FPL Energy

February 10, 2008

To: Alameda County

Cc: Monitoring Team
Scientific Review Committee

From: FPL Energy

Subject: Comments on *Bird Fatality Study at Altamont Pass Wind Resource Area, October 2005 to September 2007, Draft Report*

The following comments have been complied in response to the County Monitoring Team's recent summary report for the first two years of the ongoing research being conducted in the Altamont Pass Wind Resource Area.

1. p. 3, Survey Design: The discussion on p. 3 indicates that "turbines were distributed in 84 randomly selected plots stratified by geographic location and turbine size." We are concerned that the calculation methods employed throughout the report seem inconsistent with the survey design. Although not discussed on p. 3, we understand that at least three turbine size categories were censused. These classes include: very small (40-65kw installed capacity (IC)- e.g. Patterson Pass's Nordtank 65kW IC machines); small (100-150 IC- e.g. KCS 56-100's found in many areas throughout the Altamont, 100kw IC machines); medium class (≥250 kW IC- e.g. Diablo Winds' 660kW IC machines, Tres Vaqueros' Howden 330kW IC machines, and the WEG turbines- 250kW IC machines). First, we question whether the classification of the medium turbines is appropriate, given the disparity between the size and potential power output of the turbines currently grouped together. Second it is not clear that the turbine size classes were taken into account in the analyses. Also, the small (KCS 56-100's) size category was sampled based on a quasi-random selection of turbine strings (not turbines as indicated in the discussion). Implications of these issues follow:

a. Conceptual Model: A basic tenant of good statistical practice is that the methods employed for statistical inference must be consistent with the conceptual model underlying the survey design (Kalton 1983, Cochran 1977)[1]. In this case, the censused and sampled strata have differing inclusion probabilities which must be taken into account by the methods employed for statistical inference. The conceptual model, although not stated in the report, seems to be that mortality is a

[1]Kalton, G. (1983). *Introduction to Survey Sampling*. Sage Publications, Beverly Hills, CA. ; Cochran, W.G. (1977). *Sampling Techniques*, Third Edition. New York: Wiley.

function of turbine size. The survey approach and associated inclusion probabilities seem to be based on this premise, therefore, the statistical inference methods must reflect this concept. Simply stated, the data from different turbine size strata cannot be simply pooled to generate an expected mortality rate. Also, the report states that "plot" is a stratum, but the report does not address randomization within plot. We find the survey design and the randomization of sampling units confusing.

b. Calculation of an expected mortality rate: The methods employed for statistical inference should result in an expected mortality rate. The unit of this metric (i.e, the rate) must reflect the survey design (i.e., if turbine string is the sampling unit, an estimate of the expected mortality per turbine string is an appropriate statistic). The statistical methods employed in this report are not clear. For example, the equations and data used to generate the fatalities per turbine string (Table 1) are not apparent. Given that strings are not surrogates (or replicates), it is difficult to interpret the values presented in Table 1. Are the fatalities per string weighted by the number of turbines in a string?

c. Sampling unit: What is the sampling unit? The report must clarify this very important concept (see disconnect between the language on p. 3 and Appendix A). The methods employed for statistical inference must correctly incorporate the sampling unit definition, the number of units sampled, the inclusion probability associated with an individual sampling unit, and the number of replications per unit. There is little information in the report that provides us with confidence that the statistical inference equations were properly implemented.

d. Weights: The mathematical methods used to weight the resulting data are unclear. Equations should be provided, including equations for the calculation of uncertainty in the expected mortality rate. There seem to be a number of possible weights that could be employed, including the following: number of replications per sampling unit, number of turbines per turbine string (providing the sampling unit is a turbine string), amount of time turbines operate per search interval, or area surveyed per sampling unit (see discussion in Appendix A which suggests that the area surveyed per sampling unit may not be consistent for all sample units). A discussion of the choice of sampling weight, and the equations used for weighting, must be presented in detail. Also, it seems that the weighting scheme employed in this report was not applied at the level of a sampling unit for a single unit of effort, but at the level of the sum across units. If this is true, the mortality estimates will be incorrect (see above references). Also see a discussion for the proper application of weights in Sokal and Rohlf (1995 *Biometry*, W.H. Freeman and Co, New York page 133).

2. p. 5, equation 1: We have the following issues with equation 1:

a. The numerator (the unadjusted mortality rate) seems to be incorrectly calculated (see above).

b,. The denominator (R x p) assumes that the scavenging rate and observer bias are independent events and that they are inversely proportional to mortality. This is clearly not the case. Simply stated, should a bird be scavenged, it cannot then be "unobserved" by the investigator. And, should the bird be unobserved, for the purpose of our mortality estimate a future "scavenge" is not relevant. Although in practice scavenging and observer bias studies are conducted independently, from a biological perspective these factors are not independent and can greatly misrepresent the true mortality at the site.

The multiplicative terms, and the assumption that the result is inversely proportional to mortality, results in an overestimation of mortality that increases in error as observer bias and scavenging rate increases. For example, take the simple case where the observer finds 10 birds and site-specific studies show that R=0.2 and p=0.2. If R and p are independent (which they are not), than on average "at most" the number of birds unobserved is 8, and the number of birds scavenged is 8, thus resulting in an estimate of 26 birds (10 + 8 + 8). The true mortality remains unknown.

