DOCKETED

Docket Number:	09-AFC-07C
Project Title:	Palen Solar Power Project - Compliance
TN #:	202526
Document Title:	Exh. 3134 Smallwood Article - Response to Huso and Ericksonâ€™s Comments on Novel Scavenger Removal Trials
Description:	N/A
Filer:	Lisa Belenky
Organization:	Center for Biological Diversity
Submitter Role:	Intervenor
Submission Date:	6/23/2014 3:41:59 PM
Docketed Date:	6/23/2014

The Journal of Wildlife Management 77(2):216–225; 2013; DOI: 10.1002/jwmg.482

Commentary

Response to Huso and Erickson's Comments on Novel Scavenger Removal Trials

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ABSTRACT Trials involving volitionally placed carcasses are often used to estimate the portion of the collision-caused fatality population that is undetected by periodic fatality searches at wind turbines. Huso and Erickson criticized our paper reporting on a comparison of carcass persistence rates between what we termed conventional versus novel approaches to these trials. In our novel approach, we measured carcass persistence rates by placing only 1–2 fresh carcasses per week, instead of the typical 10 or more carcasses at a time, often using found carcasses of unknown time since death. Huso and Erickson directed most of their critique to this novel aspect of our approach, although the novelty of our approach also included the use of event-triggered camera traps, which we used to record exact times of removals and to identify vertebrate scavenger species responsible for the removals. In our replies to Huso and Erickson's major criticisms, we acknowledge flaws in our field methods for arriving at fatality rate estimates, but we also point out the larger flaws in the methods used by Huso and Erickson, especially in their use of mean days to removal as a measure of carcass persistence. We conclude by introducing a more appropriate detection trial, which combines searcher detection and scavenger removal trials, and integrates this detection trial into periodic fatality monitoring. © 2013 The Wildlife Society.

KEY WORDS bird fatalities, camera traps, carcass persistence trial, fatality rate estimates, scavenger removal trial, searcher detection trial, wind turbine collisions.

Huso and Erickson (2013) criticized our reporting of a novel scavenger removal trial that we implemented at a wind project site in Vasco Caves Regional Preserve, Alameda County, California (Smallwood et al. 2010). Scavenger removal trials, also known as carcass persistence trials, are typically implemented along with searcher detection trials to estimate the proportion of birds or bats killed by wind turbines but not discovered during periodic fatality searches. The conventional approach to performing persistence trials has been to place $\geq$10 carcasses at a time within or nearby the fatality search areas, followed by scheduled status checks by a designated person. Carcasses are considered removed if remains total <10 body feathers, <3 flight feathers, and no bones or connective tissue. Carcass persistence rates have been measured as either 1) the proportion of carcasses remaining after the number of days into the trial period that corresponds with the average search interval of the fatality monitoring, or 2) mean days to removal. Our trial was novel by using event-triggered cameras to identify the species of vertebrate scavengers and to record times of removal to the minute and second rather than to the day or week. The latter novelty improved our understanding of the proportion of carcasses remaining with increasing time into the trial. Our trial was also novel by placing 1–2 trial carcasses per week, which we believed would minimize the potential effects of scavenger swamping (Smallwood 2007). We compared our removal rates to the average removal rates from other wind projects across the United States, and we compared fatality rates at our studied wind project based on conventional versus our novel persistence trials.

Huso and Erickson (2013) made numerous criticisms of our paper, summarized by the following 3 major complaints:

1. Comparing persistence rates from our single study to an average from several studies was irrelevant.
2. Many of the statistical methods were implemented incorrectly, calling our results into question.
3. Conclusions drawn from the presented evidence were not supported by the reported results.

Before addressing each of these 3 critiques specifically, we will compare the most often used fatality estimators to highlight the 2 competing approaches for quantifying carcass persistence. Huso and Erickson advocate for mean days to removal, which is one of these competing approaches, and they criticized our use of proportion of carcasses remaining, which is the other competing approach. Both approaches to

Received: 29 August 2012; Accepted: 4 September 2012
Published: 7 January 2013

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quantifying carcass removal rates warrant critical discussion, because biases or errors in either approach can result in wildly different fatality rate estimates. The following fatality estimators demonstrate how carcass removal rates are measured and how they contribute to fatality rate estimates.

COMPARISON OF FATALITY ESTIMATORS

Horvitz–Thompson Estimator

Fatality estimates based on the proportion of carcasses remaining are often derived from a simple formula introduced by Horvitz and Thompson (1952), which we modified for our fatality rate estimation as follows:

$$F_A = \frac{F_U}{p \times R_c}$$

where F_A and F_U are adjusted and unadjusted fatality rate estimates, respectively, p is the search detection rate expressed as the proportion of available carcasses that are actually found, and R_c is the scavenger removal rate expressed as the proportion of carcasses remaining at the time of the search, assuming scavengers were responsible for removing the missing carcasses:

$$R_c = \frac{\sum_{i=1}^{I} R_i}{I}$$

where R_i is the predicted proportion of carcasses remaining at the ith day into the trial, and I is the day into the trial which corresponds with the average search interval of the fatality monitoring. To smooth the daily representation of R_i values that derive R_c, we use least-squares regression to fit a curve to R_i as a function of days into the trial, but other approaches can be used, such as survival analysis and visual approximation of the plot.

The standard error of the adjusted fatality rate, SE[F_A], is calculated using the delta method (Goodman 1960):

$$\mathrm{SE}[F_A] = \sqrt{\left(\frac{1}{p \times R_C} \times \mathrm{SE}[F_U]\right)^2 \times \left(\frac{F_U}{p} \times \frac{-1}{{R_C}^2} \times \mathrm{SE}[R_C]\right)^2 \times \left(\frac{F_U}{R_C} \times \frac{-1}{p^2} \times \mathrm{SE}[p]\right)^2}$$

Erickson–Shoenfeld Estimator

Erickson et al. (2000) formulated a fatality estimator based on mean days to carcass removal, which P. Shoenfeld (unpublished report; Table 1) found to be biased low by about 20%. Shoenfeld modified the estimator to mitigate the bias, and the resulting estimator was intended for use with periodic fatality searches:

$$F_A = \left(\frac{N \times I \times F_U}{n \times t \times p}\right)\left(\frac{e^{I/t} - 1 + p}{e^{I/t} - 1}\right)$$

where N was the total number of turbines in the project (or total megawatts [MW] of rated capacity in project), n was the number of turbines sampled (or number of MW sampled), and $\bar{t}$ was mean days to removal of carcasses placed in removal trials.

