. Therefore, for each fatality find, we used the logit regression LCL and UCL estimates from the trial carcasses nearest in size to the fatality find. As for the golden eagle, it was likely about 2,000 g smaller than our largest trial placements and 2,000 g larger than the next smallest placement, but 95% LCLs estimated for D at these smaller and larger body sizes were both large at 0.88 and 0.93, respectively, and UCLs were 0.97 and 0.99. Because the fatality adjustments are so small at these body sizes, we represented golden eagle with averaged values between the next largest and smallest trial placements, resulting in an LCL of 0.905 and an UCL of 0.98.

To estimate confidence intervals associated with fatality rate estimates based on our new approach, we used the delta method to pool the standard errors associated with D and with inter-turbine variability in detected fatalities/MW/year (f):

$$\mathrm{LCL}, \mathrm{UCL} = \sqrt{\left(\frac{1}{D}\times \mathrm{SE}[f]\right)^2 + \left(f \times \left(\frac{-1}{D}\right) \times \mathrm{SE}[D]\right)^2}$$

in which SE[D] was asymmetrical, so needed separate entries for LCL and UCL.

Our new integrated detection trial approach opened an opportunity to examine fatality rate estimates at wind projects in a new manner (Fig. 1). For the first time, detection trial data could imitate fatality data by estimating faux fatality rates along with actual fatality rates, where the faux fatality rates were derived from the detection trial data. Pretending that the placed carcasses were actual fatalities and relying on a detection probability model based on body mass, we applied the same adjustment factor to placement finds (p) that we used on fatality finds (f) to estimate total placements ($\hat{P}$; Fig. 1C). We validated $\hat{P}$ by comparing it to the true number of placements. We additionally validated the new approach by using half the trial placements to fit a logit regression model of trial outcomes on $\log_{10}(M)$ and comparing $\hat{P}$ to the true number of the other placements excluded from model development. We alternately assigned trials to the model development group or validation group from a list of trials sorted by body mass. Although we assumed that the trial placements contributing to the model would be tested independently by the validation group, we also suspected the reduced sample size could fundamentally change the model. Nevertheless, this approach could further inform about potential validation shrinkage and the effects of sample sizes.

Analysis

We quantified our fatality finds and predicted overall detection rates of trial carcass detection outcomes logit-regressed on body mass, and then we estimated fatality rates adjusted by our new overall detection rates (D). We next used our integrated detection trial data to estimate conventional detection terms to explore potential biases. We used our data to quantify searcher detection rate (S) derived from placements 1 day and >1 day prior to fatality searches, and to compare mean daily carcass persistence (R_C) and the probability the carcass will persist to the next day (r) derived from various trial periods (I). We divided fatality estimates derived from conventional approaches by those derived from our new approach and related this ratio to body mass as a means to reveal the magnitudes of over- or under-estimation resulting from the specific approach used, trial duration, and body mass.

Using our trial placements as training data, we took a first step toward validating our new approach by regressing adjusted placement rates on known placement rates. We further attempted validation by using half the placement data to derive another model of D logit-regressed on $\log_{10}$ body mass, which was then used to adjust placement rate estimates from the other half of the placement data.

Finally, we used the residuals from trial outcomes logit-regressed on $\log_{10}$ body mass to reveal potential biases that are typically neglected in reports of fatality monitoring. We plotted mean residuals against levels of ground visibility, distance from the wind turbine, and month of the year when trial carcasses were placed. To assess whether clearing searches are warranted, and how often subsequent searches detect carcasses missed upon the first search, we also compared the number of searches needed to detect placed carcasses of various ranges of body mass. We quantified the percentage of species detected, the percentage not detected, and the percentage of species misidentified by the searchers' examination of carcass remains. We also examined the role of time since trial placement in the ability of searchers to categorize carcasses to species, larger taxa, or body size, and we tested searchers' ability to estimate time since death.

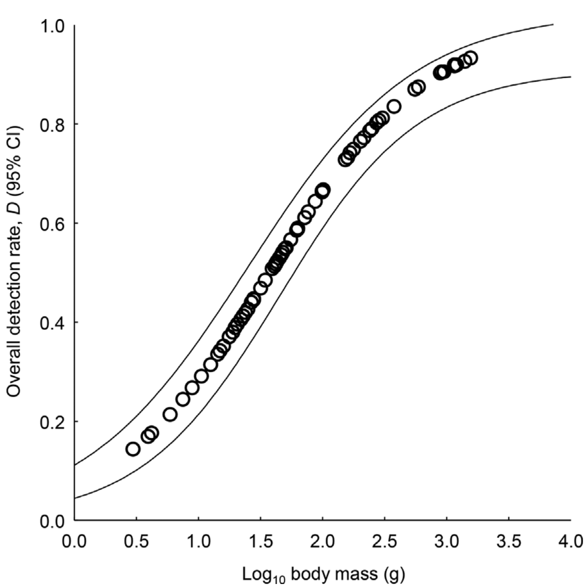

Figure 2. Overall detection probability and 95% confidence intervals predicted from logit regression of detection outcomes on $\log_{10}$ body mass (g) of 276 bird carcasses that were placed in integrated detection trials July 2012 through March 2015, Sand Hill project, Altamont Pass Wind Resource Area, California, USA.

RESULTS

During 34,863 turbine searches for fatalities spanning 3 years, we found carcass remains of 954 birds and 2 bats. Of these fatalities, 869 were determined to have been probably or certainly caused by wind turbines. Fifty-four fatalities were possibly caused by wind turbines but also showed evidence suggesting predation, entrapment, or collisions with distribution or transmission lines. Thirty-three were likely caused by predation, nest failure, entrapment, electrocution on distribution poles, and collision with electric distribution lines or transmission lines. Half ($n = 476$) of the found fatalities were rock pigeons (*Columba livia*), 100 were European starlings (*Sturnus vulgaris*), but we also found 1 golden eagle, 17 red-tailed hawks (*Buteo jamaicensis*), 1 ferruginous hawk (*Buteo regalis*), 18 American kestrels (*Falco sparverius*), 40 burrowing owls (*Athene cunicularia*), 8 barn owls (*Tyto alba*), 4 great-horned owls (*Bubo virginianus*), 2 turkey vultures (*Cathartes aura*), and another 287 birds of various species.

