

SRC Comments on CalWEA Research Plan

Altamont Pass Scientific Review Committee

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JOANNA BURGER COMMENTS ON CALWEA PROPOSAL

OVERALL: I believe it is a worthy cause to try and come up with better methods of refining estimates of bird/bad mortality, and it is essential to conduct a pilot study to determine feasibility and the potential costs to the monitoring team, as well as whether it will accomplish the projects objectives. It is also a positive that personnel from the current monitoring team and people from industry are involved in oversight and the project. I think the experiments as designed will provide information – but on cowbirds, which is not the main question at the Altamont. The applicability of the data need discussion. Also, how is this experiment relevant to repowering, the main recommendation of the SRC? I am not questioning the validity of the study, but its applicability to the Altamont, given the constraints we already face with these same issues.

SPECIFIC COMMENTS:

Project goals: It may be that the goals are a bit over-stated, particularly for a study conducted at one site, in less than a year. Such a study cannot really look at a variety of environmental conditions. The goals could be a little more specific. What hypotheses are being tested?

I always find it a bit presumptuous when someone states that “there are no publications in the peer-reviewed literature based on actual field observations...”

Literature Review: adequate

Site Selection: You might include something about why you chose Altamont, and why there isn't a reference site or different site that might include different environmental conditions.

I am wondering how visibility class takes into account contours, and the matrix of contours, which is an extremely important variable in the Altamont and elsewhere. Also, it is not clear how seasonal and grazing effects will be included.

The question of cover type around the carcass vs the string or general area needs further discussion

I am still not completely comfortable with the study species selected, since they are not the relevant species or sizes of birds of interest in the Altamont. Further, there are still the questions about fresh versus frozen.

Draft experimental design:

Why isn't there a specific section for the Pilot Study (with goals, what will constitute acceptable or unacceptable logistics or effects)? I would like to see a very clear section on the pilot study, its goals, methods, and outcome measures. How will we know if it is a success, and what is the timing relative to conducting the full study?

There is a question about logistics in that if up to 11 birds are found on one survey, this involves nearly half the Altamont, yet not all strings monitored by the monitoring team will be part of the CAWE study – so how can we ensure that numbers are swamping the available predators – especially since this number is ADDED to the normal mortality out there?

I am not sure where the “without operational turbines or in isolated strings” is coming from. The CAWE is expecting the monitoring team to be conducting these, so are there additional strings they will need to cover? There needs to be a little more detail concerning the actual monitoring in connection with the strings.

What is the relationship or timing between the different types of trials, how many days between trials, how much separation for a given string? We need details of how it will work.

Specific Considerations:

1. The Project Manager, I assume, is a full time position?
2. Carcasses can age from decay and decomposers, as well
3. It would be nice to have references for the statements in this section.
4. I am still worried about the issue of inter-observer biases, and how they affect small bird carcass detection.

On-going Altamont study:

1. How will isolation remove the effects?
2. Independent experiments? I am wondering if the total increase in carcasses at the Altamont, due to these experiments which could double the number of carcasses found, will have the ability to change the presence and number of scavengers. Will they learn to look at these non-working turbines?
3. Given the length of experiments, and the number of strings involved, I am wondering if it really can be done in 11 months. Why not take more time so that swamping and effects are less?
4. What will the effect be of not treating these experimental carcasses the same as those of the usual monitoring (e.g. removal).
5. Need more details on the pilot study.
6. Does this mean Mr. Karas will NOT be part of the regular monitoring team for a year? I have the greatest confidence in Mr. Karas.

7. How will data be entered, how will it be kept separate from the usual data.

QA Plan

1. Some information on topography needs to be added to the QA plan
2. Is time of day likely to affect the results?

I would like to see a section on expected results or outcomes. What specific questions will be asked, how will the data be analyzed, what are the hypotheses being tested.