An early version of mean days to removal was the following:

$$\bar{t} = \frac{\sum_{i=1}^{S} t_i}{S}$$

where S was the number of carcasses placed, and t_i represented the days into the trial when the ith carcass was removed. Carcasses persisting to the end of the trial were undefined in terms of days to removal, so we assumed they would have been excluded from the calculation of the mean. A later version of mean days to removal included all placed carcasses in the calculation by using a maximum likelihood estimator:

$$\bar{t} = \frac{\sum_{S=1}^{c} t_i}{S - S_c}$$

where S_c was the number of carcasses persisting to the end of the trial, which were right-censored.

To calculate standard error for the adjusted fatality rate, P. Shoenfeld (unpublished report; Table 1) recommended use of Monte Carlo simulation, where p was drawn as a binomial random variable based on the sample sizes of carcasses placed in the detection trials, and $\bar{t}$ was drawn as a normal random variable.

Huso Estimator

Huso (2010) proposed an adjustment to the Erickson–Shoenfeld estimator to account for search intervals that are inappropriately long for the average time to removal of particular species, especially small birds and bats:

$$F_A = \frac{F_U}{r \times p \times v}$$

where $r = t \times (1 - e^{-\min(I_e, I)/t})/\min(I_e, I)$, $I_e = \log(0.01) \times t$, and $v = \min(1, I_e/I)$.

The term I_e was referred to as the effective interval, intended for species whose carcasses were not expected to persist on site throughout the periodic fatality search interval.

Key Differences and Similarities

Both Huso (2010) and Erickson et al. (2000) assumed that the probability that a carcass will persist for d days is exponential, $r_i = e^{-d/\bar{t}}$, whereas we measured the probability distribution more directly using the daily proportion of carcasses remaining. Consider an example, where Insignia Environmental (unpublished report; Table 1) volitionally placed common quail (*Coturnix coturnix*) and rock pigeon (*Columba livia*) carcasses to estimate persistence rates that they could use to adjust fatality estimates from 15 day search intervals (Fig. 1). At 15 days, the measured probability of persistence of common quail carcasses was >twice the probability that would have been assumed by Huso and Erickson (Fig. 1).

Differences also existed in determining when during the trial the persistence rate should be measured, and how persisting carcasses were treated in the measurement. In our

Table 1. Unpublished reports of bird collision studies at wind projects in the United States, from which we used data or cited as examples of methodology.

Report	Reference
1	Arnett, E. B., M. R. Schirmacher, M. M. P. Huso, and J. P. Hayes. 2009. Patterns of bat fatality at the Casselman Wind Project in south-central Pennsylvania: 2008 Annual report. Report to Bats and Wind Energy Cooperative and the Pennsylvania Game Commission. Bat Conservation International, Austin, Texas, USA
2	Derby, C, A. Dahl, W. Erickson, K. Bay, and J. Hoban. 2007. Post-construction monitoring report for avian and bat mortality at the NPPD Ainsworth Wind Farm. Report to Nebraska Public Power District, Columbus, Nebraska, USA
3	Downes, S., and R. Gritzki. 2012. White Creek I wildlife monitoring report: November 2007–November 2011. Report to White Creek Wind I, LLC, Roosevelt, Washington, USA
4	Enk, T., K. Bay, M. Sonnenberg, J. Baker, M. Kesterke, J. R. Boehrs, and A. Palochak. 2010. Biglow Canyon Wind Farm Phase I post-construction avian and bat monitoring second annual report, Sherman County, Oregon: January 26, 2009–December 11, 2009. Report to Portland General Electric Company, Portland, Oregon, USA
5	Enz, T, and K. Bay. 2010. Post-Construction Avian and Bat Fatality Monitoring Study, Tuolumne Wind Project, Klickitat County, Washington. Final Report: April 20, 2009 to April 7, 2010. Report to Turlock Irrigation District, Turlock, California, USA
6	Erickson, W. P., G. D. Johnson, M. D. Strickland, and K. Kronner. 2000. Final report: Avian and bat mortality associated with the Vansycle Wind Project, Umatilla County, Oregon: 1999 study year. Report to Umatilla County Department of Resource Services and Development, Pendleton, Oregon, USA
7	Erickson, W. P., K. Kronner, and B. Gritski. 2003. Nine Canyon Wind Power Project avian and bat monitoring report. Report to Nine Canyon Technical Advisory Committee, Energy Northwest, location not given
8	Erickson, W. P., J. Jeffrey, K. Kronner, and K. Bay. 2004. Stateline wind project wildlife monitoring final report, July 2001–December 2003. Technical Report submitted to FPL Energy, the Oregon Energy Facility Siting Council and the Stateline Technical Advisory Committee
9	Erickson, W. P., J. D. Jeffrey, and V. K. Poulton (WEST, Inc.). 2008. Puget Sound Energy Wild Horse Wind Facility Post-construction Avian and Bat Monitoring First Annual Report: January–December 2007. Report to Puget Sound Energy, Ellensburg, Washington, USA
10	Gruver, J., M. Sonnenburg, K. Bay, and W. Erickson. 2009. Post-Construction Bat and Bird Fatality Study at the Blue Sky Green Field Wind Energy Center, Fond du Lac County, Wisconsin. July 21, 2008–October 31, 2008 and March 15, 2009–June 4, 2009. Report to WE Energies, Milwaukee, Wisconsin, USA
11	Howe, R., and R. Atwater. 1999. The potential effects of wind power facilities on resident and migratory birds in Eastern Wisconsin. Report to Wisconsin Department of Natural Resources, Monoana, Wisconsin, USA
12	Howe, R., W., W. Evans, and A. T. Wolf. 2002. Effects of wind turbines on birds and bats in northeastern Wisconsin. Report to Wisconsin Public Service Corporation and Madison Gas and Electric Company, Green Bay, Wisconsin, USA
13	Insignia Environmental. 2011. Draft Final Report for the Buena Vista Avian and Bat Monitoring Project. Report to County of Contra Costa, Martinez, California, USA
14	Jain, A., P. Kerlinger, R. Curry, and L. Slobodnik. 2007. Annual report for the Maple Ridge Wind Power Project postconstruction bird and bat fatality study—2006. Prepared for PPM Energy and Horizon Energy. Location not given
15	Jain, A., P. Kerlinger, R. Curry, L. Slobodnik, J. Histed, and J. Meacham. 2009. Annual report for the Noble Clinton Windpark, LLC post-construction bird and bat fatality study—2008. Report to Noble Environmental Power, LLC, location not given
16	Jeffrey, J., K. Bay, W. Erickson, M. Sonnenberg, J. Baker, M. Kesterke, J. R. Boehrs, and A. Palochak. 2010. Portland General Electric Biglow Canyon Wind Farm Phase I post-construction avian and bat monitoring first annual report, Sherman County, Oregon: January, 2008–December, 2008. Report to Portland General Electric Company, Portland, Oregon, USA.
17	Johnson, G. D., W. P. Erickson, M. D. Strickland, M. F. Shepherd, and D. A. Shepherd. 2000. Final report: Avian monitoring studies at the Buffalo Ridge, Minnesota Wind Resource Area: Results of a 4-year study. Report for Northern States Power Company, Minneapolis, Minnesota, USA
18	Johnson, G. J., W. P. Erickson, J. White, and R. McKinney. 2003. Avian and bat mortality during the first year of operation at the Klondike Phase I Wind Project, Sherman County, Oregon. Report to Northwestern Wind Power, Goldendale, Washington, USA
19	Kerlinger, P., R. Curry, L. Culp, B. Fischer, A. Hasch, A. Jain, and C. Wilkerson. 2008. Post-construction avian monitoring study for the Shiloh I Wind Power Project, Solano County, California: Two year report. Report to PPM Energy, Portland, Oregon, USA
20	Kronner, K., B. Gritski, and S. Downes. 2008. Big Horn Wind Power Project Wildlife fatality monitoring study 2006-2007. Report to PPM Energy, Portland, Oregon, USA
21	Northwest Wildlife Consultants, Inc., and WEST, Inc. 2007. Avian and bat monitoring report for the Klondike II Wind Power Project, Sherman County, Oregon. Report to PPM Energy, Portland, Oregon, USA
22	Shoenfeld, P. 2004. Suggestions regarding avian mortality extrapolation. Report to West Virginia Highlands Conservancy, Davis, West Virginia, USA
23	TRC Environmental Corporation. 2008. Post-construction avian and bat fatality monitoring and grassland bird placement surveys at the Judith Gap Wind Energy Project, Wheatland County, Montana. Report to Judith Gap Energy, LLC, Chicago, Illinois, USA
24	URS Corporation. 2010. Final Goodnoe Hills Wind Project Avian Mortality Monitoring Report. Report to PacifiCorp, Salt Lake City, Utah, USA
25	Young, D. P., W. P. Erickson, R. E. Good, M. D. Strickland, and G. D. Johnson. 2003. Final Report: Avian and bat mortality associated with the initial phase of the Foote Creek Rim Windpower Project, Carbon County, Wyoming. WEST, Inc., Cheyenne, Wyoming, USA
26	Young, Jr., D. P., W. P. Erickson, J. D. Jeffrey, and V. K. Poulton. 2007. Puget Sound Energy Hopkins Ridge Wind Project Phase 1 post-construction avian and bat monitoring first annual report. Report to Puget Sound Energy, Dayton, Washington, USA
27	WEST, Inc. 2006. Diablo Winds wildlife monitoring progress report: March 2005–February 2006. Report to unstated recipient at unknown location

