

**SRC COMMENTS ON DRAFT 48-HOUR SEARCH INTERVAL (KB) STUDY, JUNE
2009, M32**

July 2009

Alameda County Scientific Review Committee

The Altamont Pass Avian Monitoring Team recently produced a draft 48-Hour Search Interval (KB) Study and asked for members of the Scientific Review Committee (SRC) to submit written comments on the draft report for discussion at an SRC conference call meeting on 29 July 2009. Individual SRC comment documents and e-mails are compiled here, in the order they were submitted.

Smallwood's Review of M32

K. Shawn Smallwood

19 June 2009

I appreciate the opportunity to review draft report M32, and I was often impressed by the level of effort devoted to this draft. It was an interesting good first draft, which can be improved in a number of ways. The following comments are directed to opportunities for improving the report, and are intended to be constructive.

Paragraph 3 on page 1 of M32 presents the study's goal and objectives as understood by the current monitoring team. The specific reason for this study was a lack of scavenger removal data on American kestrels and burrowing owls, which was important because fatality rates for these species have been surprisingly high. The primary goal of this study, as originally described by the SRC, was to simply test whether fatality searches performed every other day would detect more fatalities of small raptors than searches performed monthly. If more carcasses were detected, then how many more? Another objective of the study was to locate fresh carcasses of burrowing owls and American kestrels to obtain species-specific scavenger removal rates. Fresh carcasses were to be those determined to have died within two days of discovery. Tracking carcass condition after discovery was at the bottom of the SRC's interests in this study, but it emerged as more important to the monitoring team. I suggest that the monitoring team confers with the SRC to gain a better understanding of the objectives, though this review might suffice.

An example of how this report gets somewhat off track is the development of scavenger removal rates using older carcasses found during the study (see Data Analysis on page 4 and results that followed). The SRC intended for only fresh carcasses to be used for this purpose, as was clearly written in meeting notes and the monitoring team's original proposal. Using old carcasses was inconsistent with the study plan, and was inconsistent with how scavenger removal trials have been performed nationwide. Nobody that I know of has ever attempted to fit a removal rate model to fatalities represented by carcasses of old or indeterminate age because there is no

control over the variation in the data. I suggest deleting removal rate models resulting from the use of any carcasses other than fresh carcasses.

Also, M32 presents results that are peripheral to the report's objectives. It presents results in text that are redundant with information provided in the Tables. M32 could be shortened a great deal by focusing on the results relevant to the study's objectives and eliminating redundancy. Doing so would highlight the important, relevant results, thereby improving the report.

Draft Report M32 could be improved by including more of the background work that was done in 2006-2007 by the Scientific Review Committee (SRC) and the Avian Monitoring Team. It would have benefited from using (1) the sample size determination methods developed by the SRC and Monitoring Team, (2) review of the original project proposal, following the SRC's recommendations, and (3) consultation with the SRC subcommittee, as recommended by the SRC during its 18 May conference call. In addition to more background material, the report's readers would benefit from inclusion of more data, perhaps in appendices. I suggest either listing all the fatalities found or referencing a spreadsheet on the SRC web site. If referencing a spreadsheet, then I suggest also including a very clear data dictionary along with the spreadsheet. Different readers may wish to examine the data in different ways than presented in M32.

M32 described at great length how carcasses were processed for determining cause of death, age since death, and other screening factors used to determine whether the carcasses were included in the comparisons that were central to the study objectives. Therefore, it was odd that fatality searches were only slightly described and the report did not describe at all the fatality rate estimators or the mathematical context in which the study's objectives are relevant. For example, to help the reader understand the implications of including carcasses found only within 40 m of turbines, the report could have described the typical separation distance between parallel transects walked by the searchers, thereby giving the reader some idea about how far beyond 40 m the searchers would likely have detected birds during the 48-hour searches. Also, the regular search interval was reported to be about monthly, but a parenthetical statement noted 34-37 days, so which is it? It would be helpful to report specific search dates per turbine string.

Back to the Objectives

I used the information in M32 to address the SRC's primary objective, which was whether and how many more bird fatalities would be detected using a 2-day search interval. I was unable to estimate error terms, however, because the fatalities were not reported to turbine strings. I could have used the error terms around the scavenger removal rates, but I decided to hold off doing that until I can also use the error terms associated with the carcasses found per MW among turbine strings. (I suggest M32 be revised to include error terms and the information necessary to calculate error terms.) I found that the 2-day search interval detected 1.41 times the number of small raptors, i.e., American kestrels and burrowing owls combined, compared to the number that would be expected based on the detections from the 30-day search interval and use of the models in Smallwood (2007), and I found the 2-day interval detected 2.43 times the number of small birds (Table 1). It appears, therefore, that the fatality rates so far estimated for the APWRA are biased low. Minimally, existing American kestrel and burrowing owl fatality rates

need to be multiplied by a factor of 1.41, and those of western meadowlark and other small birds need to be multiplied by a factor of 2.43.

