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FORUM

Reanalysis indicates little evidence of reduction in eagle mortality rate by automated curtailment of wind turbines

Manuela Huso[1,2] | **Daniel Dalthorp**[1,3]

[1]U.S. Geological Survey, Forest and Rangeland Ecosystem Science Center, Corvallis, OR, USA

[2]Statistics Department, Oregon State University, Corvallis, OR, USA

[3]PowerStats, Albany, OR, USA

Correspondence
Manuela Huso
Email: mhuso@usgs.gov

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Abstract

1. Unintended consequences of renewable energy development include collision-caused deaths of birds and bats. Energy companies may risk prosecution if protected species are among the casualties. Shutting down turbines during high collision-risk conditions could reduce mortality rates, and several companies are developing systems to identify such conditions.
2. A recent peer-reviewed article published in the Journal of Applied Ecology reported a remarkable '82% (75%–89%) reduction in the fatality rate' of eagles at a wind energy facility due to a device marketed as Identiflight®—remarkable because of the impressive effect size and the extremely high level of precision. We show that reported results stem from four major errors, which, when corrected, give an unremarkable estimate of 50% (−159%, 89%) reduction (or possible increase) in the fatality rate.
3. The errors include the following: (i) *Ignoring annual variation*. They compare the average number of eagle fatalities over 4 years before activation of Identiflight® to the number in a single year after, ignoring annual variation in fatalities. (ii) *Unfounded causal inference*. Lack of replication (one treatment year at one site) is ignored, leading to unwarranted causal inference. (iii) *Inflated effect size*. Effect size is inflated by assuming (without providing evidence) that the difference in fatality relative to the mean at a neighbouring site would be exactly repeated at the treatment site. Furthermore, the observed difference in fatalities at the control site depends strongly on the arbitrarily chosen date distinguishing the 'Before' and 'After' periods, yielding unreliable results. (iv) *Inconsistency of data*. It is unclear why 7 of 42 reported eagle fatalities were not included in the data analyzed, potentially further inflating the estimated effect size.
4. *Synthesis and applications*. The recent claim, published in the Journal of Applied Ecology, that 'Eagle fatalities are reduced by automated curtailment of wind turbines' is not supported by the data but stems from errors that led to strongly overstated effect size and precision, and unfounded inference. In theory, automated curtailment has obvious potential for reducing eagle fatalities, but several more years of data at several locations and appropriate statistical analyses will be required to evaluate its effectiveness and to inform management prescriptions involving this technology.

KEYWORDS
automated curtailment, BACI, eagle mortality, Identiflight®, migratory bird treaty act, Top of the World, wind energy

1 | INTRODUCTION

In 2013, the U. S. Fish and Wildlife Service (FWS) finalized the Eagle Conservation Plan Guidance (ECPG), which provides guidance to wind developers on reducing their impacts to both bald eagles *Haliaeetus leucocephalus* and golden eagles *Aquila chrysaetos* at wind power generation facilities (U.S. Fish and Wildlife Service, 2013). That same year, the U. S. Department of Justice issued its first criminal conviction under the Migratory Bird Treaty Act for unlawful avian takings at wind projects (U.S. Department of Justice, 2013). The charges stemmed, in part, from the discovery of 14 golden eagles at two of Duke Energy's Wyoming wind projects, 'Campbell Hill' and 'Top of the World'. As part of the sentence, Duke Energy was required to implement a migratory bird compliance plan describing specific measures to avoid and minimize golden eagle and other avian wildlife mortalities at the company's four commercial wind projects in Wyoming (U.S. Department of Justice, 2013).

