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THE UTILITY OF CENSUS OR SURVEY FOR MONITORING WHOOPING CRANES IN WINTER

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Abstract: We discuss recent changes in the monitoring program for endangered whooping cranes (*Grus americana*) on their winter habitat in Texas. A 61-year annual census was replaced in the winter of 2011-2012 with a distance sampling procedure. Justification for the change was, in part, based on criticism of the previous methods of counting cranes and the assessment of crane mortality on the wintering grounds. We argue here that the arguments, methods, and analyses employed to discount the census procedure and mortality estimates were applied incorrectly or with flawed logic and assertions. We provide analysis and logical arguments to show that the census and mortality counts were scientifically valid estimates. The distance sampling protocol currently employed does not provide the accuracy needed to show small annual changes in population size, nor does it provide any estimate of winter mortality. Implications of the relative merit of census and mortality counts versus distance sampling surveys are discussed in the context of management of the whooping crane.

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Key words: causation, census, distance sampling, endangered species, *Grus americana*, population count, whooping crane.

The only naturally remaining endangered whooping crane (*Grus americana*) population has been monitored since 1938 on its sole winter habitat in and around the Aransas National Wildlife Refuge (hereafter Aransas) in Texas (CWS and USFWS 2007). The population, with a low of just 15-16 birds in 1941, has increased to an estimated 338 birds in 2015 (Butler and Harrel 2016). For 61 years (1950 to 2010) census flights were conducted annually by refuge biologists in order to track changes in abundance and guide progress toward recovery. The census was designed to count, as completely as possible, the total population of wintering cranes, estimate winter mortality, and document habitat use (Stehn and Taylor 2008). Beginning in the winter of 2011-2012, U. S. Fish and Wildlife Service (USFWS) personnel discarded the census protocol in favor of a distance sampling (Buckland et al. 1993) procedure as per the general recommendations for wildlife estimates of the National Wildlife Refuge System inventory and monitoring initiative. In doing so, they abandoned the notion of a population count and opted instead for a statistical estimate of crane abundance with confidence intervals. We are unaware of any criticism of the former census methods until the inventory and monitoring initiative was implemented.

The justification to shift whooping crane population monitoring to a sampling protocol from the census method centered around 3 perceived problems (Strobel et al. 2012; Butler et al. 2013, 2014*a*, 2014*b*). First, the census was stated to lack validity because it was possible to both miss individuals and double-count individuals (i.e., the census was not a complete count or enumeration). Second, the census method was perceived to be biased in the manner in which the aerial count was conducted. Third, estimates of mortality during the wintering period were claimed to be inaccurate and biased. All of these perceived shortcomings were alleged to invalidate the census method. We address each of these criticisms here. We present data analysis that demonstrates that the population census closely matched another key indicator of abundance, the nest count on the summer breeding grounds in Canada. In addition, we present methodological arguments and new analyses that refute the assertions that census and mortality estimates were invalid measures. Finally, we address the logic and validity of the criticisms leveled at the census method and the analyses that were used to make them.

METHODS

We compared the census results (population size) from 1966 to 2010 (n = 45) to nesting pair counts obtained by the Canadian Wildlife Service (CWS) on the breeding grounds in Wood Buffalo National Park, Canada, the following summer. Census methods are described in detail in Stehn and Taylor (2008). Nest count data from 1966 to 2005 can be found in the

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Canadian Wildlife Service and U.S. Fish and Wildlife Service international recovery plan for the whooping crane (2007), and from 2006 to 2010 in annual reports of the Canadian Wildlife Service, Prairie and Northern Wildlife Research Centre, Saskatoon, Canada.

Using linear regression, we estimated the relationship between the number of wintering adults and total nest count. We calculated a Durbin-Watson *D* statistic (Neter et al. 1985) to assess autocorrelation among regression residuals.