Because there is an unknown probability that the bird could be scavenged prior to observation (as well as an unknown probability that the bird is later scavenged given it is not observed), the estimate of 26 birds is overly large. In truth, the degree to which the 26 birds overestimates the true number of birds is unknown, but the 26 bird estimate is at least conservative. We note that this approach, while conservative, is consistent with the methodology and underlying assumptions involved in the observer and scavenging study protocols.

Examination of the mortality estimate for this simple problem using equation 1 shows that R x p =0.04, resulting in 250 birds. This approach assumes that R and p are independent and inversely proportional to mortality. Clearly, this equation should be closely considered prior to use.

We note that equation 1 is not widely used or referenced in the wildlife literature. We also note that a large number of differing equations have been proposed for adjusting mortality by numerous investigators. However, none of the equations we are aware of assume inverse proportionality of multiplicative effects.

Therefore, prior to adopting equation 1, we believe that a study of the published and peer-reviewed methods for mortality adjustment be conducted. As shown above, small changes in R and p result in large changes in the mortality estimate. Therefore, the selection of R and p has a disproportionate influence on the resulting mortality estimate.

In fact, we recommend that a corrected expected mortality rate be calculated both with and without any adjustment factors.

c. Given the large influence of this equation form, and the large influence associated with the selection of a specific R and p, a comparison of mortality generated using equation 1 with the baseline value of 1300 birds is not possible unless the baseline numbers are recalculated in the same manner as is used in this document.

3. equation 2, p. 5:

a. We understand that scavenger removal trials were conducted during the survey period. Therefore, we strongly suggest that the site-specific value be used to adjust mortality.

b. On p. 6, it seems that equation 2 was used under the assumption that the bird died at the end of the previous observation period (i.e., 37 or 44 days prior). This results in a maximum scavenger removal rate over the search interval. Given the sensitivity that the selection of R has on the mortality estimate resulting from equation 1(see above discussion), the calculation of R in this manner has an extreme influence on the resulting estimated mortality. If site-specific values of R are not available (the preferable approach), we suggest using the median value of the search interval in equation 2.

c. Also, note that on p. 6 the values of R for small raptors approximates the value we used in the example above. Again, given the mathematical form of equation 1, combined with the values of R on p. 6, the estimates of mortality provided in this report are likely over estimates of the true mortality.

4. p. 6, searcher detection efficiency: We suggest that searcher detection bias be studied on-site and the results used in the mortality calculations. We believe that the degree of observer bias is strongly case specific. Extrapolating estimates of p from historical studies is not generally appropriate. Again, given the sensitivity of equation 1 to small changes in p, we suggest that a site-specific and accurate estimate of p be generated by running on-site observer bias studies.

5. Tables 1 (p. 9) and Table 4 (p.15):

a. Although it is noted in the text that bones (fatalities estimated to be over 90 days old) are not included in the total recorded bird fatalities table, it is not explicit what other kinds of incidents are included in these totals. Are these birds only from within the search plot (50, 60 or 75m)? Are birds that were injured counted as fatalities? Are all feather spots included?

b. Why are the total number of birds in Table 1 (1596) and Table 4 (1804) different? We also note that the species-specific values are different between the tables.

c. The extrapolation methods seem to suggest that the probability of species presence is uniform across the Altamont. Our understanding is that this is not true, particularly for burrowing owls.

6. Stdev, Tables 1 - 3:

a. How is the Stdev calculated? Please note that a metric such as mortality per turbine string is not normally distributed, but most likely follows a lognormal distribution. Therefore, the authors should consider presenting an uncertainty estimate of the median, or the transformed log mean. Please include all equations used for the calculations in the text. What should be reported in Tables 1 -3 is the standard error of the weighted mean. The label may be wrong, or the estimating equation may be wrong, but we can not tell which is the case because the equations are not provided.

b. Also, note that the coefficient of variation (CV = std./mean) values are larger than would be expected. For example, in Table 2 the burrowing owl has a CV of 2.6. The table does not indicate the sample size (n), which provides additional confusion. But, we suspect that the number of sampling units used to generate the weighted mortality rate is large, which generates confusion over the size of the CV. Again, please provide exact equations, sample sizes, and methods used for the calculations in the tables.

7. Table 2: If the sampling unit is a turbine string, please explain how the extrapolation to individual turbines was accomplished. Statistically, extrapolation to units of smaller size than that used in the survey (i.e., a turbine string extrapolated to an individual turbine) is difficult if not impossible to defend.

8. Appendix A: are the EnXco and Diablo turbines included in the calculations presented in Tables 1- 4. If so, were the mortality estimates adjusted for the change in search area?

SUMMARY

We have the following 3 general concerns with this document:

a. The statistical methods seem inconsistent with the survey design.
b. The equations used to calculate mortality are, by mathematical definition, over estimates of the true mortality rate.
c. The report is not transparent.