One approach to reducing collision rates of birds and bats is to shut down turbines when risk of collision is high (Watson et al., 2017). In a recent article, McClure et al. (2021) (hereafter McClure et al.) reported on a study to test the efficacy of a device marketed as Identiflight® that is designed to detect eagles approaching turbines and trigger curtailment of the nearby turbines when the eagles are considered to be at risk of collision. In their study, eagle mortality was monitored at two of Duke Energy's wind projects cited in the compliance plan: Top of the World and Campbell Hill. At the first site, referred to as the 'Treatment' site (T), eagle mortality was monitored at its 66 GE 1.5 MW turbines with 82.5 m rotor diameter (Wind-turbine Models, 2021b) and 44 Siemens 2.3 MW turbines with 110 m rotor diameter (Wind-turbine Models, 2021a) for ~4 years before (B) Identiflight® was installed, and then for about 6 months to 1 year afterwards (A). The range in monitoring time arises from the devices not all being activated to cover all turbines on a single date. Eagle mortality was concurrently monitored at the 66 GE 1.5 MW turbines with 82.5 m rotor diameter at Campbell Hill, referred to as the 'Control' site (C), where no mortality reduction device was installed.

In this paper, we discuss the major errors in McClure et al.'s analysis and reanalyse their published data. We show that after correctly accounting for variation, results do not support the study's bold, generalized inference that 'eagle fatalities are reduced by automated curtailment'. Justifying this statement would require demonstration that (1) the mortality rate (rather than simply the number of fatalities) changed at the treatment site, (2) the change was very likely due to the treatment and unlikely due to random chance, and (3) a similar reduction occurred at different sites and other times following implementation of Identiflight®. McClure et al. fail to address these basic requirements of experimental design and statistical inference.

For our analysis, we downloaded the data published by McClure et al. (2020). After some minor quality control (described in Appendix S1 in Supporting Information), we used functions in `GenEst` (Dalthorp, Madsen, et al., 2018; Dalthorp, Simonis, et al., 2018; Simonis et al., 2018) to estimate detection probabilities and mortality, and were able to recreate the same estimates of detection probability and mortality as McClure et al. We then reanalysed the data, correctly accounting for annual variation, to estimate the reduction in mortality *rate* of eagles, not simply whether the estimated numbers of fatalities were different.

2 | ESTIMATED REDUCTION IGNORES ANNUAL VARIATION

McClure et al. reported finding 32 eagle carcasses (M_{TB}) at the treatment (T) site during 475.5 turbine-years[1] of monitoring before (B) installation of Identiflight® and 3 eagle carcass (M_{TA}) during 118.7 turbine-years after (A) installation of Identiflight®. Multiplying M_{TB} by 0.25, the ratio of turbine-years after to turbine-years before (118.7/475.5), standardizes the mortality from the two periods to the same scale. As was reported, this suggests that an average of 8 fatalities per turbine-year before the treatment decreased by 62.5%, to 3 fatalities per turbine-year in the single year after treatment. Acknowledging possible error in estimating M_{TB} and M_{TA} due to imperfect carcass detection ($\hat{g}$ ≈ 0.97), a 95% confidence interval for this decrease was reported as 59%–66%. This very narrow confidence interval accounts solely for the small uncertainty due to imperfect detection. It does not account for random variation in fatalities from year to year.

Although McClure et al. reasonably assert that the *number* of fatalities (M_{TA}) was lower in the year after implementation of Identiflight® than the *average* number over the 4 years prior, they do not address whether that difference could be explained by random chance. To do this, one would need to consider the variance among the counts in the 4 years prior and ask whether the observed count in the single year after implementation of Identiflight® was outside the range of normal variation. A chance decrease in fatalities relative to the mean, in one year would not be a surprise unless, perhaps, it was of a magnitude never before seen. The number of fatalities inevitably varies by season, year, location, eagle population and behaviour, weather, and other variables (U.S. Fish and Wildlife Service, 2013), as well as random chance. One year the number may be up, and the next year, down. In overlooking the random variation in fatalities from year to year, the study fails to consider whether the apparent observed reduction in fatalities after implementation of the Identiflight® system could plausibly be explained by random chance. Indeed, the data from the treatment site suggest that the year-to-year variation in the number of fatalities during the study period swamps the apparent treatment effect, and the number of fatalities in the single year after the implementation of Identiflight® appears to be wholly in line with the fatality numbers in previous years (Figure 1).