Mortality data and methods are described in detail in Stehn and Haralson-Strobel (2014). Mortality was assigned when 1 individual of a known group, usually a mated pair or pair with offspring, was determined missing after follow-up attempts failed to locate it. We compared total winter mortality estimates (hereafter referred to as Mortality) of whooping cranes from the winters of 1958-59 to 2010-11 to the number of census flights flown over the crane wintering period (n = 53). We did not include data from 1951 through 1957 because the total number of flights and whether data on mortality were obtained by aerial census or by ground search could not be determined from historical records of that period. Mortalities discovered by means other than aerial flights (e.g., ground reports) were excluded. Flights were conducted from October through April and often into May each wintering season. Number of flights computed using all flight data are referred to as Total Flights (TF). Occasional waterfowl survey flights with whooping crane observations made incidentally during the flight were excluded. Additional partitions of the TF data were analyzed: (1) TF-ZC – Total Flights minus flights where zero cranes (ZC) were found, (2) TF-NC – Total Flights minus flights in which there was no chance of detecting mortality (e.g., only a few subadult cranes and no family groups were present at Aransas), (3) TF-DM – Total Flights within the period from December through March (DM), and (4) FWS-DM – Total DM Flights published by Butler et al. (2014*a*). The number of December through March flights published by the USFWS differs slightly from our count. These additional partitions allowed us to exhaustively search for a relationship between the number of flights and Mortality with subsets of the TF data that could have been superior to the complete data set, and in the case of FWS-DM, to mimic the data set used by Butler et al. (2014*a*).

Using linear regression, we estimated the relationship between Mortality and the number of flights per winter and subsets of that variable described above. We also computed a ratio of total winter mortality by population size (MRatio) and performed the same set of regressions on this variable. In some analyses, we partitioned the data to analyze the periods 1982-83 to 2010-11 (n = 29) when data were collected by the same observer. Multiple regression and correlation analyses were performed on mortality, flight number variables, year, and population size in order to examine potential confounding variables that might affect the interpretation of Butler et al. (2014*a*) that the number of flights biased the mortality estimate. Data were analyzed with SAS (2008) PROC REG, and PROC CORR.

RESULTS

The number of nesting pairs located in the cranes' Canadian breeding grounds was highly associated with the previous winters' population census in Texas (r^2 = 0.94; $F_{1,44}$ = 674.69, $P \leq 0.001$) (Figure 1). A Durbin-Watson *D* statistic value of 2.03 indicated that there was no first-order auto-correlation among residuals of the regression analysis.

All linear regressions between mortality and the number of flights for the wintering periods 1958-1959 to 2010-2011 were significant (TF r^2 = 0.12, $F_{1,52}$ = 6.98, P = 0.011; TF-ZC r^2 = 0.12, $F_{1,52}$ = 6.70, P = 0.013; TF-NC r^2 = 0.11, $F_{1,52}$ = 6.31, P = 0.015; DM r^2 = 0.11,

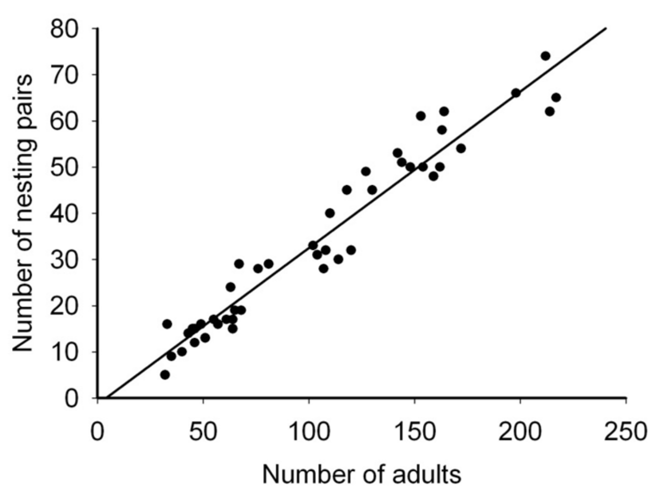

Figure 1. Plot and regression line of the yearly winter census of whooping cranes at Aransas National Wildlife Refuge, Texas, versus nesting pair counts in Wood Buffalo National Park, Canada, during the summer following the census, 1966-2010.