Shawn Smallwood

Review of Research Plan to Improve Methods for Estimating Wildlife Fatalities at Wind Energy Projects

26 June 2010

Shawn Smallwood

The California Wind Energy Association (CalWEA) submitted a research plan to the Alameda County Scientific Review Committee (SRC) for review. The research plan is for a combined searcher detection and scavenger removal trial in the Altamont Pass Wind Resource Area (APWRA). As a member of the SRC I am providing the following comments on the research plan.

Some elements of the research plan have merit, such as the objective of characterizing the interaction effects between removal rates and searcher detection. The plan can be improved in multiple ways, however, and I would prefer to see it revised significantly before endorsing it.

(1) Clarity

The plan is confusing and vague in many places, including some of the generalizations made about removal and searcher detection trials performed around the USA. For example, there is no author name(s) associated on the plan, so it is unclear who wrote it. I was also confused over the distinctions between Project Manager, Study Director, and Project Director. I understand that Brian Karas is to be the Project Manager, but if the Project Director and Study Director are other people, then is Karas not the person who will be making daily checks on bird carcasses? If not Karas, then whoever is to do the daily visits needs to be identified so that the SRC can assess the person's credentials, because the daily visits are a critical part of the plan.

The plan often referred to the "literature," but without citing sources. For example, bullet (1) on page 4 states that some fatality equations have been evaluated using computer

simulation, but no mention was made of who did this. Bullet (8) on page 5 implies that someone incorrectly stated a statistical bias occurred (on something) but there's no indication of who made the incorrect statement. On page 17 the plan states that studies on the east coast show that air temperature is a major factor in affecting scavenging rates. Whereas I have my own experience to support this notion, it would be helpful to cite the reports of the east coast studies.

In another example of confusion, on page 4 the plan states that removal trials performed during the day can miss scavenging events that occur at night. This statement makes little sense because the time of day when removals take place is irrelevant to estimating removal rates. This concern reveals unfamiliarity with scavenger removal trials.

(2) Objectives

The objectives could be stated with more humility. I find it highly unlikely that the objectives will be achieved as stated, though as I said earlier, I nevertheless see value in some elements of this study. It is unlikely that Objective 1 will be achieved because the “variety of environmental conditions” might prove more difficult to find in the APWRA. In the plan's description of the experiment, the variety of environmental conditions would appear to consist of two grass heights: low and deep.¹ I am unclear what this means in the APWRA. The APWRA is covered by annual grasses that are grazed by livestock, so grass height is seasonal. Tall grasses in the APWRA lay down, so I'm not sure whether this situation would be classified as low or deep (I'd go with neither). What would qualify as an example of deep cover in the APWRA? Also, visibility can be affected by whether the grass is green or yellowed, which is another seasonal factor, and in some places there are herbaceous plants and in others the ground is bare. Perhaps a better way to factor in visibility would be to measure it using a painted grid viewed by a fatality searcher from 6 to 8 m away. The viewer would count the visible squares from the bottom to the top of the grid.

Objective 2 will be difficult to achieve because the monitoring team would be visiting placed carcasses only after days 1, 5, and 10 since placement. It will be difficult to generate a defensible removal curve from only 3 experimental time periods. Furthermore, although the plan discusses monitoring feather piles, body parts, and carcasses in a variety of stages of decay, the truth is that the placed carcasses will only present the monitoring team with the range of carcass conditions that can be expected within 10 days of placement. In only 10 days, there will be fewer of the small, weathered feather piles, digits with attached feathers, and surprise mandibles that the monitoring team finds after 30-day search intervals.

Some elements of Objective 3 will not be achieved due to the experimental design and the realities of implementation. For example, the effects of carcass size will not be tested against removal rates or observation error because only one species of birds will be used – brown-headed cowbird. Also, as explained in the previous paragraph, age of carcass

¹ Note that low and deep are categories that are inconsistent with the Pennsylvania Game Commission visibility classifications on page 8.

will be limited to within 10 days, which is only a third of the search interval most commonly used around the US. Age of carcass will likely prove to be an unsatisfactory covariant with removal rates and observation rates in this study.