McClure et al. used `GenEst` (Dalthorp, Simonis, et al., 2018) to accurately estimate confidence intervals around the number of fatalities Before (M_B) and After (M_A), correctly accounting for the uncertainty in the count. The confidence limits are quite narrow because the detection rate was quite high ($\hat{g} \approx 0.97$). They did not estimate the number of fatalities in each year prior ($M_{B1}, M_{B2}, M_{B3}, M_{B4}$) that would represent the annual variation that could occur at this site against which to compare the number After (M_A). They only estimated the total number of fatalities Before and the total After. Dividing these estimates by the number of turbine-days represented in each period may put the two numbers on a common scale but does not provide the important measure of annual variance in the numbers that is necessary to evaluate a change in annual mortality rate. Consequently, there is little uncertainty in the estimated number of fatalities Before or After and therefore little uncertainty in their ratio (standardized or not). The failure to account for annual variation is perhaps more striking if one considers what their estimate would be had their detection probability been 100% rather than the ~97% they reported. Using this methodology, they would have reported a reduction in mortality rate of exactly 62.5%, with no uncertainty associated with it. Clearly this is wrong.

3 | UNFOUNDED CAUSAL INFERENCE

After ignoring interannual variation in the number of fatalities, McClure et al. erroneously infer that the smaller number of fatalities in the year after implementation of Identiflight® reflects a significant change in the underlying fatality *rate*. They then take the inference a step further, attributing the cause of this presumed reduction to the treatment itself—another unsubstantiated assertion. Experimental design allows the careful researcher to infer causation from an observed correlation through replication and randomization, but that cannot be done with only $n = 1$ post-treatment year at a single site.

McClure et al. have conducted a Before-After-Impact-Control (BACI) study without replication, that is, they have only four measures, one for each of the 4 BACI components. This design is known to be confounded and any differences that might be observed

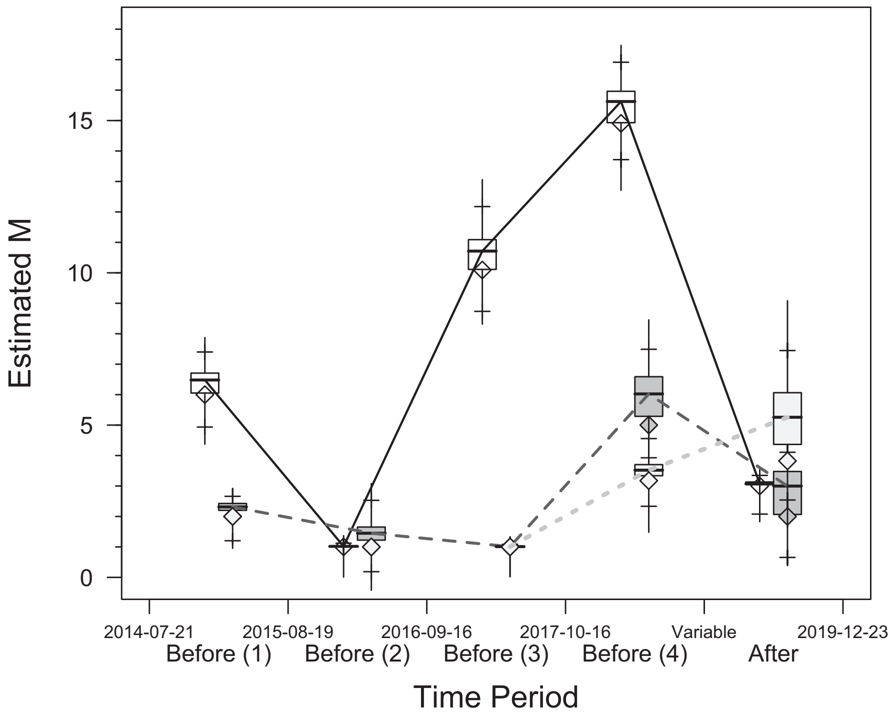

FIGURE 1 Boxplots of the estimated numbers of fatalities in five consecutive, equal-length periods of 1.1 years of operation of 110 turbines at the treatment site (open boxes) and 66 turbines at the control site (grey boxes), before and after implementation of the Identiflight® system. Dates defining the transition between periods at the treatment site are on the *x*-axis. Dates defining these periods for the control site are close to but not exactly as defined by the treatment site because of different start and stop dates of searches. The transition date between the fourth 'before' period (before [4]) and 'after' was variable among the treatment turbines, but arbitrarily defined by McClure et al. (2021) as 1 August 2018 for control turbines. Boxes depict the interquartile range and median. Whiskers show 99% confidence limits, and tick marks on whiskers show 95% confidence limits. Diamonds signify observed carcass counts. Dark grey boxes represent the control site with equal-length periods of 1.1 years of operation of 66 turbines. Light grey boxes represent estimates from periods whose transition point was defined by McClure et al. as 1 August 2018. All estimates were generated in `GenEst` using detection probabilities as calculated by McClure et al.