$F_{1,52}$ = 6.64, P = 0.013; FWS-DM r^2 = 0.08, $F_{1,52}$ = 4.60, P = 0.037). The number of mortalities declined with increasing number of flights. There were significant intercorrelations among the mortality estimate, number of flights, population size, and year (Table 1). Mortality decreased with number of flights, and increased with population size and year of study. The number of flights was inversely related to the population size and year of study, with correlation coefficients approximately twice the size of that of mortality with other variables. As the years of the study progressed and the population size increased, the number of flights declined. Substitution of the other Flight Number variables in Table 1 resulted in no material differences in the magnitudes or directions of correlations reported there.

Linear regression analysis of the ratio of mortalities to population size with the number of flights was not significant (MRatio r^2 = 0.0, $F_{1,52}$ = 0.09, P = 0.762). Similar non-significant results were obtained on all measures of number of flights.

Multiple regression of the dependent variable Mortality with independent variables Total Flights (TF), Population Size, and Year was significant (Mortality r^2 = 0.20, $F_{3,52}$ = 3.97, P = 0.013). The t-values and significance levels for independent variables were: TF t = −0.28, P = 0.782; Population Size t = 1.79, P = 0.080; Year t = −1.25, P = 0.216.

All linear regressions between Mortality and measures of the number of flights for the wintering periods 1982-83 to 2010-11 were non-significant (TF r^2 = 0.08, $F_{1,28}$ = 2.43, P = 0.130; TF-ZC r^2 = 0.07, $F_{1,28}$ = 2.29, P = 0.142; TF-NC r^2 = 0.06, $F_{1,28}$ = 1.91, P = 0.179; TF-DM r^2 = 0.11, $F_{1,28}$ = 3.03, P = 0.093; FWS-DM r^2 = 0.08, $F_{1,28}$ = 2.32, P = 0.139). Total Flights decreased with population size (Pearson r = −0.74, $P \leq 0.001$) and with year (Pearson r = −0.68, $P \leq 0.001$).

Linear regression analysis, for the periods 1982-83 to 2010-11, of Total Flights with the ratio of mortalities to population size was not significant (MRatio r^2 = 0.05, $F_{1,28}$ = 1.32, P = 0.260). Similar non-significant results were obtained on all measures of the number of flights.

Table 1. Pearson correlations (at P < significance level) among relevant variables in the yearly census (n = 53) of whooping cranes, Aransas National Wildlife Refuge, Texas, winters 1958-59 to 2010-11.

	Mortality	Flights	Population size
Mortality			
Flights	−0.35 (0.01)		
Population size	0.41 (0.01)	−0.74 (0.001)	
Year	0.35 (0.01)	−0.68 (0.001)	0.96 (0.001)

DISCUSSION

Population Census versus Distance Sampling

Claims have been made that those conducting whooping crane censuses prior to and including winter 2010-11 assumed that they were doing a complete census (Strobel et al. 2012, Strobel and Butler 2014) and that these results are, therefore, not scientifically valid. However, Stehn and Taylor (2008) explicitly detailed the potential sources of error that may have influenced the population count's accuracy and, to our knowledge, no claim of a complete enumeration was ever made in any publication or official documentation of the whooping crane censuses. The USFWS has taken a strict definition of a census to be a complete count of all individuals in the population, as have other authors (Conroy and Carroll 2009). As such, they cite potential for errors in the census as reason to discount the method as flawed. However, censuses that are not complete enumerations are routinely performed to monitor animal abundance (e.g., Pugesek et al. 1995, Bibby et al. 2000, Ross and Reeve 2003), including for some species that are far more elusive, secretive, and difficult to observe in the wild than are wintering whooping cranes (e.g., Guschanski et al. 2009). Unfortunately, the USFWS chose to discount the validity of the population census by claiming that the most extreme definition of a population census is the only valid one.