Based on 276 trial carcasses placed within fatality search areas over 2.5 years of the 3-year monitoring effort, we predicted overall detection rate from logistic (logit) regression of detection outcomes on $\log_{10}$ body mass (Fig. 2):

$$D = \frac{e^{-2.5638+1.6198\times\log_{10}M}}{1+e^{-2.5638+1.6198\times\log_{10}M}}$$

We fit the following nonlinear regression models to the 95% confidence limits around D: 95% $\mathrm{LCL}[D] =$

Table 2. Comparison of estimated fatalities/MW/year at 138 40-KW and 65-KW wind turbines selected for known high fatality rates and adjacent turbines within the maximum search radius (10.979 MW) searched 3 years at Sand Hill (left columns) and 36 95-KW Vestas turbines (3.42 MW) searched 1 year at Santa Clara (right columns), Altamont Pass Wind Resource Area, California, USA. Wind turbines at Sand Hill were monitored 3 April 2012 through 31 March 2015, and those at Santa Clara were monitored 21 April 2014 through 31 March 2015.

		Estimated fatalities/MW/year			
		Sand Hill		Santa Clara	
Common name	Species name	$\bar{x}$	CI	$\bar{x}$	CI
Mexican free-tailed bat	*Tadarida brasiliensis*	0.096	0.007–0.186	1.132	0.074–2.186
Grebe		0.067	0.023–0.110	0.000	
American coot	*Fulicra Americana*	0.110	0.038–0.181	0.000	
Killdeer	*Charadrius vociferus*	0.162	0.074–0.251	0.000	
Spotted sandpiper	*Actitis macularis*	0.057	0.004–0.109	0.000	
California gull	*Larus californicus*	0.033	0.003–0.064	0.000	
Glaucous-winged gull	*Larus glaucescens*	0.033	0.003–0.062	0.000	
Herring gull	*Larus argentatus*	0.033	0.003–0.063	0.000	
Thayer's gull	*Larus glaucoides*	0.073	0.024–0.122	0.000	
Gull		0.283	0.192–0.375	0.000	
Turkey vulture	*Cathartes aura*	0.031	0.003–0.060	0.000	
Golden eagle	*Aquila chrysaetos*	0.050	0.004–0.096	0.000	
Ferruginous hawk	*Buteo regalis*	0.052	0.004–0.099	0.000	
Red-tailed hawk	*Buteo jamaicensis*	0.335	0.214–0.457	0.348	0.028–0.669
Large raptor		0.065	0.023–0.107	0.000	
American kestrel	*Falco sparverius*	0.623	0.450–0.796	0.459	0.036–0.881
Barn owl	*Tyto alba*	0.223	0.138–0.307	0.000	
Burrowing owl	*Athene cunicularia*	2.264	1.830–2.701	0.000	
Great-horned owl	*Bubo virginianus*	0.095	0.045–0.145	0.000	
Mourning dove	*Zenaida macroura*	2.198	1.879–2.521	0.000	
Rock pigeon	*Columba livia*	16.245	14.723–17.840	0.389	0.031–0.747
Dove		0.872	0.698–1.048	0.450	0.036–0.635
Common poorwill	*Phalanoptilus nuttallii*	0.084	0.006–0.161	0.000	
White-throated swift	*Aeronautes saxatalis*	0.063	0.005–0.122	0.000	
Northern flicker	*Colaptes auratus*	0.084	0.029–0.138	0.000	
Ash-throated flycatcher	*Myiarchus cinerascens*	0.065	0.006–0.125	0.000	
Black phoebe	*Sayornis nigricans*	0.128	0.011–0.246	0.000	
Pacific-slope flycatcher	*Empidonax difficilis*	0.383	0.193–0.572	0.000	
Say's phoebe	*Sayornis sayo*	0.120	0.008–0.233	0.000	
Horned lark	*Eremophila alpestris*	0.119	0.043–0.195	0.000	
American crow	*Corvus brachyrhynchos*	0.031	0.003–0.060	0.000	
Common raven	*Corvus corax*	0.398	0.294–0.502	0.000	
Corvid		0.041	0.003–0.078	0.000	
American robin	*Turdus migratorius*	0.047	0.004–0.091	0.505	0.040–0.971
Loggerhead shrike	*Lanius ludovicianus*	0.279	0.172–0.386	0.000	
European starling	*Sturnus vulgaris*	5.775	5.194–6.362	0.510	0.040–0.981
Yellow-rumped warbler	*Setophaga coronata*	0.284	0.096–0.470	0.000	
Lazuli bunting	*Passerina amoena*	0.171	0.092–0.250	0.000	
Lincoln's sparrow	*Mellospiza lincolnii*	0.072	0.005–0.138	0.000	
Rufous-crowned sparrow	*Aimophila ruficeps*	0.070	0.005–0.134	0.000	
Song sparrow	*Melospiza melodia*	0.063	0.005–0.121	0.000	
Sparrow		0.123	0.009–0.236	0.000	
Red-winged blackbird	*Agelaius phoeniceus*	0.085	0.007–0.164	0.000	
Tricolored blackbird	*Aegolaius tricolor*	0.053	0.004–0.101	0.000	
Western meadowlark	*Sturnella neglecta*	1.564	1.285–1.844	0.000	
Blackbird		0.294	0.171–0.418	0.000	
Bullock's oriole	*Icterus bullockii*	0.061	0.005–0.118	0.000	
House finch	*Haemorhous mexicanus*	0.341	0.179–0.502	0.000	
Finch		0.111	0.008–0.214	0.000	
Lesser goldfinch	*Spinus psaltria*	0.087	0.006–0.167	0.000	
Goldfinch		0.090	0.006–0.173	0.000	
Large bird		0.254	0.156–0.351	1.050	0.508–1.593
Medium bird		0.678	0.498–0.859	0.389	0.031–0.747
Small bird		5.468	4.783–6.148	1.426	0.505–2.348
All bats		0.096	0.007–0.186	1.132	0.074–2.186
All raptors		3.738	2.711–4.769	0.807	0.064–1.550
All birds		41.385	33.662–49.194	5.527	1.255–9.801