cannot be logically attributed to having been caused by the impact (Hurlbert, 1984). Although McClure et al. have temporal replication Before the Identiflight® intervention (which they fail to incorporate it in their analysis), there is no temporal replication After intervention and no spatial replication (one Control site and one Impact [Treatment] site). Even if the study were extended several more years to include temporal replication, comparison of a single Impact to a single Control location will still be confounded by the potential for any observed differences to have been caused by something other than the treatment. Underwood (1994) describes the issues succinctly:

> There are many practical problems of detection of human influences on abundances of populations, but two are paramount in designing sampling programs. First is the large temporal variance of many populations, so that their abundances are very "noisy." Second, many populations show a marked lack of concordance in their temporal trajectories from one place to another. This results in considerable statistical interaction between changes in mean abundance from time to time and differences from place to place. Sampling must therefore be sufficient to identify unusual patterns of change in a very interactive and very variable measurement.

Clearly, the abundance of eagle fatalities at the two sites is quite variable in time and there appears to be little concordance in their temporal variation (Figure 1).

3.1 | Reanalysis

A formal statistical analysis supports the visual impression that the fatality rate after implementation of Identiflight® was not significantly different from rates before treatment. To test, we split the Before segment at the Treatment site into four periods, each with virtually identical number of turbine-days as the After period, tallied the fatality counts in each period, and performed negative binomial regression[2] on the counts in R using glm.nb in the MASS package (Venables & Ripley, 2002). The treatment effect was $exp(-0.98) = 0.375$ (Table 1) or an estimated 62.5% decrease in fatality rate in the After period, in close agreement with McClure et al. However, its p-value was only 0.27 and the 95% confidence interval on the estimated treatment effect was [0.065, 2.158]. In other words, after implementation of Identiflight®, the change in fatality *rate* (not count) could be anywhere between a 93.5% decrease and a 115.8% increase, giving no evidence that the treatment was clearly associated with a reduction in fatality rate.

However, McClure et al. go beyond claiming an association between the treatment and the fatality rate—a claim not supported by the data—and assert that the treatment *caused* a reduction in fatality rate. With adequate experimental design, researchers can control for random variation and unknown factors to be able to justly

TABLE 1 Estimated parameters from the negative binomial model fit on $\log_e$ scale. 'Intercept' represents average annual fatality rate before, 'treatment' represents change in fatality rate after relative to before

Parameter	Estimate	Std. error	*z*-value	Pr(>|z|)
Intercept	2.079	0.343	6.056	0.000
Treatment	−0.980	0.892	−1.098	0.272

attribute observed effects to the treatment. The key is replication. Unfortunately, this study is limited to a single site and a single time. An experiment with $n = 1$ year cannot establish a correlation as a cause, especially when it fails even to establish a correlation.

4 | INFLATED EFFECT SIZE

To quantify the magnitude of the effect, McClure et al. compare the numbers of fatalities at the treatment site to a smaller, control site (Campbell Hill), where no Identiflight® devices were installed. The four fatalities found at this site in the After period was 2 more than (or twice as many as) the annual average of 2 over the 4 years prior. McClure et al. interpret this unremarkable fluctuation in annual fatality numbers as the sole basis for an expectation that had it not been for the Identiflight® system, the number of fatalities at the treatment site in the After period would also have been twice the average, that is, 16 rather than the average of 8 per year. They conclude that the observed 63% reduction in the number of fatalities would really have been a roughly 100% increase if Identiflight® had not been used, so the actual reduction in fatality rate was 82% (or 1 − [1−0.63]/2). They provide no explanation for why the number of fatalities would fluctuate in perfect tandem at the two sites, no evidence that they do, and no rationale for why this highly variable estimator of a rate of change at one site would be appropriate to use as a fixed and known, expected response at another site.