In arguing that the distance sampling method is superior to the census method (Strobel et al. 2012; Butler et al. 2013, 2014*b*; Strobel and Butler 2014), the USFWS failed to recognize that a population census and a sample are 2 distinctly different methods with different data requirements (Gregory et al. 2004). A population census does not require unbiased sampling procedures to "estimate" the population because it is not a statistical sample and therefore does not require for its validity a rigorous set of procedures that are precisely repeated (Ross and Reeve 2003). Instead, the population census "counts" used a systematic and thorough aerial coverage of the wintering area to locate nearly all birds in the area with remarkably consistent search effort, area covered, and results from week to week.

Perhaps it would do well at this point to demystify the situation for the reader. We are considering here a search of a specified area of low-lying vegetation for a conspicuously colored white, red, and black bird standing upwards of 1.5 m tall. The animals are readily distinguished from their habitat, thus making them quite amenable to a census count procedure. There is simply no place for a whooping crane to "hide" from the census aircraft unless it leaves the census area, and whooping cranes rarely leave the census area (Stehn 1992). With a species this easily detected at long distances from the observer, we question the necessity of distance sampling. The USFWS provided no scientific evidence that the population census method was inaccurate and relied instead on a specious argument. The USFWS claimed as their proof that the census method was flawed is that they obtained a poor detectability of 0.558 in their attempts to analyze census data using distance sampling techniques (Strobel and Butler 2014). They then concluded that a census could not possibly be accurate with detectability so low that nearly half the birds were not seen during aerial flights. The low measure of detectability derived by Strobel and Butler (2014) is illogical. In fact, on the 4 census flights used by Strobel and Butler (2014) to calculate detectability, the census methodology reported finding 92.4% and 100% of the cranes estimated present on 2 of those flights (Stehn 2011). The other 2 flights occurred in early December with the migration still ongoing, so no comparison was made between the number of cranes seen and number estimated present. USFWS erred by attempting to derive detectability from census flights when detectability is clearly a measure derived from surveys. There is no reasonable way that the data from the census procedure could be analyzed or the procedure duplicated so that distance sampling estimates of detectability could be calculated. There are simply too many differences between survey and census methods.

It is important to note the differences in the way census flights were conducted versus survey flights utilized for distance sampling estimates. Chief among these differences was that in the census flights, at least twice as many transects were flown in the same area than on survey flights. On census flights, the single observer did not attempt to look into the sun to count birds, and transects were sometimes flown at an angle to the coast to improve the sun angle. In contrast, the survey flights used 2 observers looking out opposite sides of the aircraft, and although they attempted to count during mid-day as much as possible, given the winter sun and the time required to complete the survey, 1 of the 2 observers was undoubtedly hampered by sun glare the majority of the time. Survey results showed detectability, when compared to looking toward the sun, was 2.7 times greater when the sun was overhead, and 3.9 times greater when the sun was at the observer's back (Strobel and Butler 2014), demonstrating a significant advantage for the census methodology where the single observer always looked away from the sun. On census flights, the single observer would look down sun a distance of at least 1,000 meters. In full sunshine, cranes could be detected at a distance of over 1,600 meters (Stehn and Taylor 2008). Transects were usually a maximum of 500 meters apart, narrow enough to enable the observer to detect the same cranes on 2 adjacent transects, an essential practice needed to counter most of the ways to overlook cranes described by Stehn and Taylor (2008). Thus, each area of marsh was viewed at least twice. If there was uncertainty as to what was observed, the census pilot was directed to fly toward sightings and to circle them to verify group size and composition, and to sometimes make simulated landings close to the cranes to observe color bands to identify individual cranes. Also, whooping cranes seen in flight were followed to record the location to which they moved. In contrast, survey flights with transects spaced 1,000 meters apart only examined each area of marsh 1 time, with half of that area seen with the observer looking toward the sun or with the sun only partly overhead. In contrast to census flights, survey flights would not deviate from those lines to check on the identity of birds or determine, in cases of uncertainty, whether cranes were adult or juveniles.

These differences allowed the census flight to achieve, on average, a recount of 95.3% of the estimated number of whooping cranes present on subsequent census flights (Stehn and Taylor 2008), an indicator of detectability of 0.953 and not the 0.558 postulated by Strobel and Butler (2014). The 95.3% recount then provides an estimate of reliability of the census count that would be unattainable were detectability in the census counts actually 0.558.