Furthermore, by placing so many of the same species at experimental strings, fatality searchers will probably be alerted to the trial each time they find a brown-headed cowbird. The searchers will know that once they find a cowbird or a bat, they better search harder to find the rest of the cowbirds and bats because they are being tested. This is a fundamental source of bias that this study alone cannot prevent. It is not a problem in the SRC's recommended double survey approach, however.

(3) Plan efficiency

The plan's implementation seems inefficient. The plan's objectives appear to hinge on placed birds being visited by the monitoring team, but the 30-day trials will allow for only one monitoring visit per placed bird. The plan describes a designated person visiting placed carcasses once per day over 30 days, but the monitoring team will have an opportunity to encounter each bird only once. It remains unclear to me what value will be brought by the designated monitor's 30 daily visits. The daily monitor himself will be doing nothing new in a removal trial, since daily visits have already been made in removal trials and we just finished debating the results of our own trials that were based in searches every two days.

It is also unclear how the daily visits would be performed. I assume the designated monitor would be visiting the specific sites where carcasses were placed, but what would be done if the carcass is missing? Would the monitor search a larger area to determine whether the carcass was moved within the monitoring team's search area? Without searching the entire monitoring team's search area around each turbine string, how can one control for relocated carcass parts that might be found by the monitoring team? We just experienced this problem in the KB study.

Using only one bird species in the trials also poses an interesting implementation problem. Placing 15 and up to 30 brown-headed cowbirds at the same string ought to cause some confusion about which bird part came from which bird, once scavengers spread some parts around. It might prove difficult determining how many brown-headed cowbirds were removed once feather trails, feather piles, and body parts are encountered in locations other than where birds were placed.

(4) One bird species

Using one bird species in the trials will not suffice for parameterizing fatality rate estimators of the many bird species killed by wind turbines in the APWRA and elsewhere. Not only will the one species tip off the searchers to trials, but the visibility of cowbird carcasses will not be the same as the visibility of carcasses of American kestrels, western meadowlarks, house finches, and a long list of other species. The results may not

be sufficiently robust to change the scavenger removal adjustment factor for small birds as a group.

(5) Carcass load

It appears from the tables on page 11 that the study would place 1,310 bird and bat carcasses in the APWRA over the next year. That's an awful lot of carcasses for one monitor to visit every day for 30 days, tallying to about 39,300 visits. Specifically, the daily carcass monitor would need to visit 8 turbine strings per day, limited in time to only the early morning or evening hours when the regular monitoring team is not in the field, and in that time he is to visit about 15.6 carcasses per string (1,310 carcasses divided by 84 trials), or 125 carcasses. If the strings are widely spaced apart, as explained on page 18, simply traveling from string to string without making a single carcass check would consume most if not all the available early morning or evening time available to the monitor. Visiting 125 carcasses per day in this limited time window will not be possible. The logistics of this study need to be reconsidered.

On pages 13 and 18, the plan defends the use of so many bird and bat carcasses. First, the plan states that high density placements of carcasses will be made at strings without operational turbines or at isolated strings. However, I believe this approach could generate results that are different from placing the same numbers of birds and bats at strings with operating turbines. Scavengers likely patrol turbine strings they've learned will produce food, so placing larger numbers of carcasses where scavengers are not patrolling will likely bias the removal rates low. Also, strings lacking operational turbines will likely be abandoned by fatality searchers in the new monitoring plan, anyway.

On page 18, the plan states that high density carcass placements could go to non-operating turbines at Tres Vaqueros. I suspect, however, that those turbines will soon be removed. Furthermore, CalWEA would need to gain land access permission from East Bay Regional Park District.