5 | DECISIONS REGARDING DEFINITION OF BEFORE–AFTER PERIOD IN CONTROL

Furthermore, the observed change in mortality at the control site depends strongly on the date chosen to distinguish the Before and After periods, a date which is arbitrarily defined in the study and yields unreliable results. Identiflight® units were not all activated on a single day, but at intervals over an entire year, making it difficult to pinpoint an exact date for distinguishing Before from After at the Control site. McClure et al. decided: 'For turbines at the control site, we specified all months before August 2018 as the 'before' time period. We specified the 'after' time period at the control site as post-August, 2018 because that is the month when curtailment of the first turbines began being controlled by automated curtailment units at the treatment site' (depicted as light grey boxes in Figure 1) At this time, however, < 50 % of turbines were controlled by Identiflight®.

TABLE 2 Carcasses reported by McClure et al. (left 4 columns) matched with those reported in the ECP (table 8, Duke Energy Renewables, 2021; right 4 columns). CO = carcass identification number, BA = before or after as assigned by McClure et al., date found[M] = date assigned by McClure et al. for when the carcass was found, Turb = turbine number, model = turbine model (derived from McClure et al.'s turbine number), date found[ECP] = date reported in the ECP for when the carcass was found, Species = golden eagle (GOEA) or bald eagle (BAEA), fatality/injury = indicator of whether the eagle was found dead (fatality) or injured (injured), search/incidental = indicator of whether the eagle was found on a scheduled search within plot boundaries (search) or not (incidental), assigned by Duke Energy. Dates from both datasets do not necessarily coincide as McClure et al. assigned 'date Found' for eagles found incidentally to the date of the next scheduled search. Blank entries in McClure data indicate eagles that were reported in the ECP but were not included in the analysis of McClure et al.

McClure					ECP			
CO	BA	Date Found[M]	Turbine	Model	Date Found[ECP]	Species	Fatality/injury	Search/incidental
X1	B	9/25/2014	T40	GE1.5	9/25/2014	GOEA	Fatality	Search
X2	B	12/4/2014	T95	Siem2.3	12/5/2014	GOEA	Fatality	Search
					11/21/2014	GOEA	Fatality	Incidental
					2/9/2015	GOEA	Fatality	Incidental
X3	B	3/11/2015	T90	Siem2.3	2/21/2015	GOEA	Fatality	Incidental
X4	B	3/26/2015	T43	GE1.5	3/26/2015	GOEA	Fatality	Search
X5	B	5/21/2015	T15	GE1.5	5/8/2015	GOEA	Fatality	Incidental
X6	B	8/11/2015	T52	GE1.5	8/11/2015	GOEA	Fatality	Search
X7	B	5/20/2016	T64	GE1.5	5/17/2016	BAEA	Fatality	Incidental
X8	B	12/7/2016	T30	GE1.5	12/1/2016	GOEA	Fatality	Incidental
X9	B	1/25/2017	T82	Siem2.3	1/21/2017	GOEA	Fatality	Incidental
X10	B	2/27/2017	T83	Siem2.3	2/6/2017	GOEA	Fatality	Incidental
					2/13/2017	BAEA	Fatality	Incidental
					4/14/2017	GOEA	Fatality	Search
X11	B	4/17/2017	T51	GE1.5	4/17/2017	GOEA	Fatality	Search
X12	B	4/17/2017	T53	GE1.5	4/17/2017	GOEA	Fatality	Search
					4/20/2017	GOEA	Fatality	Search
X13	B	5/23/2017	T67	Siem2.3	4/23/2017	GOEA	Fatality	Incidental
X14	B	4/26/2017	T101	Siem2.3	4/26/2017	BAEA	Fatality	Search
X15	B	5/22/2017	T57	GE1.5	5/22/2017	GOEA	Fatality	Search
X16	B	8/17/2017	T74	Siem2.3	8/17/2017	GOEA	Fatality	Search
X17	B	9/22/2017	T21	GE1.5	9/10/2017	GOEA	Fatality	Incidental
X18	B	11/8/2017	T25	GE1.5	10/24/2017	GOEA	Fatality	Incidental
X19	B	12/18/2017	T70	Siem2.3	11/30/2017	GOEA	Fatality	Incidental
X20	B	12/13/2017	T45	GE1.5	12/1/2017	GOEA	Fatality	Incidental
X21	B	2/15/2018	T42	GE1.5	2/6/2018	GOEA	Fatality	Incidental
X22	B	2/16/2018	T51	GE1.5	2/11/2018	GOEA	Fatality	Incidental
X23	B	3/13/2018	T47	GE1.5	3/13/2018	GOEA	Fatality	Search
X24	B	4/2/2018	T79	Siem2.3	4/2/2018	GOEA	Fatality	Search
X25	B	4/20/2018	T45	GE1.5	4/4/2018	GOEA	Fatality	Incidental
X26	B	4/20/2018	T53	GE1.5	4/15/2018	GOEA1	Injured	Incidental
X27	B	4/25/2018	T78	Siem2.3	4/16/2018	GOEA	Fatality	Incidental
X29	B	4/19/2018	T44	GE1.5	4/19/2018	GOEA	Fatality	Incidental
X28	B	4/25/2018	T78	Siem2.3	4/19/2018	GOEA	Fatality	Incidental
X30	B	6/13/2018	T68	Siem2.3	5/28/2018	GOEA	Fatality	Incidental
X31	B	9/13/2018	T44	GE1.5	9/13/2018	GOEA	Fatality	Search
X32	B	2/18/2019	T45	GE1.5	2/18/2019	GOEA	Fatality	Search
X1	A	2/27/2019	T107	Siem2.3	2/26/2019	GOEA	Fatality	Incidental
X2	A	4/9/2019	T23	GE1.5	4/9/2019	BAEA	Fatality	Search