The biases likely revealed by the American kestrel-burrowing owl study were probably caused by factors discussed in Smallwood (2007), such as scavenger swamping. Smallwood et al. (2009) attempted to eliminate the effects of scavenger swamping by placing bird carcasses at a pace intended to simulate deposition rates from wind turbines, instead of placing 20, 30, or more carcasses at once. Applying the scavenger removal rates from Smallwood et al. (2009) to the data collected by the monitoring team in this study, the ratios of observed to expected numbers of fatalities found were closer to 1.0 for all birds combined, dropping from 1.75 to 1.32 (Table 2). However, it made no difference to small raptors. Scavenger swamping is probably not the only factor explaining the differences in the numbers of bird carcasses found using a 2-day search interval versus a 30-day search interval. Perhaps the searchers spent more time searching during the 2-day interval searches because they were finding more bird carcasses and so having to search for evidence of those carcasses each time they returned, thereby turning up even more carcasses. In other words, tracking carcasses may have increased searcher detection rates.

Table 1. Comparison of fatalities found between 2-day and 30-day search intervals. To estimate fatality rates adjusted for search detection error and scavenger removal rate, F_A , the following equation is often used:

$$F_A = \frac{F_U}{p \times R_C},$$

where F_U is unadjusted fatality rate, p is proportion of fatalities found by searchers during searcher detection trials (Smallwood 2007), and R_C is estimated cumulative proportion of carcasses remaining since last fatality search, assuming wind turbines steadily deposit carcasses through the search interval. R_C is calculated from estimates of scavenger removal rates based on trials (Smallwood 2007):

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

where I is average search interval in days, and R_i is the proportion of carcasses remaining the i th day into a scavenger removal trial, and usually best fit with a logarithmic model: $R_i = a + b \cdot \ln(i + 1)$, where a and b are fitted parameters. The Adjusted fatalities in the Table were limited to R_C , assuming p was a constant. Expected (Exp) F_U values serving as the denominator in the right-most column were calculated as: $\text{Exp} = F_A [I = 30] \times R_C [I = 2]$. For example, the ratio of observed to expected fatalities found during 2-day searches was calculated as $55 \div (29.4 \times 0.77) = 2.43$, so the searchers found 2.43 times more small birds during the 2-day searches than would have been expected based on the scavenger removal adjustment applied to the fatalities found during monthly searches.

Species/Group	Carcasses found		R _C , Proportion carcasses remaining (Smallwood 2007)		Adjusted fatalities, F _U /R _C (holding p constant)		Obs ÷ Exp F _U
	I = 2	I = 30	I = 2	I = 30	I = 2	I = 30	
All bats	3	0	0.77	0.34	3.9	0.0	---
Rock pigeon	10	6	0.96	0.83	10.4	7.2	1.44
Small birds	55	10	0.77	0.34	71.4	29.4	2.43
Medium/large birds	6	5	0.83	0.56	7.2	8.9	0.81
Small raptors	16	4	0.91	0.32	17.6	12.5	1.41
Medium/large raptors	5	4	1.00	0.93	5.0	4.3	1.16
All raptors	21	8			22.6	16.8	1.34
All birds	92	29			111.7	62.4	1.75

Table 2. Comparison of fatalities found between 2-day and 30-day search intervals using the RC adjustments derived from scavenger removal trials at Vasco Caves Regional Preserve and intended to avoid scavenger swamping (Smallwood et al. 2009). Rates used were from non-winter trials.

Species/Group	Carcasses found		R_C , Proportion carcasses remaining (Smallwood et al. 2009)		Adjusted fatalities, F_U/R_C (holding p constant)		Obs \div Exp F_U
	I = 2	I = 30	I = 2	I = 30	I = 2	I = 30	
All bats	3	0	0.567	0.209	5.3	0.0	---
Rock pigeon	10	6	0.538	0.245	18.6	24.5	0.76
Small birds	55	10	0.567	0.209	97.0	47.8	2.03
Medium/large birds	6	5	0.538	0.245	11.2	20.4	0.55
Small raptors	16	4	0.567	0.209	28.2	19.1	1.47
Medium/large raptors	5	4	0.553	0.286	9.0	14.0	0.65
All raptors	21	8			37.3	33.1	1.13
All birds	92	29			164.0	125.9	1.32

Additional comments:

Page 1, para 1: Isn't the permitting for 580 MW rather than 5400 turbines? There were 5400 turbines at one time, and there were more than 7,000 at another time. Presently there are probably fewer than 5,000.