approach, all placed carcasses contributed to the estimate, and the measurement of proportion of carcasses remaining was made after the same number of days that corresponded with the average search interval. Huso and Erickson right censored carcasses persisting to the end of the trial, and they provided no guidance on when mean days to removal should be estimated. Across multiple scavenger removal trials, mean days to removal has been measured after a time into the trial that was shorter, equal to, and longer than the average search interval in fatality monitoring (Table 2).

A similarity between our approaches was sacrificial pseudoreplication (sensu Hurlbert 1984), explained below.

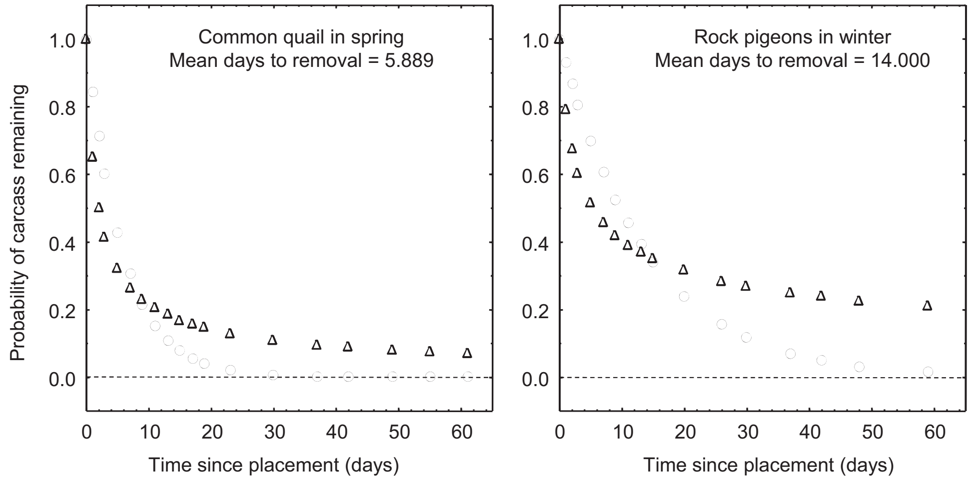

Figure 1. The daily probability of carcass persistence based on the proportion of carcasses remaining into the trial (triangles) and on the assumed exponential carcass removal rate, $r_i = e^{-d/\bar{t}}$ (circles) for common quail placed in spring with a mean days to removal of 5.889 (left graph) and for rock pigeons placed in winter with a mean days to removal of 14 (right graph). Data were from the Buena Vista Wind Energy project in the Altamont Pass Wind Resource Area, California (Insignia Environmental, unpublished report; Table 1).

Another similarity was the unrealistic assumption that in performing the trials, no interaction existed between scavenger removal rates and searcher detection rates, such as repeat opportunities to find persisting carcasses and changes in conspicuousness among persisting carcasses.