(6) Monitoring to be performed by *neutral third party*

The 2005 County of Alameda Board of Supervisors Resolution R-2005-453 states that scientific, independent monitoring, including other research objectives to be established by the SRC, is to be conducted by a "neutral third party." CalWEA is not a neutral third party. It is a lobby organization supported by members of the wind industry, and includes membership by several APWRA wind companies. I have first-hand experience with CalWEA's aggressive attempts to discredit scientific findings that CalWEA regards as threatening to the wind energy industry. For example, in a letter to the California Energy Commission, CalWEA characterized my 2004 report by stating, "*it appears that the overall mortality estimates were inflated compared to what is supported by the available data,*" and "*it would be pure coincidence for a correct analysis to produce the same fatality-turbine associations, given the breadth and depth of errors found*" (<http://www.calwea.org/pdfs/CalWEALettersReports.pdf>). One of CalWEA's members,

Pilz & Co., accused my work of being fraudulent, including data alterations. Bill Warren-Hicks, who I assume was the principle author of the study plan at issue, was part of CalWEA's campaign to discredit the 2004 report (http://www.calwea.org/pdfs/EcoStat_Comments_on_Consultants_Report.pdf). Within multiple submitted letters and reports labeled "peer review" or "independent audit," CalWEA and its associates conducted themselves in a manner that I find impossible to characterize as neutral.

The Board Resolution also states that the monitoring consultant is to "assemble...data for use by the SRC." The SRC previously encountered a conflict with this part of the Board Resolution when AWI wanted to control the data collected as part of their proposed blade painting study. The SRC made it clear at the time that the monitoring team and ultimately the SRC needed to be in initial possession and control of the data. The meeting notes from 24 September 2007 (SRC document P59) read, "*AWI stated that they would then like to analyze the data. The SRC raised concern that scientists would question the validity of this analysis and might perceive it as biased. The SRC believes that an independent team, such as the monitoring team, needs to conduct the analysis. The County clarified that the permits require independent monitoring and analysis. The SRC concurs that independent monitoring and analysis is critical. AWI can also conduct analyses; however, for the purposes of the SRC, an independent analyst will be necessary.*" Therefore, to be consistent with the Board Resolution and the SRC's previous recommendation on data management, the data collected under CalWEA's proposed study should be managed initially by the monitoring team and overseen by the SRC.

Additional comments

On page 5 the plan presents a paragraph summarizing the Environment Canada guidelines. The plan could also summarize the Japan guidelines and guidelines from additional countries, but I don't see the point. What do the Environment Canada guidelines have to do with this study or with estimating fatality rates in the APWRA?

Bullets (4) through (8) list shortfalls of conventional removal trials, but all of these shortfalls would be remedied if the double survey approach proves effective.

How would the proposed study improve on the shortfall mentioned in bullet (9)? Bullet (9) addresses error associated with carcasses being missed during a survey event but found again in repeated survey trials. The SRC is familiar with this error. However, I fail to see how the proposed study would eliminate or reduce this error, since the daily visits would present the same problem as 48-hour visits. Furthermore, the monitoring team would visit each carcass only once.

The site description on page 6 could be improved by deleting oak woodland, conifer forest, and agricultural land. There is no conifer forest in the APWRA.

At the top of page 9, I disagree that the four visibility classes on page 8 cover the range of cover that can be expected between wind resource areas in California.

On page 10, the plan states that the Study Director will visit each carcass placement site every 30 days, recording the true number of animals on the plot each day. As we just observed in the KB study, it is unrealistic to expect the Study Director to record the true number of carcasses present once removals have begun taking place. The recommended double survey approach would avoid this problem.

On page 17, the plan defends using the current APWRA monitoring team by characterizing it as expert. I agree that this team is expert. However, the team should be tested. It is a long-standing recommendation of the SRC that the team be tested. The plan states that the team routinely finds "minute carcass sign that is at the lowest level of their fatality definition," and I agree with this statement, but I must add that we do not know how often this team misses the same type of sign. Even the best fatality searchers are going to miss some fatality remains. Indeed, our experienced team in the APWRA failed to detect 90% of the carcasses found by the KB monitoring team (many of the KB carcasses were likely removed by scavengers prior to each APWRA-wide monitoring team visit), and the KB monitoring team failed to detect 30% of the APWRA-wide monitoring team. The Alameda County Avian Monitoring Team is expert, but it is not perfect. This proposed study or another detection trial should be performed to establish searcher efficiency rates in the APWRA.