$\frac{1}{1.100+21.434\times 0.1667^{\log_{10}M}}$ ($r^2 = 0.999$, root mean square error [RMSE] = 0.019) and 95% UCL[D] = $\frac{1}{0.975+11.785\times 0.2148^{\log_{10}M}}$ ($r^2 = 0.996$, RMSE = 0.046). We then predicted D from the representative body mass of each species found as fatalities to estimate $\hat{F}$ (Table 2).

Estimated avian fatality rates were relatively high among wind turbines monitored in this study, and as expected because of the selection of wind turbines, raptor fatality rates were higher at Sand Hill than at Santa Clara (Table 2). Across the 14.399 MW of monitored turbines between the 2 projects, annual fatality estimates based on our new approach were 5 bats (CI = 1.6–6.5), 53 raptors (CI = 41–61), and 563 birds (CI = 396–591). Monitored wind turbines caused the annual deaths of an estimated 8.4 American kestrels, 25 burrowing owls, 2.4 barn owls, 1 great-horned owl, 0.5 golden eagles, 0.6 ferruginous hawks, 5 red-tailed hawks, 24 mourning doves, 180 rock pigeons (these pigeons were often found with leg bands), 65 European starlings, 4.4 common ravens, 3 loggerhead shrikes (*Lanius ludovicianus*), 5 gulls, 17 western meadowlarks (*Sturnella neglecta*), and various numbers of other species.

Conventional Detection Adjustments

Searcher detection rates (S) from trial carcasses that were confirmed available to be found by searchers were 0.42 for 120 small birds (<280 g) and 0.73 for 53 large birds (≥280 g). However, among the small trial carcasses confirmed available to searchers, searcher detection averaged 0.59 for the 32 carcasses placed within 1 day of the next search and 0.39 for 88 carcasses placed 2 to 6 days before the next search. Among the large trial carcasses confirmed available to searchers, searcher detection averaged 0.65 for 23 carcasses placed within 3 days of the next search and 0.80 for 30 carcasses placed 4 to 6 days before the next search.

We fit nonlinear models to mean daily carcass persistence rates (R_C) from trials curtailed at 5, 15, 30, 45, 60, 90, 120, 150, and 670 days (Fig. 3). For small bird trials curtailed at 5 days ($I = 5$) and for large bird trials curtailed at 5 and 15 days, mean daily carcass persistence was fit by the function: $R_C = \frac{1}{a\times b^I}$, where a and b were best-fit coefficients. For all other combinations of carcass size class and trial duration, mean daily carcass persistence was fit by the function: $R_C = a - b \times \log_2(I)$. Values of r^2 ranged 0.95 to 0.99, with $r^2 \geq 0.98$ for 13 of 18 models. Root-mean-square

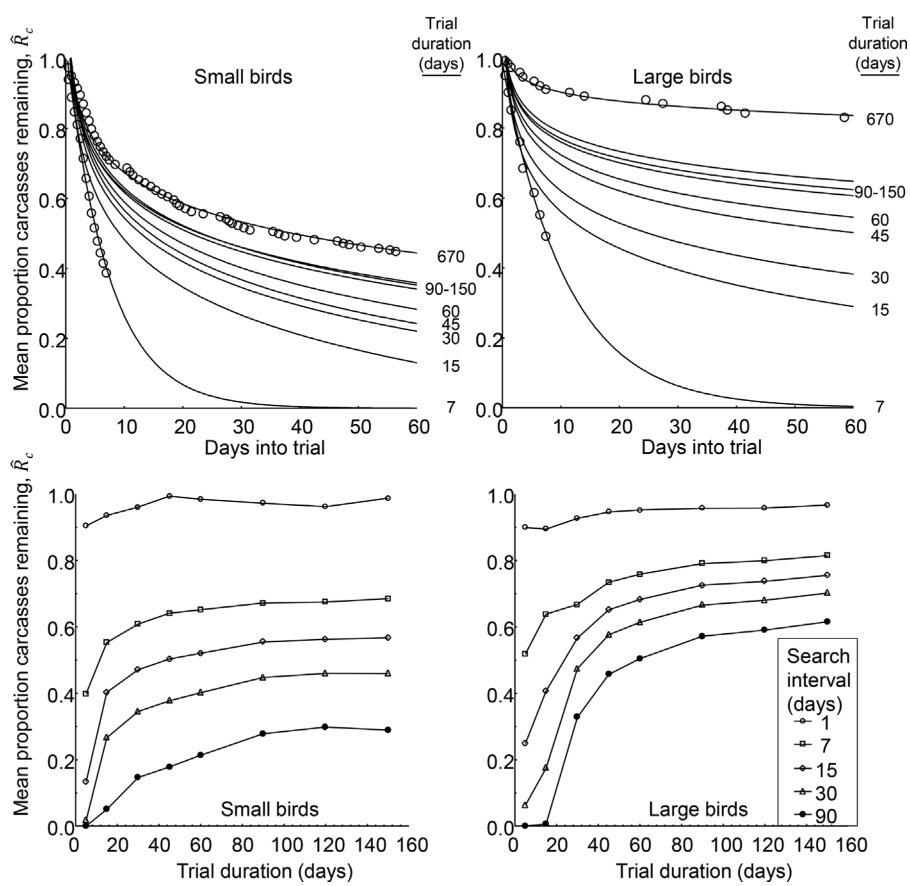

Figure 3. Nonlinear model predictions of mean daily carcass persistence, where models were derived from detection trial durations of 7, 15, 30, 45, 60, 90, 120, 150, and 670 days (top graphs), and model predictions at typical fatality search intervals by trial duration (bottom graphs) at Sand Hill, Altamont Pass Wind Resource Area, California, USA, July 2012 through March 2015. Because of crowding, data in the top graphs are shown only for models derived from 7 days and 670 days.