(Continues)

TABLE 2 (Continued)

McClure					ECP			
CO	BA	Date Found[M]	Turbine	Model	Date Found[ECP]	Species	Fatality/injury	Search/incidental
					5/14/2019	GOEA	Fatality	Search
					6/24/2019	GOEA	Fatality	Incidental
X3	A	9/10/2019	T38	GE1.5	9/3/2019	GOEA	Fatality	Incidental

There were several other choices of transition date possible, among them:

1. 23 October 2018: when 80% of the total turbine-days in the Control site had occurred, parallel to the percent of total turbine-days in the Before period at the Treatment site (depicted as dark grey boxes in Figure 1)
2. January 2019: when automated control by Identiflight® of ≥ 50% of turbines at the Treatment site was in place
3. August 2019: when automated control by Identiflight® of *all* of the turbines at the Treatment site was in place

The choice of date is highly influential in determining the magnitude of change at the Control site and hence the inflated effect size. In the first alternative, one eagle found on 09/26/2018 would shift from After to Before, and in the latter two, a second eagle found on 11/09/2018 would shift, halving and then completely erasing, respectively, any increase at the control site After relative to its average Before.

6 | INCONSISTENCY OF DATA

The reported 63% reduction in fatalities may itself be inflated. McClure et al. state that 'From 2014–2019 … 35 eagle carcasses were discovered at the treatment site, three of which were found during the after period'. In contrast, the Top of the World Eagle Conservation Plan (ECP; Table 8, pg 45, reproduced in Table 2; Duke Energy Renewables, 2021), lists 42 eagles reported as having been found during the study period. It is unclear why 7 of the 42 carcasses were not included in the analysis, apparently 2 of which were found 'After' treatment intervention.

In the ECP report, each carcass was coded as 'Search' ($n = 17$) or 'Incidental' ($n = 25$), with 'Search' defined as 'Eagle found during scheduled eagle mortality search within search plots' and 'Incidental' as 'Eagle was found outside of scheduled search or search plot' (Duke Energy Renewables, 2021). McClure et al. included both search ($n = 14$) and incidental ($n = 21$) carcasses in their analysis yet did not include three search and four incidental carcasses (2 After, 5 Before.) They included an injured eagle as well, perhaps because it subsequently died.