On page 20, the plan identifies Brian Karas as the Project Director. Whereas the plan argues throughout that little or no impact would be caused to the ongoing monitoring

program as a result of this study, I would argue that losing Mr. Karas would be a substantial impact on the monitoring team.

Suggested Improvements to the Plan

The proposed plan is logistically impossible as written, highly inefficient due to emphasis on daily carcass checks over 30 days, and will be unable to achieve the stated objectives. I suggest the following major revisions to the plan.

- Transfer monitoring and data management and analysis responsibilities to the Alameda County Avian Monitoring Team under the SRC's review.
- Modify the objectives so that they are achievable.
- Develop realistic measures of environmental variation as a covariate of detection rates.
- Incorporate carcass placements and daily monitoring into the SRC-recommended double survey approach, so that carcasses are placed at turbine strings that will be visited by two monitoring teams (double survey) instead of one. In this way, the monitoring team visits to placed carcasses would be doubled, which would enable the sample size to be halved and the range of time periods between carcass placement and monitoring visits would be expanded considerably.
- Use more than one bird species.

If these changes can be made, then I would be optimistic about the success of this proposed study.

Julie Yee

Yee review of CalWEA draft 1 proposal
July 21, 2010

SUMMARY. I appreciate the project goals of developing a better understanding of (1) observation error, (2) carcass removal rates, and (3) their relationship with one another, as these are very important to estimating fatalities of birds and bats. While previous studies have provided much information on the first two elements, there is relatively little information about how they vary with respect to one another and under different environmental conditions. In the April 2010 SRC meeting, the CalWEA proposal was discussed, and some comparisons were made to the adjustment factor study previously proposed by the Monitoring Team, as there are strong similarities. Although both proposals have similar aims, and can be expected to contribute valuable information to the current body of knowledge, they are not the same. My comments are organized into two sections dealing with merits or limitations, some of which follow-up the comparison to the Monitoring Team proposal, and clarification about the way the proposal is written.

I. MERITS AND LIMITATIONS

Truth is known, or is it? During the April 2010 SRC meeting, one of the noted merits of this study was that the rates would be analyzed based on known truths about the number of carcasses deposited, because the experiment will set those truths by placing a pre-specified number of fresh (or frozen) carcasses and monitoring them for removal and detection by observers.

This level of truth is not particularly novel, because previous scavenger removal studies have also placed known numbers of carcasses for monitoring scavenger removal. Similarly, observer detection trials have placed known numbers of carcasses for experimentally evaluating observer detection. But, an important benefit of the CalWEA proposal is that it couples the combined processes of scavenger removal and observer detection into one experiment, which should help resolve important and lingering concerns on how best to combine those two rates when calculating adjusted fatalities.

The Monitoring Team's quality control study proposes a similar approach except that the monitored carcasses would enter the study through the course of discovering naturally deposited fatalities. In the April 2010 meeting, it was suggested that the Monitoring Team's proposal offered a less complete truth, because an unknown number of carcasses will have already been removed or remain undetected. In other words, what is discovered doesn't pass for truth.

Since that meeting, I've come to the opinion that the truth value in CalWEA's proposal will be no better than the Monitoring Team's. Although CalWEA's plan prescribes an exact number of carcasses for deposit, there is no assurance that this will be the effective number of carcasses there. Undetected additional fresh carcasses might exist at the time and location of deposit, and unknown future carcasses can continue to deposit naturally over the subsequent 30 days of monitoring. So, the CalWEA study's truth will not be true either. Arguably, this is not too great of a concern, because I think the study will primarily consider the fates of only the CalWEA carcasses. By the same argument, it is just as irrelevant if the Monitoring Team's quality control study carcasses are an incomplete set. Basically, as long as the fatalities are fresh, then I don't see how it matters whether they enter into the study by way of prescription (CalWEA) or discovery (Monitoring Team).