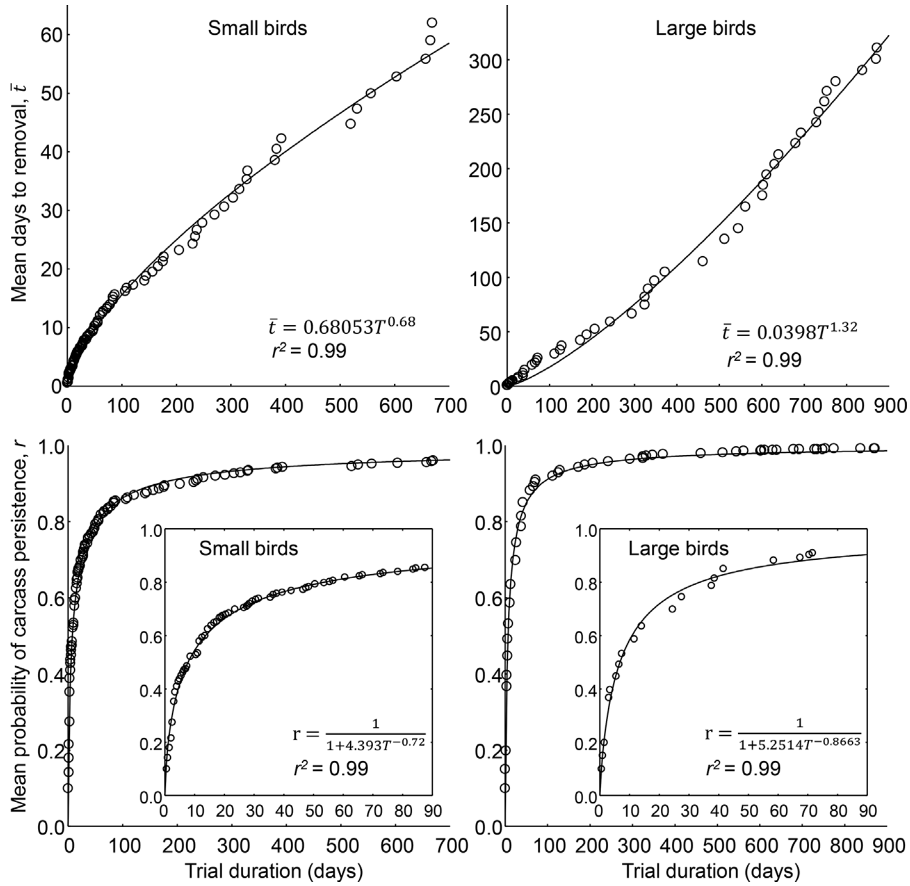

Figure 4. Increase in mean days to carcass removal (top graphs) and the resulting average probability of a carcass persisting (lower graphs) with increasing duration of detection trials performed at Sand Hill, Altamont Pass Wind Resource Area, California, USA, July 2012 through March 2015. In the lower graphs, the relationship between mean probability of a carcass persisting and trial duration are shown at 2 scales, the inset graphs showing the relationship over spans of time more typical of detection trials performed at wind projects.

error ranged 0.001 to 0.046 among the models. Curtailed from our carcass persistence data, a trial lasting 15 days instead of 60 days would lead to an 18% upward adjustment of fatality rates for small and large birds found in monitoring with a 7-day search interval, and it would lead to upward adjustments of 51% for small birds and 2.51 times for large birds found in monitoring with a 30-day search interval (Fig. 3). Trials of very short duration, such as 5 days or

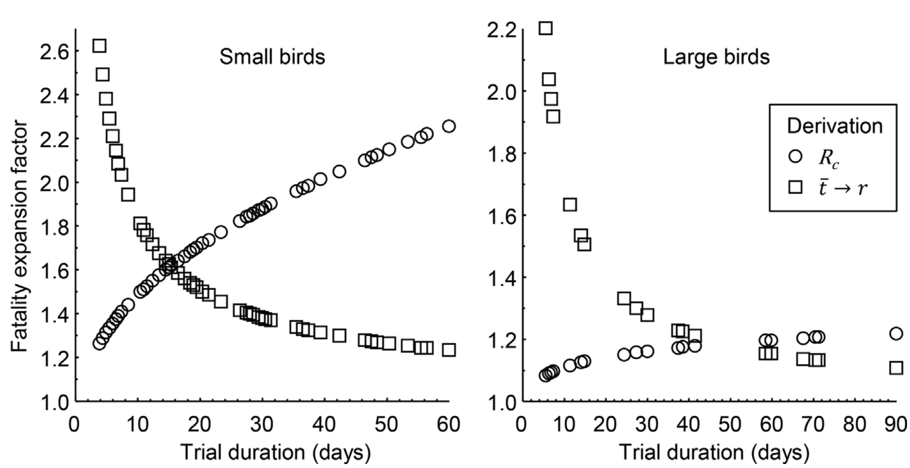

Figure 5. Change in fatality rate expansion factors with increasing trial duration (constrained to 4–60 days), where expansion factors were derived from mean days to carcass removal ($\bar{t}$) and probability that a trial carcass would remain to be found (r) versus mean daily proportion of carcasses remaining (R_C) between the conventional detection trial approaches at Sand Hill, Altamont Pass Wind Resource Area, California, USA, July 2012 through March 2015.

15 days, would lead to very large upward adjustments of fatality rates if monitoring were based on a 90-day search interval (Fig. 3). For monitoring consisting of daily searches, however, upward adjustments of fatality rates remained ≤11% for carcass persistence regardless of trial duration for both small and large birds (Fig. 3).