If the omitted carcasses are included in the analysis of the Treatment site,[3] the change in fatality *rate* would be estimated as a 45.9% decrease, rather than the 63% reported in the study, with 95% CI ranging from an 88.7% decrease to a 159.2% increase. There may be reasonable cause for not including so many carcasses from the analysis, but the omission of a substantial proportion (17%) of the publicly reported ECP data requires explanation.

7 | DISCUSSION

Generalized linear models, typically used to estimate differences in average rates of an occurrence between two groups, take into account the inherent variance in the observed counts. McClure et al. took into account only the uncertainty in the representation of the true count by the observed number of fatalities, but not the inherent variance in counts expected among sampling periods. The consequence was that although their estimated reduction in mortality due to Identiflight® was appropriate (assuming the exclusion of seven carcasses was justified), the uncertainty in the estimate was greatly understated. Reanalysis of the reported data resulted in an estimated change in mortality rate with far less certainty than McClure et al. reported. Furthermore, when all fatalities publicly reported from the site were included, the estimated effect of Identiflight® ranged from a ~90% decrease to a >150% increase in mortality. This reflects the intuitive perception that data from only a single year post-implementation of an intervention at a single site cannot support strong inference regarding its effectiveness.

8 | MANAGEMENT IMPLICATIONS

If it were discovered that many of the carcasses in the McClure et al.'s study were not eagles but had been misidentified by crew members as such, the results of their study would certainly be called into question and the data reanalysed using only those observations unequivocally confirmed to be eagles. An error in statistical analysis is of no less importance and would demand a reanalysis using appropriate models and input data, as we have done here.

This reanalysis shows that the data analysed by McClure et al. provide little evidence that implementation of the Identiflight® system effected any change in underlying mortality rate at this study site. Data from several more years and additional sites, analysed using an appropriate statistical model, will be needed before managers can make informed decisions regarding its utility in reducing impacts to eagle populations at wind power facilities.

AUTHORS' CONTRIBUTIONS

M.H. and D.D. were involved in conceptualization, formal analysis and writing the manuscript.

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CONFLICT OF INTEREST STATEMENT

Manuela Huso and Dan Dalthorp were both employed by the U.S. Geological Survey, in the same science centre as Todd Katzner, co-author of the McClure et al. (2021) paper and Manuela Huso has co-authored several papers with Todd Katzner. Nonetheless, the authors declare that these relationships have not influenced their fair assessment of the scientific validity of the work they are critiquing.

DATA AVAILABILITY STATEMENT

Data available via the Dryad Digital Repository https://doi.org/10.5061/dryad.pnvx0k6kx (McClure et al., 2020). Minor edits made to the data for this analysis are described in Appendix S1 in Supporting Information.

ORCID

Manuela Huso https://orcid.org/0000-0003-4687-6625
Daniel Dalthorp https://orcid.org/0000-0002-4815-6309

ENDNOTES

[1] Calculated as described by McClure et al. as the sum 'total number of turbine-days during each period across all turbines at a given site [divided] by 365'.

[2] The weights in the regression are proportional to the length of the period times the reciprocal of the detection probabilities. The number of turbine-years in the Before periods are 118.74, 118.74, 119.04, 118.98, respectively, and 118.7 turbine-years After. After normalizing the weights for the After period to be 1, the Before weights would be 0.999, 0.999, 0.997 and 0.997. Because the detection probabalities appear to have been constant at the Treatment site throughout time (table 3 in McClure et al., 2021), they do not affect the relative weights and can safely be ignored in the regression. The R code was: nbmod <- MASS::glm.nb(x~treatment, data = data. frame(x = c (6, 1, 10, 15, 3), treatment = c(0, 0, 0, 0, 1)), link = log, weight = c (0.999, 0.999, 0.997, 0.997, 1.000)).

[3] nbmod2 <- MASS::glm.nb(x~treatment, data = data. frame(x = c(8, 1, 13, 15, 5), treatment = c(0, 0, 0, 0, 1)), link = log, weight = c(0.999, 0.999, 0.997, 0.997, 1.000)).

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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of the article at the publisher's website.

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