A third similarity was the introduction of another searcher detection probability into the carcass persistence trials. This probability is separate from searcher detection rates measured in directed trials. In both conventional and our novel persistence trials, designated investigators are needed to perform scheduled status checks on placed carcasses. Even though our camera traps could record exact times of removal, we needed to be certain the carcass was not moved just beyond the camera's infrared event field. In both types of persistence trial, the designated investigator knows where to check on the placed carcass and will likely devote more effort toward detecting these particular carcasses than a fatality searcher would devote, on average, toward finding wind

Table 2. Scavenger removal trials have varied in duration relative to the average search interval used in fatality monitoring: sometimes briefer, sometimes equal, and sometimes longer than the search interval. Whether the mean days to removal was right-censored, also varied, but uncensored calculations were more common earlier.

Source	Project site	Trial duration	Search interval	Right-censored?
Erickson et al. (2000)[a]	Vansycle, OR	28	28	No
Johnson et al. (2000)	Buffalo Ridge, MN	14	14	No
Young et al. (2003)[a]	Foot Creek Rim, WY	28	28	No
Johnson et al. (2003)[a]	Klondike I, OR	28	29	Yes
Erickson et al. (2003)[a]	Nine Canyon, WA	30	15	Yes
Erickson et al. (2004)[a]	Stateline, WA/OR	40	23	Yes
Fiedler (2004)	Buffalo Mountain, TN	21	7	Yes
Anderson et al. (2005)	Tehachapi Pass, CA	8	90	No
Anderson et al. (2005)	San Gorgonio, CA	8	90	No
WEST, Inc. (2006)[a]	Diablo Winds, CA	62	33	Yes
Northwest Wildlife Consultants and WEST, Inc. (2007)[a]	Klondike II, OR	28	28	Yes
Young et al. (2007)[a]	Hopkins Ridge, WA	40	28	No
Derby et al. (2007)[a]	Ainsworth	30	14	Yes
TRC Environmental Corporation (2008)[a]	Judith Gap, MT	20	30	Yes
Kronnor et al. (2008)[a]	Big Horn, WA	30	28	No
Young et al. (2008)[a]	Wild Horse, WA	40	28	Yes
URS (2010)[a]	Goodnoe Hills, OR	7	28	No
URS (2010)[a]	Goodnoe Hills, OR	20	28	No
Arnett et al. (2009)[a]	Casselman, PA	20	1	Yes
Gruver et al. (2009)[a]	Blue Sky, WI	30	1	Yes
Gruver et al. (2009)[a]	Blue Sky, WI	30	5	Yes
Jeffrey et al. (2009) and Enk et al. (2010)[a]	Biglow Canyon, OR	40	28	Yes
Insignia (2011)[a]	Buena Vista, CA	20	15	No
Downes and Gritski (2012)[a]	White Creek, WA	35	3.5	No
Downes and Gritski (2012)[a]	White Creek, WA	35	7	No
Downes and Gritski (2012)[a]	White Creek, WA	35	28	No

[a] Unpublished report. Additional information available in Table 1.

turbine-deposited carcasses in routine monitoring. No adjustment has been made for this biased detection probability, not by Huso or Erickson, nor by us.

RESPONSES TO SPECIFIC CRITICISMS

Comparing Persistence Rates From Our Single Study to Averages From Several Studies Was Irrelevant

In support of the first criticism, Huso and Erickson argued that our novel removal trials should have been conducted simultaneously with conventional removal trials as an on-site experiment. For the sake of discussion, we will define the conventional carcass persistence trial as windfall placement, meaning the placement of 10, 20, or more carcasses at a time. In our trials, we placed 1–2 carcasses at a time at regular intervals throughout the fatality monitoring period to minimize the potential for scavenger swamping. We would rather have performed the experiment advocated by Huso and Erickson, but comparing persistence rates between windfall and regular carcass placements at the scale of our wind project would have confounded the treatments. At the scale of our study, and of most wind projects, the effects of windfall placement will certainly interfere with the effects of regular placement, because individual common ravens (*Corvus corax*), coyotes (*Canis latrans*), and other vertebrate scavengers can readily forage over our entire study area as well as over large portions of the largest wind projects. Huso and Erickson's argument for an on-site experiment was unrealistic in our case.

Huso and Erickson further argued that comparing persistence rates from a single trial to averages from multiple other trials provided no insight because the averages were based on persistence rates that were both greater and less than the averages. We agree that our comparison was weak, which was why we performed no statistical test on the comparison. To provide a more complete comparison, we present all the available data from similar persistence trials compared to the results of our trial (Fig. 2). Carcass removals appeared to have been faster in our trial, but we agree that additional trials are needed.

Since our 2010 paper, we realized that we had the means to test the magnitude of another potential bias in persistence trials, and that this bias could also explain our lower persistence rates. Greater time since death at the time of carcass placement could increase persistence rates, because vertebrate scavengers likely grow increasingly disinterested in carcasses as decomposition advances. This bias is relevant to our comparison of persistence rates between trials, because other trials across the United States often included carcasses found at wind turbines or on roadways, meaning that decomposition had advanced for up to several days before use in the trial. We tested this effect by re-assigning trial start dates for each of our placed carcasses yet to be removed at 0, 2, and 4 days since thawing (i.e., since placement). For carcasses persisting after 2 days since placement, we began another trial as if we had just placed the 2-day old carcass, and we did the same for carcasses persisting after 4 days. Persistence rates increased after beginning trials with older carcasses (Fig. 3). Many of the persistence rates presented from other studies (Fig. 2) could have been greater than ours because of carcass age. At this point, we do not know whether our persistence rates were lower because we reduced the likelihood of scavenger swamping, because we used fresher carcasses, or other reasons.

Many of the Statistical Methods Implemented Incorrectly, Calling Results Into Question

Believing we did not provide our data, Huso and Erickson relied on speculation to come to conclusions about flaws in our study design and statistical analysis. However, Smallwood et al. (2010) referenced www.wildlifejournals.org as the location of Supplemental Materials, including our data.