Our data indicated that estimators based on mean days to carcass removal are also vulnerable to bias when carcass persistence is assumed exponential. Mean days to removal increased as power functions of increasing trial duration (Fig. 4). Compared to a 1-day trial duration, mean days to removal was 21 times longer in 90-day trials for small birds and 383 times longer for large birds. Compared to a 7-day trial, a 60-day trial yielded mean days to removals 4.3 and 17.2 times longer for small and large birds, respectively. The average probability of a carcass persisting (r) was 4.6 times and 5.6 times greater for small and large birds, respectively, when the trial lasted 90 days instead of 1 day, though it was 2 times and 1.9 times greater in 1-day trials after correcting for the effective interval (Huso 2010). It was 1.7 times greater for both small and large birds when the trial lasted 60 days instead of 7 days (the 7-day interval was less than the effective interval, so no adjustment would be justified).

Fatality rate expansion factors associated with the conventional approaches changed with trial duration in opposing directions between carcass persistence curves and mean days to removal, intersecting at 16 days for small birds and 48 days for large birds (Fig. 5). For small birds, expansion factors were about equally responsive to trial duration out to the full data range (not shown in Fig. 5). For large birds, expansion factors derived from mean days to removal were more responsive to trial duration, especially among trials briefer than 40 days (Fig. 5). Mean days to removal led to fatality rate expansion factors <1.2 among trials lasting >73 days and >60 days for small and large birds, respectively. For both small and large birds, expansion factors rapidly increased with briefer trials until they were more than double based on trials <7 days (Fig. 5).

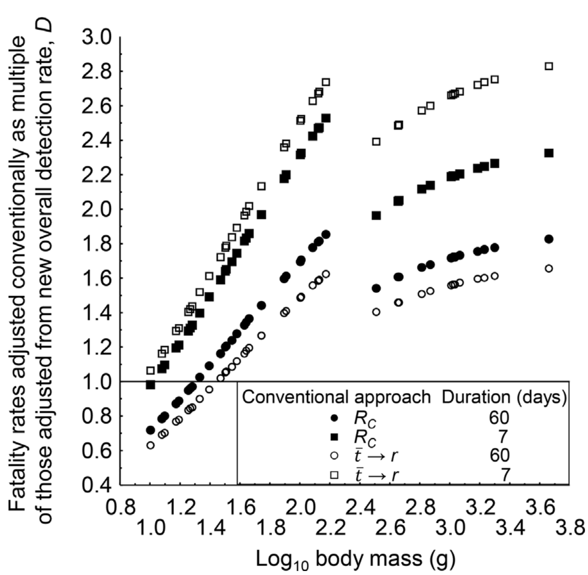

Figure 6. Conventionally adjusted fatality rates as multiples of fatality rates adjusted by overall detection rates (D) in integrated trials and related to $\log_{10}$ body mass (M), where conventional adjustments included separate estimates of searcher detection error and carcass persistence estimated from persistence curves (R_C) and mean days to carcass removal ($\bar{t}$) leading to estimates of the probability that a trial carcass would remain to be found (r) in trials lasting 7 days and 60 days at Sand Hill, Altamont Pass Wind Resource Area, California, USA, July 2012 through March 2015. The pattern breaks at $\log_{10} = 2.45$ resulted from lumping of adjustments with 2 body size classes of small and large in conventional approaches to estimating fatalities.

With increasing body mass, fatality rates adjusted from conventional detection trial results (mean daily carcass persistence rate and mean days to carcass removal) increased as a multiple of fatality rates adjusted by our new overall detection rate, more so for trials lasting 7 days than those lasting 60 days (Fig. 6). The pattern broke at $\log_{10}(M) = 2.45$ because the adjustments were lumped within small and large body size classes in conventional approaches to estimating fatalities. The conventional approaches underestimated fatality rates at the smallest body sizes and overestimated fatality rates at the larger body sizes.

Performance of Detection Trial Approaches

The training data validated the accuracy of fatality rate estimates adjusted by our new overall detection rates (Fig. 7). Adjusted carcass placement rates correlated strongly with known carcass placement rates over the first 2 years of integrated detection trials (Fig. 7), although the regression slope was about 0.75 rather than the expected 1.00 for the third year of data when extreme drought eliminated grass cover and left placed trial carcasses exposed to a desperate scavenger community.

Validity shrinkage emerged when using half the placements ($n = 138$) to fit a logit regression model of trial outcomes on $\log_{10}(M)$ to predict D, and then applying D from this model to adjust faux fatality rates based on detected trial carcasses in the other half of the placements ($n = 138$):

$$D = \frac{e^{-2.9884+1.7523\times\log_{10}M}}{1+e^{-2.9884+1.7523\times\log_{10}M}}$$

Based on half the data and 28% fewer species represented, the new model's parameters a and b increased in magnitude by 17% and 8%, respectively. Model validity shrinkage was expressed by increasingly larger over-estimates of faux fatality rates with increasing placement rates among species. Based on half the trial placements applied to the other half of the placements, adjusted carcass placement rates regressed on known carcass placement rates with slopes >1, steeper than those based on all placements: ($\hat{P} = 0.108 + 1.725 \times P, r^2 = 0.34$, RMSE = 0.24), combined weight of placements totaling ≥100 g per species ($\hat{P} = 0.032 + 1.758 \times P, r^2 = 0.80$, RMSE = 0.11), combined weight of placements totaling ≥500 g ($\hat{P} = -0.055 + 1.631 \times P, r^2 = 0.95$, RMSE = 0.06), and combined weight of placements totaling ≥2,000 g ($\hat{P} = -0.017 + 1.274 \times P, r^2 = 1.00$, RMSE = 0.01). Model accuracy and precision improved as species used in the model were increasingly restricted to greater combined weight of placed trial carcasses.