Page 1, para 2: Change 'avian mortality' to 'turbine-caused avian fatalities'. Also, the statement about underestimating fatalities could be given more explanation. Underestimates would be made only if adjustments for the removal rates were not made; that is, if someone assumed the carcasses found represented all the birds killed.

Page 1, para. 3: The SRC didn't "direct" the monitoring team to do anything. The SRC lacks the authority to direct the monitoring team. More accurately, the SRC recommended to the County that this study be conducted, and the County directed the monitoring team to do it.

Fig. 2: This figure shows all the "study turbines," but this is a little misleading because all the turbines are "study turbines." I suggest all the turbines be shown, but the monitored turbines could be given a unique symbol or color, and the intensively monitored turbines given another unique symbol or color.

Page 3, 2nd open-circle bullet: How many carcasses lacked location information?

Page 3, 4th closed-circle bullet: The 40 m search radius was the maximum distance from which searchers walked from wind turbines, but it is not the maximum distance at which the searchers can find birds. While searching, searchers are scanning the ground in front of their paths and to either side of their paths to some distance between 4 m and probably 10 m, depending on terrain and other factors. There is no reason at all why bird carcasses out to 45 m of turbines should not be visible by searchers walking a transect 40 m from the turbines. I'd be inclined to omit carcasses found beyond 45 m of turbines rather than beyond 40 m. Including all the fatalities in an appendix would allow readers to decide for themselves which fatalities to include.

Data Analysis (page 4): This description of methodology is surprisingly thin, and it is directed entirely to the study's secondary objective of attempting to calculate removal rates. It did not address the study's primary objective. No models should be fit to carcasses of old or indeterminate age since death.

Page 9, para. 3: It would be helpful to inform the reader about how well the logarithmic model fit the data. I suggest including the coefficient of determination and standard error.

Results: The monitoring teams detected 112 unique fatalities, 9 of which (8%) were mutually found by both search teams, meaning the 2-day search team found 30% of the 30-day search team's carcasses, and the 30-day search team found only 10% of the 2-day

search team's carcasses. I can understand how the 30-day search team could miss 90% of the carcasses found by the 2-day search team, but do you have any thoughts about why the 2-day search team found only 30% of the 30-day search team's carcasses?

Table 2: The first record (orange-crowned warbler) needs explanation. It was last detectable on 20 Sep 2007, but was first scavenged on 22 Sep 2007. How could the searchers determine the bird was scavenged on the 22nd if it was no longer detectable by the 20th? The same question applies to the third record. I suggest reviewing this Table to sort out these inconsistencies – it's probably just a simple mistake.

Summary

This report has the potential to make a significant contribution to the scientific literature, and this draft is a solid start to that end. I suggest revising it and re-circulating it to the SRC. I recommend that more information be included on study design and survey methods, and that all the fatality data be included. I further recommend that the report be focused on the original study objectives. Finally, I suggest that authors be identified on the report, as is standard scientific practice. ICF Jones & Stokes is a large organization and not all of its members contributed sufficiently to be considered authors. I suspect non-Jones & Stokes people also contributed, such as Brian Karas. If so, then their names should be listed as authors.

Literature Cited

ICF Jones & Stokes (no authors identified). 2009. Draft Report M32: Altamont Pass Wind Resource Area 48-hr search interval bird fatality study. Report to Alameda County Community Development Agency, Hayward, California.

Smallwood, K. S. 2007. Estimating wind turbine-caused bird mortality. *Journal of Wildlife Management* 71:2781-2791.

Smallwood, K. S., L. Neher, D. Bell, J. DiDonato, B. Karas, S. Snyder, and S. Lopez. 2009. Range Management Practices to Reduce Wind Turbine Impacts on Burrowing Owls and Other Raptors in the Altamont Pass Wind Resource Area, California. Final Report to the California Energy Commission, Public Interest Energy Research – Environmental Area, Contract No. CEC-500-2008-080. Sacramento, California.¹

¹ The models I used in my review of M32 were modified from Smallwood et al. (2009) and appear in a paper which is undergoing scientific peer review.

July 13, 2009

Comments on “M32: APWRA 48-hour search interval bird fatality study”

By Julie Yee

1. Pooling data (page 2, paragraph 1, last line). Pooling the data will obscure any seasonal differences. If sample size is large enough, then analysis should also be performed separately for each of the two 2-month periods to address possible seasonal differences.

