Review of report,

"Developing Methods to Reduce Bird Mortality in the Altamont Pass Wind Resource Area,"

in the context of criticisms by WEST, Inc. and comments by the California Energy Commission in its draft 2005 Integrated Energy Policy Report.

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Background:

I first became acquainted with the avian research at Altamont many years ago, when I heard a presentation by consultants for wind energy companies on their avian studies at wind farms at Altamont and proposed mitigation measures. At the time, I was a staff scientist at National Audubon Society. Rather than taking any comfort from the presentation, I became alarmed that the research was of such limited quality that the proposed mitigation measures had little credibility. (I now see evidence in the Smallwood/Thelander report that my concerns were well founded.)

As I learned more and more about the avian research of the period, I formed the impression that the research was basically pubic relations-- the kind of research industry funds when it feels pressure to do something to show it cares. I worried that the research effort would end up as a stalling tactic, delaying offsite mitigation at Altamont, and delaying the general solution to avian-wind problems. All my experience with energy sources had convinced me that failure to deal with the side effects of energy sources at their initial deployment would mean that society would get stuck with bad designs. It would be a terrible loss, if renewable energy went down the same road of missed opportunities traveled by so many other energy sources.

Of great concern to me at the time was the lack of an estimate of the population impact of wind development on threatened and endangered birds, or species that might soon become threatened or endangered. I translated my alarm into a public call for a moratorium on wind development, until issues related to avian mortality were better understood.

For a representative of the National Audubon Society to call for a moratorium on wind development was to provide a new pubic relations problem for the industry and wind enthusiasts. Fortunately, the National Renewable Energy Laboratory and far-sighted members of the wind industry, the environmental community, and the regulatory community, including staff of the CEC, rose to the challenge. The National Renewable Energy Laboratory, to its credit, did not try to “hand wave” the problem away as the inevitable consequence of a shift to better energy sources. Instead, they tried to understand the problem in a scientific manner and work on solutions, just as they would have approached an engineering problem with wind.[1] Instead of continuing to rely on biologists consulting for industry, NREL began to hire their own biologist consultants to review industry research proposals, including one scientist I recommended, Dr. Michael Morrison, who had been on the staff of National Audubon for a period. As a result of NREL’s new hands-on approach, I watched the quality of avian research improve dramatically.

[1] I found the same exemplary attitude among the biomass division at NREL.

The National Wind Coordinating Committee, for its part, through the Avian-Wind Subcommittee developed a series of workshops leading up to the publication of a consensus guidance document for monitoring avian-wind interactions. As one of the participants in this process, I took a keen interest in the completion of the guidance document (Anderson et al. 1999), which took place after I left National Audubon. Along with the growing evidence from avian research that most problems with wind were localized to sites with high use by important species, the need for a moratorium dissipated, although because of the memory of the world wide web, it is impossible to escape my initial call for a moratorium.

Because of my acquaintance with Michael Morrison, I was introduced to Shawn Smallwood, one of the co-authors of the report I am today reviewing. The three of us ended up collaborating on papers, the last of which was finally published in 1999. These papers had nothing to do with avian-wind interactions. I have also recommended Smallwood to persons seeking independent biologic analysis in controversial situations—situations where the consulting scientist must be immune to political pressure and the fear of losing future contracts with industry.

Thus, I have approached the review of the Smallwood/Thelander report with the expectation that it will represent a high quality product. No two scientists agree on everything, and Smallwood and I are no exception, particularly in the area of adaptive resource management and in connection with species-management priority. As for species priority, I focus my attention on threatened and endangered species, and would support a mitigation measure that reduced kills of Golden Eagles and Burrowing Owls, even though more Red Tailed Hawks, which are very common, would be killed. I hope that any opinion of mine that differs from the report's authors will not be used as an excuse to distract policy makers from the report's value as a whole.

My data-related, scientific work that relates to the Smallwood/Thelander report and the controversy surrounding it involves both wildlife and human epidemiology (see attached curriculum vita).