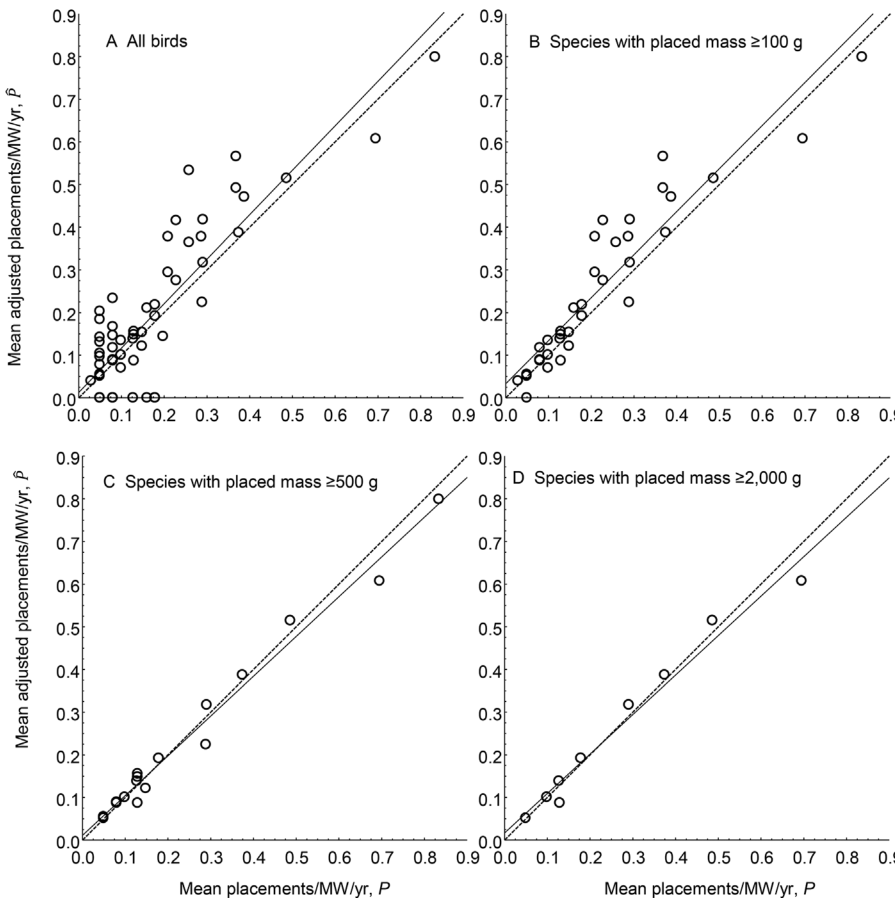

Figure 7. Adjusted placement rates ($\hat{P}$) per species of found trial carcasses regressed (solid line) on known placement rates (P) of trial carcasses prior to onset of severe drought, July 2012 through March 2014, Sand Hill, Altamont Pass Wind Resource Area, California, USA, including A) all placements ($\hat{P} = 0.013 + 1.042 \times P, r^2 = 0.75$, root mean square error [RMSE] = 0.092), B) combined weight of placements totaling ≥100 g per species ($\hat{P} = 0.033 + 1.010 \times P, r^2 = 0.87$,RMSE = 0.069), C) combined weight totaling ≥500 g ($\hat{P} = 0.011 + 0.932 \times P, r^2 = 0.98$,RMSE = 0.031), and D) combined weight totaling ≥2,000 g ($\hat{P} = 0.017 + 0.925 \times P, r^2 = 0.97$,RMSE = 0.036). Dashed lines represent equivalence between adjusted trial placements following fatality search outcomes and known trial placements.

Quantifying Errors from Integrated Detection Trials

The residual variation in D logit-regressed on M was slight, but it did suggest a few meaningful patterns related to other variables (Fig. 8). In relation to distance until the carcass was no longer visible, fewer trial birds tended to be detected when these distances were shortest, although detections also averaged fewer for carcasses that were visible for long distances. Detections tended to peak at 25–34 m from the turbine row, tended to be lower in June as compared to other months of the year, and tended to peak when placed within a day of the next fatality search, and generally declined with number of days between placements and the next fatality search (Fig. 8).

Searchers found 58% ($n = 148$) of placed birds, of which 57.4%, 15.5%, and 9.5% were found upon first, second, and third searches following placement, respectively, and the remaining 17.6% were found upon the fourth through the 121st searches. The average number of searches per first detection was 4.3, including 2.5 searches/first detection (median = 1.5) for birds weighing ≤10 g, 9.3 searches/first detection (median = 2) for birds weighing 10.1–20 g, 7.3 searches/first detection (median = 1) for birds weighing 20.1–40 g, 8.1 searches/first detection (median = 2) for birds weighing 40.1–80 g, and decreasing numbers of searches with increasing body mass including 1.5 searches/first detection (median = 1.5) for birds weighing >2,560 g.

Searchers found 57 (76%) of 75 species placed, meaning that 24% of species placed were never detected. Carcasses of 25 species (44%) were found but misidentified. Species misidentified were Anna's hummingbird (*Calypte anna*), orange-crowned warbler (*Oreothlypis celata*), Wilson's warbler (*Cardellina pusilla*), lesser goldfinch (*Spinus psaltria*), black phoebe (*Sayornis nigricans*), western bluebird (*Sialia mexicana*), dark-eyed junco (*Junco hyemalis*), black-headed grosbeak (*Pheucticus melanocephalus*), pine siskin (*Spinus pinus*), fox sparrow (*Passerella iliaca*), golden-crowned sparrow (*Zonotrichia atricapilla*), Downy woodpecker (*Picoides pubescens*), hermit thrush (*Catharus guttatus*), Swainson's thrush (*Catharus ustulatus*), spotted towhee (*Pipilo maculatus*), California towhee (*Melozone crissalis*), red-winged blackbird (*Agelaius phoeniceus*), western meadowlark, Brewer's blackbird (*Euphagus cyanocephalus*), northern mockingbird (*Mimus polyglottos*), Eurasian collared-dove (*Streptopelia decaocto*), northern saw-whet owl (*Aegolius*