With repeated census flights of the known wintering area, Stehn and Taylor (2008) concluded that 99% of the population was routinely identified at the wintering grounds. A few additional birds were added to the population total if they were still in migration or were wintering far outside the area flown and there was no reasonable chance they had been present in the area covered during the count. These birds were typically reported by the public and intensively monitored. In food shortage winters with the cranes moving more and spending considerable time on upland areas usually adjacent to their territories (Chavez-Ramirez and Slack 1999), and in winters where the number of census flights was below approximately 8, census accuracy was believed to drop several percentage points. However, by piecing information together from multiple flights on the location of territories and the makeup of the population (number of adults, subadults, and juveniles), we believe that the population estimate was 95-99% accurate.

The close correspondence of the census counts of the number of adults to the number of nesting pairs observed in the subsequent breeding season indicates that the counts were consistent and accurate and that winter territories as described by Bonds (2000) were delineated correctly. The non-significant autocorrelation among residuals indicates that error rates of population estimates were consistent irrespective of population size. In addition, mathematical analysis of changes in population size fit closely with expected values of a small population (Miller and Botkin 1974, Boyce and Miller 1985, Boyce 1987, Link et al. 2003). Only in 2 winters have mathematicians suggested inaccuracies in the counts, both in the 1940s before regular census flights were done.

There are some serious drawbacks to implementing the distance sampling procedure. Our experience conducting census flights tells us that it is necessary to look at all areas at least twice to minimize observer error, and to sometimes circle groups of cranes to detect birds directly under the plane. Using a high-wing aircraft such as a Cessna 172 or 210 creates a blind spot directly in front of the aircraft from the high instrument panel and aircraft engine. Once GPS flight tracking was implemented, experience showed that the most frequent reason for overlooking cranes on a census was that the aircraft flew directly over them. As a consequence, we do not believe that an important assumption of distance sampling has been met, namely, the assumption that 100% of individuals are counted at 0 distance from the transect line (Buckland et al. 1993). Our experience also tells us that the shortened 2-week time frame utilized for distance sampling flights, relative to previous census flights conducted throughout the winter, will likely result in missing late arrivals to the wintering grounds and other dynamics associated with estimating population size that we discuss here, nor will it monitor habitat use throughout the winter.

The census method had a system of detecting cranes outside the typical area flown. As sightings of cranes in unusual areas were reported by the public, the area covered on the census was expanded to include those areas. Nearby areas of unoccupied crane habitat were also occasionally flown to see if the known crane range had expanded. The distance sampling method has a more formal method of covering areas where crane use only occurs occasionally, but may spend substantial flight hours finding very few, if any, cranes. It also does not have the flexibility to respond to cranes being found in any unusual area for a relatively short period of time. For example, the survey protocol (Butler et al. 2014*b*) ignores cranes that may utilize farm fields between the Blackjack Peninsula and Austwell, an area used in multiple years, especially at the end of the fall migration.

Distance sampling does not delineate winter territories or record which pairs are bringing young to Aransas. This limits the ability to estimate an effective population size for whooping cranes to maintain genetic viability over the long-term, information needed to set de-listing criteria for species recovery (CWS and USFWS 2007). We maintain that the survey, as designed, has low utility. The survey protocol goal is to detect a change of 10-15% annual population decline over a 3- to 4- year period (Butler et al. 2014*b*). Conducting semi-annual sampling flights as they suggest (Butler et al. 2013) only exacerbates the situation. The detectability of cranes on USFWS survey flights is so low that 95% confidence intervals of estimates (i.e., ± 39 cranes out of an estimated population size of 329 in the 2015-16 winter) are too wide to be useful as a management tool. The crane population could be declining and the responsible managing agencies would be unaware in the short term of any threats. The Whooping Crane Recovery Team has suggested that the error rate of the abundance survey must be reduced to detect changes of 5% (The Aransas Project vs. B. Shaw et al., memorandum opinion and verdict of the court, 2013). The stated goal of the USFWS for the distance sampling is to be able to detect a 10% change in the population (Sikes 2013). However, in only 8 of the 30 winters between 1980-81 to 2010-11 has the change in population size been greater than 10%. Thus, using the current survey protocol, USFWS will, in a majority of the winters, not be able to detect and document with confidence if population size increased or decreased from the previous winter.