2. Backdated exclusions (Figure 3). On page 3, the fifth bullet states that fatalities are excluded when they are backdated to outside the two 2-month survey periods. The decision rule on backdated fatalities in Figure 3 only addresses situations where fatalities found after than the 2-month period are backdated into the 2-month period – in this case the fatalities are retained for analysis. I request the clarification: Is there a similar rule when the opposite occurs: fatalities found during the 2-month period that are backdated to prior to the 2-month period should be excluded from analysis?

3. Logarithmic regression (page 4, paragraph 1). It’s helpful to see the equation form of the model in the Methods (i.e. $\text{proportion} = a + b \log(\text{day})$). I had to infer it from Results a few pages later. The model differs somewhat from the one used by Shawn Smallwood ($\text{proportion} = a + b \log(\text{day}+1)$; Smallwood 2007). Also, it appears that the monitoring team’s 48-hour search interval analysis assumes $a=1$ (Is this true?). If comparisons with Shawn’s results are to be made, I would like to see any changes in the equation explained. Setting $a=1$ helps ensure that the model-based estimate of remaining proportion never exceeds 100% (provided b is negative), which could be better than allowing the regression to select a . However, the lack of “+1” in the log-transformation leads the model to then estimate a perfect 100% remaining proportion at 1 day after death, which is not realistic. I suspect a better model would take the best of both equations: $\text{proportion} = 1 + b \log(\text{day}+1)$. A comparison of model fit statistics such as R-squares or AIC would help determine what’s best.

4. Mutual detections (page 4, 2nd paragraph). The report points out an important observation that only 30% of the bird fatalities detected by the monthly survey was also detected by the 48 hour survey. If this were a double-observer study (which it kind of is), then such a low rate of mutual detections suggests a substantial rate of missed detections. This leads me to believe that the detection rate could be smaller than previously expected (Smallwood 2007 reported an average searcher detection rate of 75% for small raptors and 51% for small non-raptors), which would have significant implications for adjusted fatality rates. It has been speculated that species types and environmental variation, among other factors, could explain some of that difference. However, we don’t really know enough to understand. Ideally, we should try to understand the conditions that cause detection rates to vary, so that we can have better confidence in the

- adjustment rates. We should keep this information in mind when performing future adjustments on fatality rates.
5. Bat fatalities (page 6, 2nd paragraph). The observation about bats is interesting. A 3:0 ratio of bat fatalities found by 48-hour survey relative to monthly survey implies possibly very large numbers of missed bat detections. I looked to see if a similar pattern occurred with American kestrels (which had a 4:0 ratio of detections by the two surveys). However, unlike bats that were found only 8 times over the last 3.5 years (according to this report), kestrels were found 60 times over the first 2 years of the current study (according to Table 4 of M21). So, while the 48-hour study turned up a much larger per-monthly rate of bat fatalities compared with the current study, it also turned up a much smaller per-monthly rate of kestrel fatalities (1 per month in 48-hour survey versus 2.5 per month in current survey). What could *that* mean? There may be some explanations, such as seasonal as site variations (or even changes to the definition of a fatality between the M21 and M23 report?). If we're going to accept this level of inference on bats, then the same type of reasoning should work with focal species, but I'm not sure it makes whole sense. Perhaps it would be informative to have seasonal unadjusted fatalities from the current study for these turbines. Is that data available yet on the website or can it be made available in the report?
 6. Unknown age fatalities (page 9, 2nd paragraph). I'm not sure I understand the criteria for including the additional 18 fatalities of unknown age. The report reads that they were "likely to be fresh because they were discovered *after* more than two searches" had been conducted. I request the clarification: Does this mean that anything of undeterminate age found after the first four days of the 48-hour survey (i.e. after 2 searches) was included in this group?
 7. Proportion of carcasses remaining (Figure 6). There is quite a bit of difference in the regression lines, especially at around the 30-day interval where most of the adjustments would occur. One of the next things to determine is can we use any of these regressions for determining better adjustment factors, and if so then which one? I'm not keen on the 3rd regression which includes fatalities of unknown age; [I'm not convinced they were "likely to be fresh" just because they were undetected after two searches, after considering the low rate of mutual detections among fatalities detected by monthly search (back to comment 4)]. That still leaves the 1st and 2nd regressions which have wide differences, although collectively somewhat consistent with the original Smallwood regression. Are these differences statistically significant, or could they be attributed to sampling variation? It could help to know what the standard errors around the regression lines are (or at least around the parameter estimates). Are there other reasons that might explain the differences, such as species attraction differences? This topic requires SRC discussion.
 8. Survival model. This is a statistical issue. As an alternative to the log regression model, I keep thinking that a survival model is more appropriate. I believe that