Characterizing Error. What I value most about this proposal is its emphasis on analyzing variances. Not only does this include analyzing the covariance between rates, which is important to resolving important questions about properly combining both rates in the adjustment, but it also includes analyzing other variances involved in calculating estimation error. Estimation error is important for placing confidence values into the context of a decision. For example, suppose two mitigation actions, A and B, are associated with an estimated 10% and 15% reduction in fatalities respectively. This might lead a decision maker to favor mitigation B (assuming other factors such as costs were comparable). But then suppose the margin of error around these estimates were 5% and 25% respectively. This would imply that mitigation A provides a more predictable benefit, while mitigation B might not provide real benefits at all.

Applicability. Another point made during the April 2010 meeting was that CalWEA's choice of bird species (cowbird) was different from the species of interest (four focal

raptor species) among stakeholders in the Altamont. I continue to share this concern, because I'm not sure how exactly a cowbirds and bats study will contribute to the estimation of raptor fatalities (by comparison, the Monitoring Team study would use fresh raptor carcasses and feather spots). Scientists are reluctant to assume that rates for cowbirds can be substituted in place of raptors. However, I suppose one possible redeeming element of the cowbird study might be to apply the *relationship* between rates for cowbirds as if it was the same for raptors. The assumption of a same relationship between rates is, in my opinion, probably less of a stretch than the assumption of same rates, but I would be interested in hearing biologists' view on this.

As for the applicability of the CalWEA study to other situations or locations, I'm not familiar enough with other sites to comment on this. The proposal notes that the available literature stresses the use of site-specific and even team-specific studies to determine the adjustment equation (item 4 on page 5). In this regard, I'm not sure how the CalWEA study will have broader application than the Altamont and the current Monitoring Team.

II. CLARIFICATION

Page 2. Project Goals

Goals include to (1) "Explicitly evaluate observational error" and (2) "Test and evaluate the shape of carcass removal curves" under a variety of environmental conditions. The testing of observational error in (1) is conspicuously absent. If there is equal intent to test the observation error, then they should be stated equally.

The proposal mentions dependence between the two rates. While I don't expect a direct cause of dependence (i.e. I believe the proportion of carcasses that "survive" scavenging will not, by its value, affect the rate that observers will detect that set), I do agree that the two rates could co-vary as a result of dependence on the condition of the remains and the environment (for example, in areas with high visibility, then scavenging might vary higher while observer error varies lower). A covariance would certainly lead to a statistical dependence between the two rates, which then influences how those rates should be combined.

Page 4.

Extra open parenthesis "((" on the definition of t_{ij} .

Issue (1). It would be helpful to know which issues have been tested by simulation. And what were the outcomes? As best I can tell, the particular goals stated in this proposal can only be evaluated by on-site experimentation and I wonder what the simulations could have possibly tested. [By personal communication, Bill Warren-Hicks stated that this item refers to work in a 2004 technical paper by Peter Shoenfeld, which I have not read].

Issue (3). I agree that the denominator terms may not be independent, and this is one of the more important issues that this study can help to resolve. However, while the example hits this point, it then goes off on what seems to be a tangent about length of trial and frequency of other studies' searches.

Page 5.

Issue (4). In paragraph 2, I think the following sentence is vague: "The relationship between the search interval used in the post-construction monitoring survey design and the search interval used to generate scavenging probability generally are not coordinated." [By personal communication, Bill Warren-Hicks clarified that they hope to examine the influence of search interval]

Issue (6). I view this as inherently part of the same issues as (3) and (5), which makes this item kind of redundant.

Issue (7). This statement is vague. I'm unsure whether this issue refers to elapsed time, as in the dependence of scavenging rate on time between searches, or to calendar/clock time, as in seasonal or diurnal effects.

Issue (8). Which concept of statistical bias has been incorrectly stated? I recognize there is bias, but unsure if this item is referring to the same biases I'm thinking of. If so, then this item could be redundant with issues (3) and (4) combined. I wish this proposal was clearer when raising issues, and if or how it expects to resolve them.

Issue (9). True. But I don't see how the proposed will resolve this issue either. As written, each experimental carcass will be monitored only 30 days and the monitoring team will have only one opportunity to detect the carcass. This study will provide no data on second chance detections nor on the removal rate for carcasses up to 2 search interval lengths (30-60 days is the duration that fatality evidence would need to persist in order to be found in a second interval).