Although the distance sampling survey method is designed for less experienced personnel without knowledge of existing crane territories, it still requires experience identifying whooping cranes from the air (Butler et al. 2014*b*) as well as learning the technology used to record crane presence and location. Having a survey that can be used by less experienced personnel makes it workable, but does not make it better than a census done by an experienced observer. Although the survey requires fewer flights of shorter duration than doing periodic census flights, the survey is conducted during a 2-week period, usually in December, leaving biologists without any monitoring of crane habitat for much of the winter and spring. Crane arrivals of family groups have been documented to occur as late as 20 December (T. Stehn, unpublished data), therefore, some cranes could have arrived after the survey flights were completed in 2011-2016. USFWS contends that the increasing number of cranes and expanding winter range make it necessary to simply sample the population. However, we contend that 1 aircraft working over 2 days, or 2 aircraft working simultaneously could census a population of 600 or more whooping cranes.

Crane Mortality Estimate

Another critical drawback of the new whooping crane survey protocol is that it makes no estimate of winter mortality. We do not know of any mortality estimates made since the survey was initiated, despite Recovery Action 1.1.3 in the Recovery Plan stating the need to determine mortality (CWS and USFWS 2007). Whooping crane carcasses are found only incidentally at Aransas and are few in number compared to mortality estimates based on census methodology. Without these data the USFWS will not be able to relate changes in population size to environmental conditions, such as drought on the nesting grounds or reduced river inflows at Aransas. For example, without the critically important mortality estimates obtained on census flights, the connection between reduced inflows and increased whooping crane mortality would never have been proven in federal court (The Aransas Project vs. B. Shaw et al. 2011).

Collection of winter mortality data enabled researchers to examine the relationship between food abundance and mortality (Pugesek et al. 2013). Butler et al. (2014*a*) criticized the direct measures of food availability (Pugesek et al. 2008, 2013) on the main crane food source, blue crabs (*Callinectes sapidus*), calling it "precarious" to assume that food availability could be measured at 2 nearby locations on the winter habitat. Butler et al. (2014*a*) did not discuss several important facts, and as a consequence, mischaracterized the crab abundance results. There were initially 3 locations sampled, the third location far removed from the first 2 (Pugesek et al. 2008) in a 4-year intensive study. Data were collected monthly from September through mid-April. Statistical and experimental controls were employed to determine the best low-intensity sampling protocol that would accurately measure the abundance of blue crabs and minimize disturbance to the cranes (Pugesek et al. 2008). Once that protocol was developed and tested on the first 4 years of data, the sampling protocol was repeated for another 4 years and used to analyze the relationship between crane mortality and crab abundance published in Pugesek et al. (2013).

Instead of a direct measure of food availability, Butler et al. (2014*a*) chose instead to compare mortality indirectly to several drought indices that they called "surrogates". They claimed that the surrogates encapsulated food availability and several other variables. Butler et al. (2014*a*) provided us with no information as to the construct validity (Bollen 1989) of their "surrogate" measure. In fact, Butler et al. (2014*a*) provided no evidence to suggest that there was any relationship at all between drought indices and the list of variables that they were supposed to measure. Since low construct validity can be a major source of error that can bias the results of regression-based statistics (Bollen 1989, Pugesek and Tomer 1995, Pugesek 2003), we believe that a direct measure of food abundance is the superior approach for investigating relationships with mortality. The logistic regression analyses of Butler et al. (2014*a*) were also problematic in that their sample size of 59 was inadequate for this type of regression. Logistic regressions require large sample sizes with n exceeding 200 recommended (Demidenko 2007, Machin et al. 2011).