According to Huso and Erickson, we extrapolated our model predictions of persistence rates far beyond the limits of our measured data. We acknowledge that we extended our

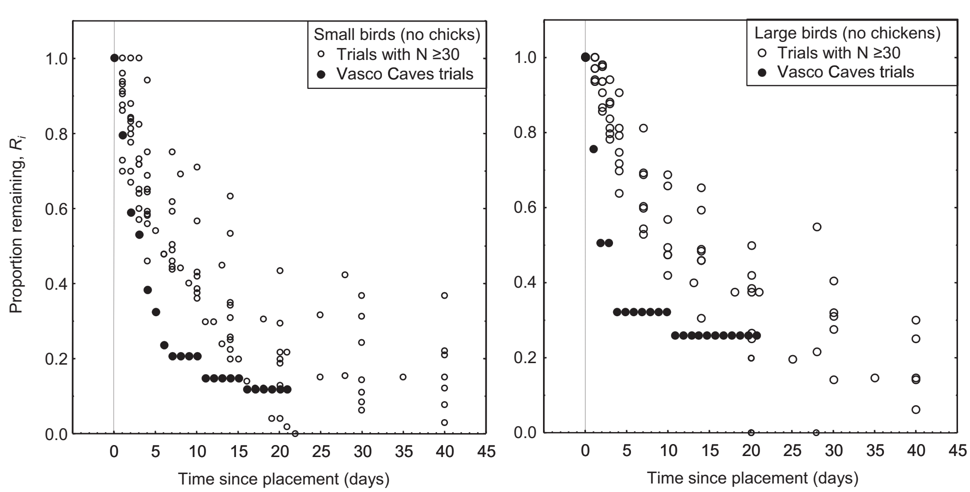

Figure 2. Bird carcass removal rates appeared to be fastest in the trial at Vasco Caves Regional Preserve, California, compared to trials across North America involving the placement of $\geq$30 small bird carcasses (left) or $\geq$30 large bird carcasses (right). Data were from unpublished reports listed in Table 1 as: Howe and Atwater (1999), Erickson et al. 2000, 2004, 2008), Howe et al. (2002), Johnson et al. (2003), Kerns (2005), Jain et al. (2007, 2009), Young et al. (2007), Kerlinger et al. (2008), TRC Environmental Corporation (2008), Jeffrey et al. (2009), Enz and Bay (2010), and Enk et al. (2010).

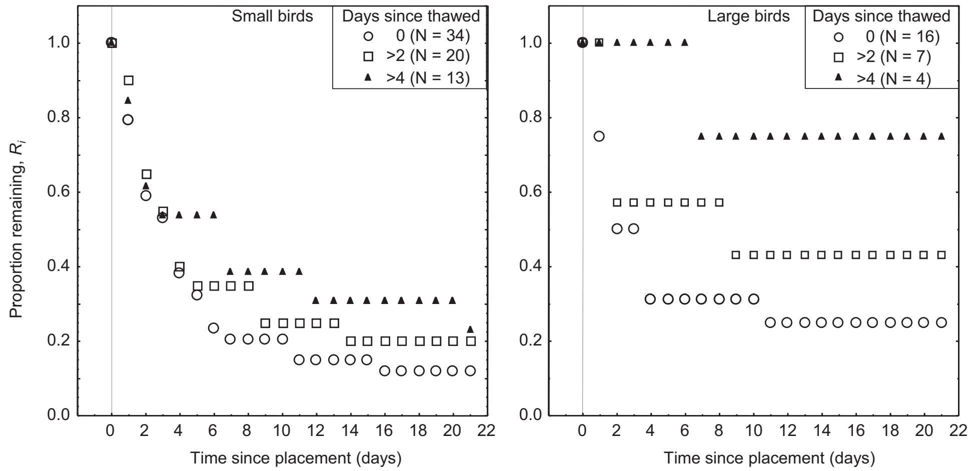

Figure 3. Bird carcass removal rates were increasingly slower in our trials at Vasco Caves Regional Preserve, California, when we started the trials 2 and 4 days since carcass placement of small birds (left) and large birds (right).

model-predicted persistence rates (Fig. 2 in Smallwood et al. 2010) for the sake of graphical comparison to Smallwood (2007), but these extrapolations were of no consequence to our estimation of fatality rates. To adjust our fatality rate estimates, we used our model predictions at the number of days into the trial that corresponded with the 15-day average search interval of our fatality monitoring (Smallwood et al. 2010:1092). Huso and Erickson's criticism applied only to an illustration, but not to our actual estimation of fatality rates.

Huso and Erickson argued that we should have used a more appropriate statistical model for predicting carcass persistence rates, such as survival analysis. We would agree if our objective was to test a research hypothesis. However, we used least-squares regression analysis only as a tool for objectively estimating proportions of carcasses remaining at 1 or more times into a trial. The results would have been nearly the same regardless of the statistical model used, including an approximation made by a visual examination of the plotted data.

Finally, Huso and Erickson alleged that our regression model of persistence rates was pseudoreplicated (sensu Hurlbert 1984) by substituting the actual observational unit with a unit that does not reflect the intended scope of inference. Specifically, they claimed that we treated days into the trial as the observational unit and not the carcass. We disagree with their conclusion, because our intended scope of inference was the proportion of carcasses remaining after time periods corresponding to the average search intervals used in fatality monitoring. Our intended scope of inference was consistent with the general use of proportions for adjusting rate estimates in the original formulation of the estimator we used (Horvitz and Thompson 1952). Our observational unit was the proportion of carcasses remaining, and days into the trial was the predictor variable.

To obtain the proportion of carcasses remaining, we observed the pool of placed carcasses. This pooling of carcasses commits a form of pseudoreplication, which Hurlbert (1984) called "sacrificial pseudoreplication." By pooling the outcomes of carcass placements, we sacrificed our ability to estimate the variation in outcomes after the time periods that corresponded with average search intervals in fatality monitoring. Recognizing this shortcoming, we averaged the proportions of carcasses remaining after each day into the trial, and we used the delta method (Goodman 1960) to carry the error from these averages through the final estimations of fatality rates (see Smallwood 2007). However, a superior method for overcoming sacrificial pseudoreplication is to treat the trial as the observational unit and to repeat the trial to obtain a range of outcomes. This approach was the basis of Smallwood (2007), who took averages and variances from all of the reported removal trials for each of several size classes.