Page 7.

The last sentence indicates that there follows a description of an approach for selecting specific strings, but I didn't see any such description other than that the sites will vary in ecological condition (with some restriction in selection for high density experiments).

Page 8.

Drop the ">" signs in Class 3 and 4 descriptions.

It's unclear whether the Pennsylvania Game Commission visibility classification is intended to serve as an information-based approach to site selection. On one hand, it was hinted on previous page that there would be a specific string selection approach, but this approach would require information of the visibility class across the APWRA. But then,

on page 9, the proposal suggests that visibility class would be collected as a covariate, which then implies that visibility class won't be part of the selection criteria. The role of the visibility classification needs to be clarified.

Page 10.

I like that there is daily visitation, at least in the initial days after depositing the experimental carcasses. I'm less certain that daily visits are required after that initial period.

For statistical reasons, it might be prudent to extend the Study Director's visits beyond 30 days. When fitting a curve to data then, depending on the approach of the estimation, it's often the case that the curve is estimated with better precision at the center of the curve than at the end. If this proposal is partly intended to improve upon the removal rates used for the Altamont's Monitoring Team's survey, then it would be desirable to have reasonably precise estimates for removal up to 30 days. Extending the support for the curve beyond 30 days (say 35 or 40 days) would improve the precision of the rate at the 30-day point. This wouldn't even have to increase the numbers of visits if >1 d intervals are used for the later days.

To what extent will the Study Director examine carcasses at the site? Will he check the status of the experimental remains and then leave, or will he routinely search throughout the plot for other evidence? Since the proposal is to evaluate removal rates under various levels of density, then it's relevant to consider the numbers of naturally deposited carcasses at each string in addition to the experimentally deposited ones. For example, if sites A and B are selected for low-density experiments of 5 carcasses each, and site A happens to also have 10 fatalities occur while site B has none, then site A would effectively be more of a medium-density experiment that is on par with sites that received 15 carcasses and no additional fatalities.

Page 11.

In the sentence that reads "The number of birds or bats...was chosen as a number representative of *expected daily maximum true mortality*..." This seems to imply that the expected maximum mortality at a single string on a single day could be as high as 30 birds or 30 bats. Is it realistic to expect a maximum of 30 fatalities per day? Was the sentence intended to read *monthly maximum*? I'd like a better understanding of how this particular range of densities (5-30 per string) was chosen. Preferably the experiment should be conducted under realistic levels of densities.

If it was intended to read *daily*, then this raises another issue about the feasibility of this proposal to experimentally control the density of carcasses, since that the effect of that control becomes diluted and eventually lost over the course of time as new fatalities accumulate. This is less of an issue if the intended wording was *monthly*, since this implies a slower rate of accumulated fatalities.

Table of experiments. This table needs a caption explaining the columns and records. It wasn't clear to me that each row represents an experiment. The pairing of "medium" and "bat" rows made me wonder if these particular combinations of birds and bats were going to be deposited together. Explain if they are or aren't. The major ecological condition is not defined – and was it supposed to correspond to the visibility classification on page 8? I also wondered why the same combinations were repeated four times. If they are multiple experiments for repeated combinations, then I recommend collapsing the table to show each experimental combination once, and adding a column to indicate the number of experiments of that particular combination. This would reduce the height of the table by 75% so that the information can be easily viewed on one page. [By personal communication, Bill Warren-Hicks said he's been advised that placing birds and bats together is OK, so that is currently the intent.]

Page 13.

I'm having trouble seeing the value of the high density experiments at the more remote sites (i.e. isolated strings or strings without operational turbines), compared to the lower density experiments. The proposal notes applicability to Altamont only with the low-density experiments (page 15; 5 per string). What will results from the high density experiments be applied to?

Page 18.

Under Independent experiments, I don't understand the point of the example. How does alternating the search sequence between north and south have anything to do with the independence of the experiments?