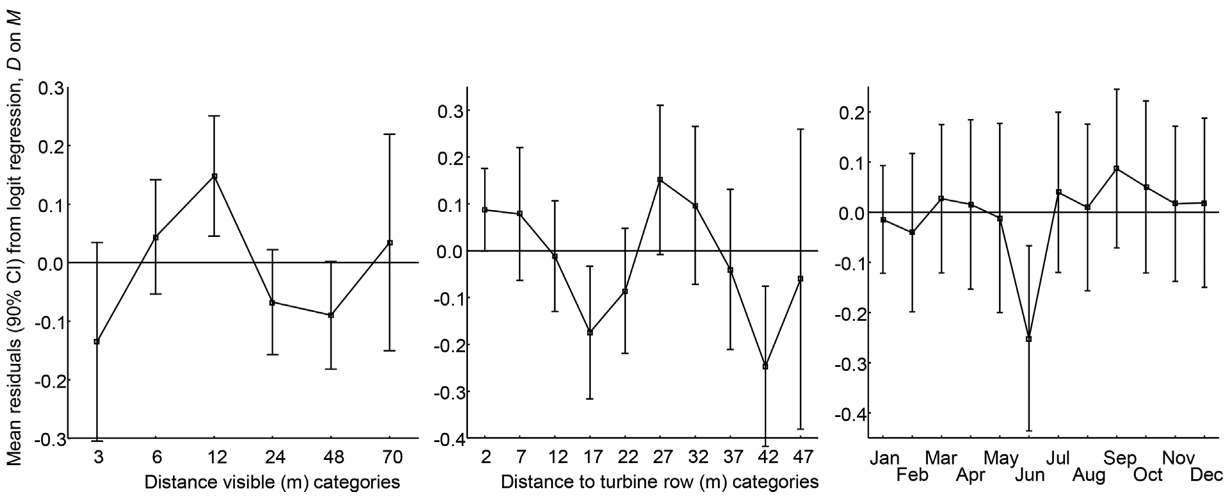

Figure 8. Patterns of mean (90% CI) residuals measured from logit regression of detection outcomes (D) on body mass (M) among placed bird carcasses July 2012 through March 2015, Sand Hill, Altamont Pass Wind Resource Area, California, USA. We compared residuals to the distance each carcass remained visible to the trial administrator upon placement (binned distances are 3 = 0–3 m, 6 = 3.1–6 m, 12 = 6.3–12 m, 24 = 12.6–24 m, 48 = 24.3–48 m, and 70 = 49–70 m; left graph), categories of distance between the placed carcass and the wind turbine row's central axis among distance categories (2 = 0–4 m, 7 = 5–9 m, 12 = 10–14 m, 17 = 15–19 m, 22 = 20–24 m, 27 = 25–29 m, 32 = 30–34 m, 37 = 35–39 m, 42 = 40–44 m, and 47 = 45–57 m; middle graph), and month of the year (right graph).

acadicus), green-winged teal (*Anas crecca*), American coot (*Fulicra americana*), and peregrine falcon (*Falco peregrinus*). Most of these misidentified species were very small, but most were also likely to occur in the area as residents or migrants. Found carcasses were also incorrectly identified to 6 species (8%) that were not placed, including violet-green swallow (*Tachycineta thalassina*), yellow-rumped warbler (*Setophaga coronata*), mountain bluebird (*Sialia currucoides*), Say's phoebe (*Sayornis sayo*), common poorwill (*Phalanoptilus nuttallii*), and burrowing owl. Another found carcass was misidentified as a fox sparrow, although a fox sparrow had been placed but misidentified. Even though the fatality search interval averaged only 5 days, the fatality searchers were unable to detect 47 species, or 59% of the species placed in the integrated detection trial, and they added another 6 species that were not placed in the trial.

Of the 148 placed birds found, searchers correctly identified 56% to species, 9.5% to a larger taxonomic group, 24% to a size category, and 9.5% to a wrong species. Average placement time preceding these outcomes were 7, 19, 23, and 47 days, respectively.

On average searchers overestimated time since death by 2.8 ± 1.4 days (SD) for 3 carcasses found within a day of placement, by 7.6 ± 11.6 days for 20 carcasses found 1 day since placement, by 8.8 ± 16.3 days for 96 carcasses found 2–12 days since placement, and by 30 ± 49 days for 14 carcasses found 13–19 days since placement. They underestimated time since death by 15.8 ± 87.3 days for 17 carcasses found 20–595 days since placement.

DISCUSSION

Bird carcasses detected in trials accurately represented the number of carcasses placed by integrating trial placements into routine fatality monitoring and adjusting the number detected using the detection rate D associated with the body masses of placed birds, thereby validating the approach as unbiased. Our approach more realistically simulated carcass detection probabilities than did separate trials for searcher detection and carcass persistence, and it eliminated multiple sources of bias discussed in Smallwood (2007, 2013), Korner-Nievergelt et al. (2011), and Smallwood et al. (2013). It avoided the significantly lower fatality estimates of small birds resulting from placement of carcasses the day before searcher detection trials. It eliminated trial administrator errors such as false conclusions of carcass removal or inadvertently alerting searchers to trial placements. Biases associated with trial duration, which strongly affect the conventional estimators, are not factors associated with our approach, and neither is the lumping of species into broad size classes. By integrating fresh trial carcasses of species typically killed by wind turbines into routine monitoring, carcasses of trial placements and fatalities are given equal opportunity for detection. In parallel with wind turbine fatalities, trial carcasses are exposed to the same number of fatality searches over the same seasons, locations, ground cover, and scavenger activity, and the same shifts or variation in search interval and personnel. In addition, by leaving all detected carcasses *in situ*, our approach leaves the ecology of scavenging unaltered other than having added trial carcasses to search areas.