USFWS made critical errors in their review and criticism of mortality detected on previous census flights (Strobel et al. 2012; Butler et al. 2014*a*,*b*). Butler et al. (2014*a*) falsely claimed that mortality was assigned when it was likely that the whooping crane had simply moved to upland habitat or outside the census area. Thus, according to them, birds were mistakenly counted as dead when they moved to other habitat and the chances of detecting a move back to the original territory increased when more flights were conducted.

In making their claims, USFWS failed to acknowledge some basic elements of whooping crane behavior that were of critical importance in making mortality estimates. Color-banding and radio-telemetry data clearly show the territoriality of wintering whooping cranes (Stehn and Johnson 1987, Bonds 2000). Whooping crane adult pairs establish winter territories that they return to annually (Allen 1952). Offspring remain, with only rare exceptions, with their parents throughout their first winter.

Using the census method allowed delineation of the population into adult pairs, family groups and subadults. When 1 crane was first noted to be missing from a pair or family group, the territory and nearby surrounding areas were searched a minimum of 2 times per flight to make sure the crane was not being overlooked. When not located on 2 consecutive flights, it was declared as a mortality. It is important to note that if all members of a pair or family group were not found on a census flight, this was never recorded as mortality; only single birds were ever declared as mortalities.

There has been only 1 instance of a bird declared as "dead" that reappeared the following fall. This involved a color-banded subadult in the 1989-90 winter, not located in the latter part of the winter, and declared "dead", that was sighted the following winter. Twenty color-banded birds have been declared as mortalities that were never resighted (note that color bands were read during yearly censuses as described above) (T. Stehn, unpublished data).

There are no known examples of a single crane in a mated pair or family group that has split off and moved outside the wintering area as postulated by Butler et al. (2014*a*) when they created their hypothetical category of "lost"; this is an illogical category because pairs or family groups almost never separate during the winter. Individual cranes belonging to pairs or family groups do not move by themselves from territories to upland habitat; the group moves together synchronously out of the territory. Movements of groups from a territory have never been counted as mortalities. Mortality was recorded only when 1 member of a group disappeared from a territory. There are no data supporting the claim that "lost" cranes were simply overlooked due to what was claimed as faulty census techniques (Butler et al. 2014*a*).

A correlation was found between upland use and crane mortality (Butler et al. 2014*a*), but this does not disprove the validity of the mortality estimate. This result, although the product of an analysis with a substandard sample size, would be expected when one postulates that increased use of uplands can be caused by food shortages in the marsh that stresses the population and leads to increased mortality. Also, predation risk from bobcats (*Lynx rufus*) increases with increased use of uplands (Chavez-Ramirez 1996). We believe that correlation is not a result of overlooking cranes on census flights that had moved to uplands as postulated by Butler et al. (2014*a*), since upland areas were thoroughly searched, and also as pointed out previously that individuals from adult pairs and family groups do not wander off by themselves.

Subadults do not have winter territories and may utilize different parts of the winter crane range over time. Also, subadult groups are variable in size and composition over time (Bishop 1984), so having 1 bird absent from a subadult group is not an indication of mortality. As a result, it was more difficult to ascertain mortality in the subadult group and generally only occurred when individuals appeared injured or sick and could not be subsequently located. Since subadults comprised approximately one-third of the population (T. Stehn, unpublished data), the mortality data likely underestimated the true mortality rate.

USFWS criticism of reported mortality is mainly based on their claim of finding an inverse relationship between reported winter mortality and number of flights conducted. This led them to falsely conclude that cranes were simply being overlooked, had left their territories in search of resources elsewhere, or left the census area (Butler et al. 2014*a*). While we believe there is some justification for the *a posteriori* partitioning of the Total Flights data set into subsets that remove flights when no whooping cranes were observed or there was no chance of detecting mortality, we know of no justification for USFWS to partition data to flights between December and March. They offer no explanation as to why they omitted a portion of the flight data or why the flight data were analyzed against mortality data from the entire winter period and included mortality discovered by means unrelated to aerial flights. Mortality as observed during census flights can be detected during periods when cranes are still arriving or departing the wintering grounds and has been documented outside of the December-March time frame. As a consequence, we believe that their entire analysis is invalid and their criticism of detection of mortality on census flights is not justified on this basis alone.