The estimators used by Huso and Erickson also suffer from sacrificial pseudoreplication. For use in their estimators, Huso and Erickson obtain mean days to carcass removal. Theirs is often nothing more than a pooling of outcomes of individual carcass placements, due to their use of the maximum likelihood estimator, which negates a direct estimation of variance (e.g., WEST, Inc., unpublished report and W. P. Erickson, J. D. Jeffrey, and V. K. Poulton, WEST, Inc., unpublished report; Table 1). Huso, Erickson, and Shoenfeld suggested using Monte Carlo simulation or bootstrapping to estimate variance, but the proposed methodology remains vaguely described in fatality monitoring reports, the methods used have likely varied considerably, and many reports using the Erickson–Shoenfeld estimator did not report standard error.

Conclusions Were Not Supported by the Reported Results

In their Figure 1, Huso and Erickson inappropriately compared fatality rate estimates derived from conventional and novel persistence trials in terms of fatalities/turbine/year. Our fatality rate estimates were from 2 types of wind turbine, 1 being 5× larger than the other. To avoid confounding by turbine size, Huso and Erickson's comparison should have used fatalities/MW/year as the fatality rate metric (see Smallwood et al. 2010).

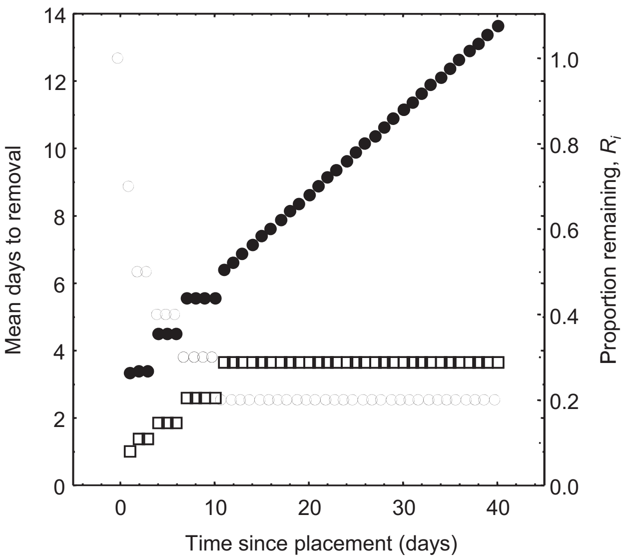

Figure 4. Hypothetical persistence rates when removals of 10 placed carcasses were 3 on day 1, 2 on day 2, 1 on day 5, 1 on day 8, and 1 on day 11. We represent persistence rates as proportion of carcasses remaining (open circles), uncensored mean days to removal (squares), and censored mean days to removal (closed circles).

Huso and Erickson had some fun suggesting that our results implied the creation of live birds by wind turbines, because our confidence intervals often extended below 0. They alleged we made a serious error in the calculation of either the average fatality rate or its standard error or both. Huso and Erickson admonished that the negative lower confidence limits (LCLs) should have alerted us to inadequacy in the statistical models chosen for the data. We believe that larger inadequacies lie in the data collection methods applied to fatality monitoring and the detection trials. These concerns can be traced to Smallwood and Thelander (2004, 2005), who showed graphically how the 0-dominated datasets typical of fatality monitoring can yield smaller error terms by applying greater search effort. They also can be traced to Smallwood (2007), who detailed many potentially serious biases and sources of error, and to Smallwood and Thelander (2008), who pointed out the frequent negative LCLs and why they believed these negative values were being obtained.

In many wind projects, including in the Altamont Pass, fatality finds are statistical rare events that are clustered among turbines, resulting in fatality rates averaging near 0

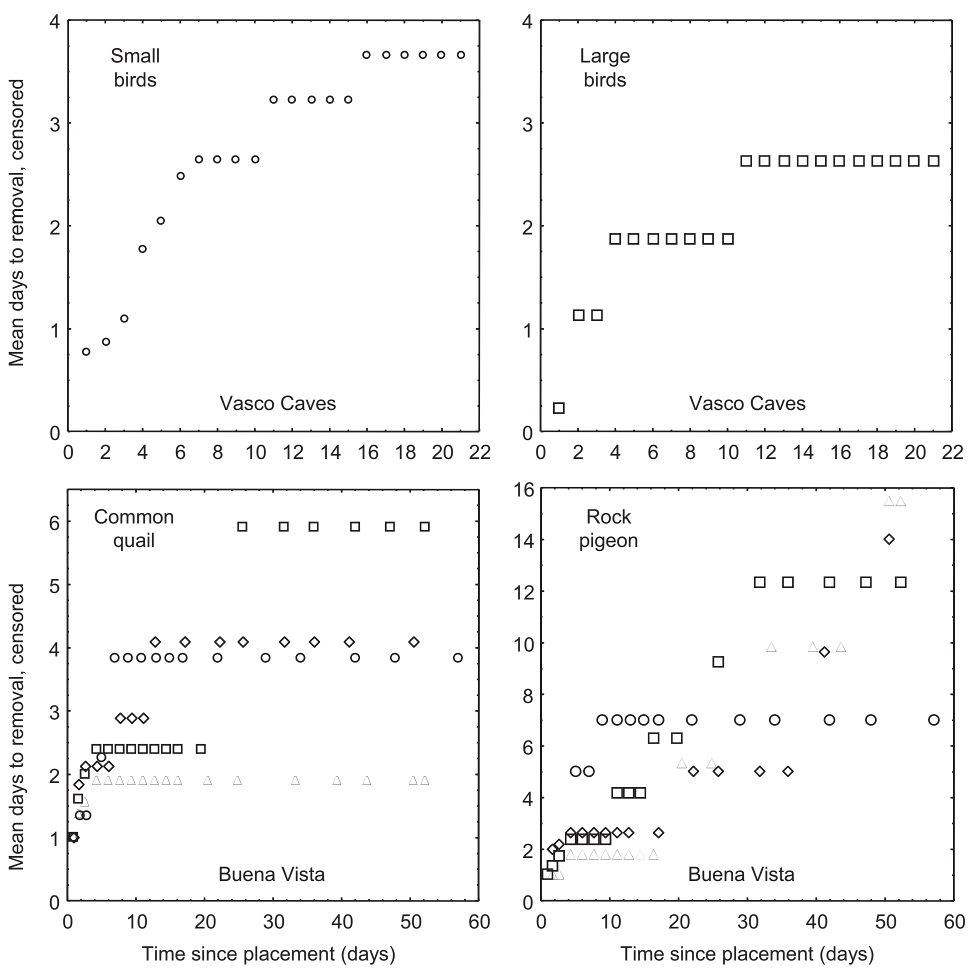

Figure 5. Estimates of censored mean days to removal increased with time into the trial for small birds (top left graph) and large birds (top right) in the Vasco Caves Regional Preserve, California, and for common quail (*Coturnix coturnix*; lower left) and rock pigeons (lower right) placed at the Buena Vista Wind Energy project, California (Insignia, unpublished report; Table 1). At Buena Vista, seasonal placements are represented by circles for spring, squares for summer, triangles for fall, and diamonds for winter.