the log regression model was used in order to make consistent comparisons with the Smallwood 2007 study which also used a log regression model. But there are differences in the manner the studies were performed. The 48-hour study is similar to survival studies which follow live subjects until death or censoring (when subjects outlive the study). Instead of live subjects, this study follows the “life” of carcasses until disappearance or censoring. It is reasonable to assume that survival times of carcasses are independent. However, by modeling the data as a log regression, then the data are converted into sampling units representing proportion of carcasses remaining. The same carcasses are used to calculate each proportion, so there is arguably a lack of independence in that data. The method raises other questions. How many proportions were calculated? One per day? One per each disappearance event at the time of disappearance? [I started to consider the latter approach (one per disappearance) since at least that sample size would correspond with the number of carcasses. But that means there would be no within-day variation; for example, the three carcasses which were last detected at 3 days would contribute the exact same proportion to the regression data]. In the end, none of these regressions seem very appropriate because the proportions are not independent. By contrast, in the Smallwood 2007 study, the proportion data were independent because they were collected from independent studies. For the 48-hour study, I would advocate using a survival model rather than simple regression (in SAS, procedures such as LIFEREG, LIFETEST, PHREG). I don’t make the suggestion lightly though, since those methods involve further decisions and work. I am open to arguments in favor of the log regression.

9. Table 2. A few clarifications. What do the superscripts ^a and ^b represent? And how should we interpret “Date First Scavenged” when it reads “< 2 days” or “<1 week”? I presume the 2nd and 3rd records should be “Sep-07” rather than “Sep-08” under “Date First Scavenged”?
10. Conclusion 2. (page 11). The potential explanations are more like Discussion items rather than conclusions.

FROM: Joanna Burger

RE: COMMENTS on M32

M32 Kestrel/Owl

OVERALL: I was impressed with the quality of the work, particularly on M32. These are extremely important topics. However, the report does not do justice to the data, and the final conclusions should be clearer in an abstract.

SPECIFIC:

1. The report needs a short, one paragraph abstract. What is the main finding, based on what?
2. The main objective needs to be clarified (3rd paragraph on page 1). It was the objective to examine fatality for burrowing owl and kestrel (not of particular interest). We need to be very very clear what the major objective was (not all small birds).
3. Following from #1, there is more attention devoted to other species and bats (this information should be in an appendix, not the main body).
4. Under methods, and elsewhere, you need to distinguish between the 2 –day search interval and others.
5. Need always to define “subset” (page 2, bottom).
6. Fatality data analysis – please give an indication of how many records fit into each category that was excluded. Otherwise, it is difficult to evaluate.
7. I ALWAYS like to see separate results and discussion. It is confusing to put these together. Then they can clearly do this.
8. Page 4, bottom. I’d like to see what percent of owl and kestrel records were feather piles.
9. Bats are not relevant (more to appendix).
10. The finding that scavenger removal of small birds is higher than expected is what should be featured and discussed.
11. Clear information on how the scavenger rates differ should be clearer.

Comments on M32 by Sue Orloff:

1. I know the sample size is small; however, I would still like to see at least some analyses broken out by raptor/non-raptor. The study focus was originally to be on small raptors, not all small birds. Your data on small raptors gets combined with all the other species and thus is lost in the mix. Examples of questions that could be answered include the following: a) How does the detectability of small raptors differ between the 48 hour and the monthly surveys? Shawn did some of this in his comments. b) How did carcass conditions when found differ for small raptors versus non-raptors? Your summary of this was for all birds. c) What percentage of small raptor carcasses remained detectable (i.e., residual evidence)? It's a very small sample but I believe it is 100%. This is quite different from the non-raptors which disappeared quickly. This might help to answer the question - can small non-raptor species be surrogates for raptors in removal studies.
2. Although I don't think you should use "fatalities of unknown age" in the primary data analyses, it is still interesting that there is a much higher proportion of small raptors in this group than in the fresher carcass groups. This supports your other data that shows small raptor carcasses remain more detectable over time. This kind of data could be placed in the appendix and referenced in the text.
3. No kestrels were found during the monthly surveys and only 4 during the 48 hour surveys. But as Julie pointed out, 60 kestrels have been found over the first 2 years of the current study. Could this discrepancy be due to low abundance by kestrels in these new sample sites? I believe we selected the sample sites for the 48 hour study based on abundance of burrowing owls, not kestrels.
4. I think it would be useful to put the number of mutual detections on Table 1.
5. I would like to see some data analysis on the distances fatalities (raptors/non-raptors) were found from the turbine bases. This might shed some light on the likelihood of predation as a causal factor.

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