As previously mentioned, sample sizes in logistic regressions published by Butler et al. (2014*a*) were probably only one-third of that necessary to provide stable results. Sampling variation is inversely related to sample size. Inadequate sample size insures greater instability (i.e., departures from reality) among regression coefficients. It is for this reason that we used simple linear regressions in analyses presented here.

Our analysis of the entire Total Flights data set and subsets also indicated significant relationships between number of flights and mortality. Both the population size and year of data collection were also positively related to mortality, a finding that is to be expected. The number of flights was higher during the 1950s because objectives during that time frame included defining the dates when cranes arrived and left the wintering area. The number of flights declined further during the winters of 1982-83 through 2010-11 due to difficulty finding certified contract aircraft, and budget shortages as flight costs increased considerably with more time needed per flight to cover an expanded crane range. A higher number of mortalities would be expected from a larger population, and since year is highly positively correlated to population size we found a significant relationship between mortality and year. Year is likely autocorrelated with mortality, with no causal relationship between the variables.

Our results demonstrate that the number of flights and mortality are also autocorrelated, with no causal relationship between the 2 variables. First, since the number of flights per year declined significantly through time and with increasing population size, the significant relationship found by us and USFWS between mortality and number of flights is likely an artifact (i.e., autocorrelation) of the relationship between mortality and population size. In other words, the low r^2 detected between mortality and number of flights resulted from the same relationships described above between mortality, population size and year (i.e., time). At the very least, we can conclude that there is room to doubt the functionality of a causal relationship between mortality and number of flights when there is a more plausible alternative explanation. Furthermore, our alternative explanation is more parsimonious compared with the theories advanced by the USFWS, whose premises are fraught with error as previously described here.

Second, we acknowledge that the multiple regression reported here has a sample size that is too small for a reliable result. Sample size in multivariate regression-based models should be at least 100 but preferably 200 or more (Kerlinger and Pedhazur 1973) and the number increases with the number of variables (Thorndike 1978). However, the multiple regression illustrates an important point. Our results on this data set showed that when all the suspect causal variables are included in the analysis, Total Flights had no effect on mortality. Only population size, just short of significance at the 0.05 level, appeared to have any relationship with mortality. Multiple regression chooses a solution using the variable that explains the most variance, followed by the next variable that can explain the most remaining variance, and so on. In our example, population size is obviously the most important variable. Once population size is accounted for, year and number of flights, both of which are significantly related to mortality in univariate analyses, explain insignificant amounts of variation in mortality. This result, although short of proof, concurs with our suspicion that the number of flights is unrelated to mortality.

Third, our analysis of the ratio of mortalities to population size converts mortality to a rate. The conversion has the effect of controlling the analysis for population size. Once this is done, we find no relationship between the mortality rate and the number of flights. Had number of flights been associated with mortality, independent of time and population size, mortality rate should also have been significantly related to number of flights. This finding provides further proof to support our alternative explanation, and removes the primary postulate made by Butler (2014*a*) to criticize census mortality estimates.

Finally, no relationship was observed between mortality or mortality rate and any measure of number of flights in the modern data from 1982-83 to 2010-11. These are the methods and data under criticism by USFWS.

The USFWS approach to the issue was unsound and did not follow basic principles of data analysis. Chief among them was that they did not address the impact of confounding variables (Hahn and Dogaksoy 2011). As a consequence, we believe that they promulgated a logical fallacy, *cum hoc*, *ergo propter hoc* "with this, therefore, because of this". They concluded that 1 thing caused another simply because event Y occurred with event X, therefore, event Y must have caused event X.

CONCLUSIONS

We believe that the criticisms leveled at the previous census methodology are unfounded. The accuracy and limitations of the current distance sampling methodology are, in our opinion, a less desirable approach to monitoring whooping cranes on their wintering grounds at Aransas. In addition, the attempt by the USFWS to discredit the previous census methodology has, unfortunately, left repercussions in its wake that can only be described as detrimental to professional biology's relationship with the public and, in particular, with elected policy makers (White 2015).

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