and with large variance terms. When Smallwood and Thelander (2008) addressed the issue of large standard errors that led to LCLs <0, they cited other studies that also reported LCLs <0, including Orloff and Flannery (1992), Anderson et al. (2004, 2005), and D. P. Young, W. P. Erickson, R. E. Good, M. D. Strickland, and G. D. Johnson (WEST, Inc., unpublished report; Table 1). Additional examples included TRC Environmental Corporation (unpublished report) and J. Jeffrey, K. Bay, W. Erickson, M. Sonnenberg, J. Baker, M. Kesterke, J. R. Boehrs, and A. Palochak (WEST, Inc., unpublished report; Table 1), both of whom obviously replaced a negative LCL with 0 for the all raptor fatality rate estimates, and T. Enk, K. Bay, M. Sonnenberg, J. Baker, M. Kesterke, J. R. Boehrs, and A. Palochak (WEST, Inc., unpublished report; Table 1) reported an LCL of 0 for all raptors, even though they found raptor fatalities. We note that Erickson was a co-author of 4 of the reports cited above.

The following case example can illustrate the problem associated with estimating LCLs <0. Assume that 1 year of fatality searches at 20 1-MW turbines resulted in 2 American kestrels (*Falco sparverius*) found at 1 turbine, 1 kestrel found at another turbine, and 0 kestrels found at 18 turbines. The fatality rate yet to be adjusted for the proportion of fatalities undetected would be 0.15 kestrels/MW/year, and the standard error would be 0.1094. A 90% confidence interval would then be $1.645 \times 0.1094 = 0.18$, resulting in the fatality rate estimate ranging from -0.30 to 0.33. The negative LCL resulted from a long average search interval applied to a brief monitoring period that yielded a 0-dominated data set. We report negative LCLs to be honest about our level of confidence in our estimates (following Smallwood and Thelander 2008), but we trust the reader to know that the more realistic confidence limits for the above example would be >0 to 0.33.

Summary

Our trial was only a step in a larger scientific effort to develop more realistic methods to estimate the proportion of fatalities not detected during periodic fatality monitoring. We have not repeated the methods used in Smallwood et al. (2010). Instead, we have taken what we learned and proceeded to improve the overall detection trial methodology. In the meantime, many deeply flawed carcass persistence trials and searcher detection trials have been performed in wind projects across North America.

THE LARGER PROBLEM

Despite sacrificial pseudoreplication, the proportion of carcasses remaining provides an easily interpretable metric of carcass persistence after a time period that corresponds with the average search interval. In contrast, Huso and Erickson have provided no guidance on how long a trial should be performed to obtain a representative estimate of mean days to carcass removal. If our average fatality search interval was 15 days, then should we have estimated mean days to removal from a trial lasting 15 days? Or should it have been 5 days, 30 days, or 62 days? The answer to these questions has not been provided by Huso or Erickson, even though it can affect the outcome, as discussed below.

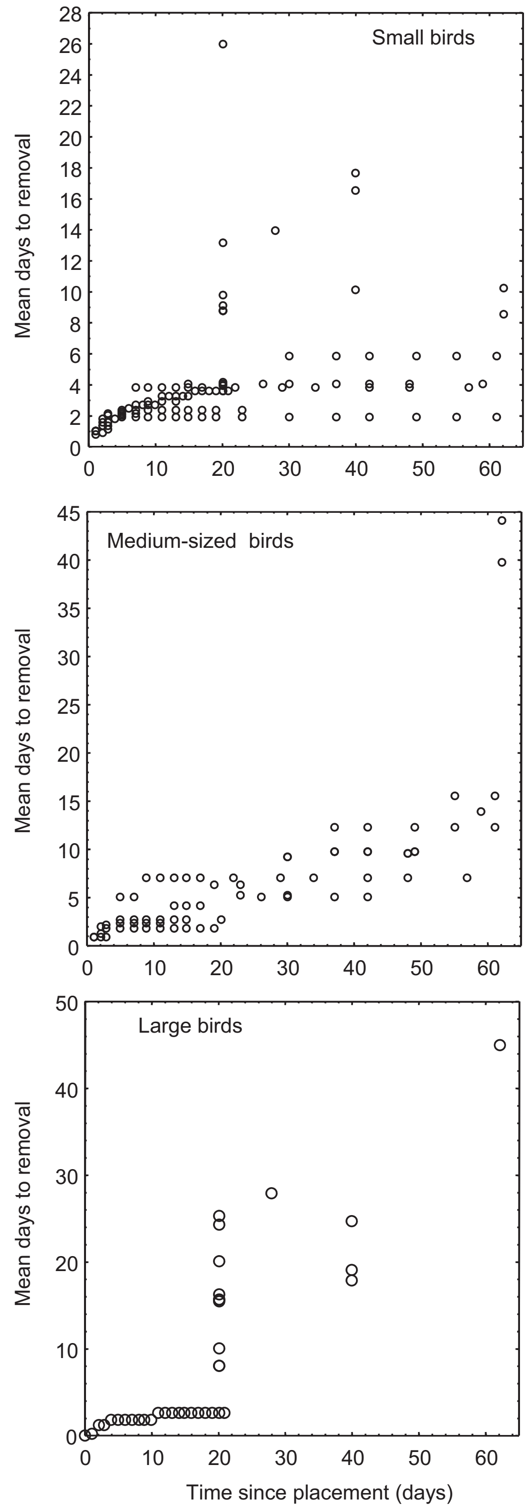

Figure 6. Including all trials in which bird carcasses were placed at wind turbines, censored mean days to removal increased with time into the trial for small birds (top), medium-sized birds (middle), and large birds (bottom) among wind projects across North America. The 2 outliers at the right side of the middle graph were small samples of rock pigeons and 3 found red-tailed hawks (*Buteo jamaicensis*) placed in the Altamont Pass Wind Resource Area (WEST, Inc., unpublished report; Table 1).