However, our validation approach using one set of trial placements to adjust the number of placements detected from the other set suggested that our trial placement sample size just barely sufficed. The accuracy of the logit regression model appears sensitive to the input data, especially sufficient sample sizes. Our validation efforts indicated that placing 0.22 trial carcasses per MW per week generated precise, accurate fatality estimates, but relying on only half the carcass placements resulted in a logit regression model that generated high estimation precision but reduced accuracy in the form of over-estimation. Additional research is needed to determine minimum sample sizes.

Unlike with conventional approaches, trial carcasses in our approach can serve as training data for estimating faux fatality rates to adjust and validate actual fatality estimates because the placement rate is measured without error. Relating detection outcomes to body mass via logit regression also provides the means to estimate confidence levels, which can be extended to wind turbine fatalities detected by searchers. Ideally, the only variation in the measured rate will be the variation in placements among wind turbines because of randomization, the trial administrator's decision about which trial bird (species, age class, size, condition) to place at each randomized location, and the degree to which days intervening searches are randomized for trial placements.

Our approach enabled quantification of errors that greatly affect reported fatality estimates, including errors in species identification, estimated time since death, and number of searches per detection. We found that accuracy of species identification declines with increasing time between carcass deposition and carcass detection by searchers, and inaccuracies propagate earlier the smaller the bird. An implication of these findings is that many species killed by wind turbines are likely missing from reports of fatality monitoring at wind projects worldwide, especially at wind projects where the average search rotation was longer than 5 days. Another implication is that some species are likely erroneously reported as wind turbines fatalities. Additionally, even at our unusually short average search interval of 5 days, relying on trial placements within 1 versus 6 days of the next search can generate a 2.7-fold difference in adjusted fatality estimates for small birds and a 1.2-fold difference for large birds.

We found that estimates of time since death vary widely, and are biased high through discovery times that overlap typical fatality search intervals. Bias in estimating time since death might have resulted in too many birds being assigned to the search interval prior to the last search, or to the wrong season, the wrong year, or the wrong experimental treatment period. In some cases, this bias leads to inappropriately omitting fatalities from fatality estimation. This bias calls into question the usefulness of any fatality estimator that is based on estimates of time since death, or any experimental treatment period that is too short relative to the error in estimates of time since death.

Despite using experienced fatality searchers within a search environment that provided relatively high ground visibility, we often needed multiple searches before detecting available carcasses. The smaller the carcass, the greater the average number of searches per first detection. Because available carcasses were often missed multiple times preceding detection, the clearing searches that are often used in fatality monitoring at wind projects likely discard valuable data and do not truly clear search areas of accumulated carcasses. Another implication is that estimators requiring exclusion of carcasses judged to have persisted through a search interval will result in many exclusions of small birds and bats, although this implication might be mitigated by inaccurate estimates of time since death.

A potential bias of both our approach and conventional detection trials could be differences in spatial distributions between the training data and fatality data. Whereas we randomized trial carcass placements within search areas, true fatalities might be deposited non-randomly. Similarly, whereas we paced trial placements at 2–3 per week, true fatalities might peak seasonally. We chose to randomize placements spatially and to pace them uniformly through time because fatality finds do not necessarily inform of true spatial and temporal distributions of fatalities. Fatality finds are products of both the true distribution of carcasses and the likelihoods of searchers finding those carcasses. Search areas usually transition from cleared pads to natural vegetation or crops and sometimes from level ground to steep slopes and vegetation cover also changes seasonally. Furthermore, scavengers compete to find carcasses first, and their detection rates and removal rates are unlikely uniform across the search areas. The potential bias in our approach might be reduced by weighting trial placements seasonally and spatially as more is learned about true seasonal and spatial distributions of fatality deposition. Randomized placements that include carcass checks should eventually help develop this knowledge by quantifying when and from where trial carcasses are removed by scavengers or are detected by searchers.

Although scavengers removed and altered carcasses, we had difficulty separating the remains of only 2 rock pigeons out of 276 placed birds and 954 found bird fatalities. The key factor for minimizing confusion over carcass tracking was weekly data uploads from searchers to the lead investigator, including carcass location, descriptions of carcass condition, and photos.

Fatality Estimates

Fatality rates estimated at wind turbines known *a priori* to be hazardous to birds were indeed extraordinarily high. The all-bird fatality rate was 41 fatalities/MW/year at the selected turbines. Contributing most to these high fatality rates was detections of fatalities representing species of small birds. Not only were these wind turbines likely the most hazardous at Sand Hill, but the shorter-than-usual search interval probably also managed to detect more of the available small bird fatalities. Small bird fatalities have likely been grossly underestimated in the APWRA because of search intervals that typically average 30–42 days (Smallwood 2017).

Several of the patterns we reported warrant additional explanation. Our finding that placed carcass detections tended to peak at 25–34 m from the turbine row might have been confounded by that distance range corresponding with the locations of wind turbine access roads. The same confounding likely affected interpretations of fatality monitoring finds across the APWRA. Our finding that placed carcass detections tended lower in June might be explained by the time of year when tall grasses senesce and collapse into matts, likely obscuring carcasses. This time of year was likely underrepresented in fatality finds during past APWRA monitoring.

MANAGEMENT IMPLICATIONS

Fatality monitoring facilitates comparison of impacts and mitigation efficacy at industrial wind projects, so underlying field methods should minimize bias and sources of error. To estimate the proportion of fatalities not found by monitors, carcass detection trials should be integrated into routine fatality monitoring to estimate overall detection rates. Integrating trials into monitoring, and leaving all found carcasses *in situ*, fosters attention to realistically simulating the detection probabilities associated with fatality detection; it carries fewer biases and sources of error, is more cost-effective, and contributes to quantifying and reporting errors of species misidentification and estimated time since death. Clearing searches should be abandoned. Comparability of fatality estimates can be improved mathematically and statistically via estimators but probably more so by improving field methods that more realistically simulate carcass detection probabilities associated with fatalities.

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