Mean days to carcass removal is a function of the duration of the removal trial (Fig. 4). The uncensored mean days to

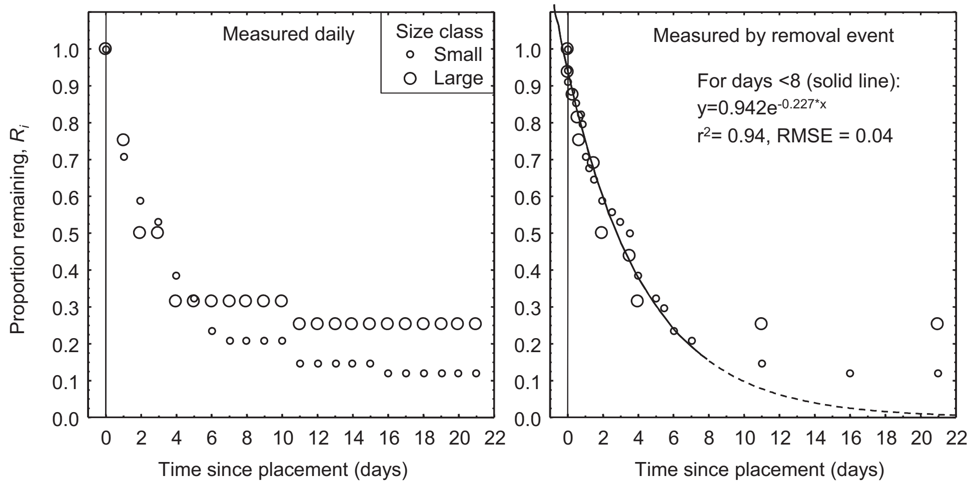

Figure 7. Persistence rates of placed carcasses of small and large birds in scavenger removal trials in the Vasco Caves Regional Preserve, Contra Costa County, California (Smallwood et al. 2010). Persistence rates were measured as daily proportion of carcasses remaining (left graph), and as proportion of carcasses remaining at each removal date (right graph). Using least-squares regression, daily persistence rates were best fit with an inverse power function (Smallwood et al. 2010), whereas persistence rates on removal dates shifted many of the data points left, resulting in an exponential function being the best fit for both small and large bird carcasses during the first 8 days since carcass placements (solid line). The fitted curve was extended to 21 days (dashed line) to illustrate the magnitudes of deviation of remaining carcasses from the exponential persistence probabilities.

removal increases with days into the trial until no more carcasses are removed, and corresponding with the same time the proportion of carcasses persisting reaches an asymptote. The uncensored mean days to removal is thus readily interpretable so long as the trial end date corresponds with the average search interval, and so long as at least 1 carcass has been removed (otherwise the mean is undefined). However, the uncensored mean requires large numbers of birds or bats to be placed in the trial because persisting carcasses cannot contribute to the mean estimate (this is not a problem for the metric we use, the proportion of carcasses remaining).

The censored mean days to removal increases with days into the trial for as long as the trial lasts (Fig. 4). The maximum likelihood estimator causes an artificial inflation of mean days to removal. We found that censored mean days to removal increased with time into the trial in our study at Vasco Caves and in the study at the Buena Vista Wind Energy Project to the east of our study site (Insignia Environmental, unpublished report; Table 1, Fig. 5). We also found the same pattern in all of the available estimates of censored mean days to removal across North America (Fig. 6). Given these patterns, and only narrowly considering the role of carcass persistence rates in adjusting fatality rate estimates, we conclude that use of mean days to carcass removal likely contributed to underestimation of fatality rates at many wind projects in North America.

On the other hand, unrealistic searcher detection trials likely contributed to overestimation of fatality rates at many wind projects. Searcher detection trials have tested the searcher's discovery rate of whole, fresh carcasses (Smallwood 2007), and this rate has been derived from a single opportunity to search for the placed carcasses. In reality, carcasses deposited by wind turbines will have been exposed to the elements for various periods of time, including periods overlapping multiple periodic fatality searches in routine monitoring. So long as it persists, a carcass missed by the fatality searchers can be found on the second search of the area, or on the third, fourth, or fifth search, etc. (Korner-Nievergelt et al. 2011). The shorter the search interval, the more opportunities searchers will have to detect a persistent carcass. Over time, some carcasses can expand their detection profile as feathers and body parts spread from the original deposition site, and others can become more obscure as material disappears, colors fade, or grass overgrows.

TOWARD SOLUTIONS

Our intent in Smallwood et al. (2010) was to reveal which vertebrate species contribute to scavenger removal, and to show how some of these species can remove carcasses so quickly that measuring removal rates by daily or multi-day status checks likely generate biased persistence curves (the same would hold for mean days to removal). Measuring removals in hours and minutes since placement, and perhaps because we placed only 1–2 carcasses per week, we are able to show that the work of Huso (2010) and Erickson et al. (2000) assumed exponential daily probability of persistence was likely correct, but only during the first 8 days since the placements of fresh carcasses (Fig. 7). In fact, we found summertime removal rates were equal between small and large bird carcasses during the first 8 days of the trials, before deviating sharply from the fitted curves (Fig. 7). Most of the carcass population undetected because of vertebrate scavengers is removed within the first 8 days since death, and most of the rest will persist for multiple detection opportunities, the number of which will depend on the average fatality search interval, and on exposure to wind and rain. One solution is to restrict fatality search intervals to no less often than weekly.

Another solution is to combine the scavenger removal and searcher detection trials into 1 trial, and to integrate this trial into routine fatality monitoring, as we are doing now in 2 studies in the Altamont Pass Wind Resource Area. Performing separate trials to obtain separate adjustment terms for carcass persistence and searcher detection rates has proven to be complicated, yet all that is needed is a single adjustment for the proportion of fatalities that is not detected during periodic searches. A more realistic fatality rate estimator is the following:

$$F_{\mathrm{A}} = \frac{F_{\mathrm{U}}}{D}$$

where D is the proportion of placed carcasses that is detected by searchers performing periodic fatality searches throughout the duration of monitoring, and is based on carcasses placed at a rate intended to simulate carcass deposition (i.e., not as a windfall event). Using this approach, why carcasses were not detected does not matter, and carcasses missed in 1 search can still be detected in subsequent searches. Larger values for D could be obtained by shortening search intervals, as needed.

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Associate Editor: Bret Collier.