As my vita indicates, I am an experienced peer reviewer of scientific reports and articles, especially those with policy implications. I am a division advisor to the National Research Council (NRC) of the National Academy of Sciences, and I regularly peer review NRC reports prepared for Federal agencies. I am also a peer reviewer for a number of scholarly journals. I have served on many study panels of the National Research Council (NRC). In all of these endeavors with the NRC, I have learned to focus on the policy implications of the science at issue, which explains why I have directed my comments on the Smallwood/Thelander report to the statements about it that appear in the CEC's 2005 draft Integrated Energy Policy Report (CEC 2005), under the section, "Repowering Wind Resources and Reducing Bird Deaths":

> "….. an extremely polarized debate has emerged between the wind industry, the Energy Commission staff and consultants, and environmentalists who believe there have been inadequate efforts to reduce the number of birds killed by wind turbines in the Altamont Pass Wind Resource Area. A focal point of that debate has been the statistical reliability of the research cited in the 2004 Energy Report Update and the subsequent use of that research by Energy Commission staff and consultants.
>
> The Energy Commission believes that the earlier research, *Developing Methods to Reduce Bird Mortality in the Altamont Pass Wind Resource Area*, represents an important initial effort to craft a methodology to prescribe mitigation measures, but that it should not be misused to form the sole basis for such mitigation measures. Inadequate access to certain turbines, time lapses between surveys, length of survey period, and various extrapolation techniques deprive it of the evidentiary value which the Energy Commission would require as the basis for mitigation measures in a power plant siting case. The scientific value of ongoing Energy Commission research into avian mortality prevention should not be jeopardized by misapplication of what are essentially experimental results. "

Thus, the three key issues for me to review are, 1) statistical reliability, 2) data completeness (in terms of turbine access and survey timing), and extrapolation techniques. I take the latter to mean predictive modeling.

The quoted passage from the CEC report contains language that sounds eerily familiar--words I have heard before in other controversies over the last 35 years in which attacks by industry on science have been carried out in polarized atmospheres. I am reminded of the fury over childhood lead exposure and the character assassination attempts made on Herbert Needleman. I am reminded of the attacks by the electric power industry on the scientific studies of acid rain. Also, similar attacks, which continue today, on the scientific studies of health effects of fine particulates. Most recently, fierce attacks have been directed at the science of global warming. How do these attacks by vested interests gain so much attention from policy makers?

Stakeholders, both public and private, want 99.99% certainty before accepting policies that they perceive will hurt them. They are naturally suspicions of statistical techniques and extrapolations that appear to work against their private or public interests. In another context, these same methods would not raise an eyebrow.

All reports have limitations. An effective advocate for a cause or a company will try to use those limitations to damn the entire report in the outsider's eyes. Such tactics are used by all sides of the political divide from time to time. For instance, it is standard practice for aggressive companies to ask for more calculations than appear in a report, to ask for alternate handling/treatment of data, and to demand that the date be split so finely into categories that statistical significance is lost due to small sample sizes. It is up to the regulators to see through these ploys. Sometimes, the noise made by advocates gets loudest when the science arrayed against them is the strongest, with a resulting undue increase in polarization. Judging a report by the polarization it engenders is a risky policy.

I have heard from all sides in these disputes words to the effect that, “if you don’t put up a fierce fight, you will get rolled over by the other side.” If an advocate fights everything to the death, the logic goes, the resulting compromise won’t be so bad. Advocates see their responsibility to the stockholders, or to the investors, or to their members, or to the larger good, depending on the stakeholder’s constituency. So, it is not surprising that stakeholders threatened by a scientific debate will “shop” for scientists who hold scientific and philosophical views compatible with the stakeholder’s perceived interest. The result is heightened scientific controversy, especially because relatively minor disagreements between scientists can get blown up into big issues when normally private discussions are held in a public fishbowl. Since each exchange is reviewed by regulators, the public, and sponsors, scientific players often feel they have to defend themselves to the spectators, which ends up sparking more exchanges and sharpened tempers.

The specialized nature of the scientific enterprise makes it easy for non-scientists to get lost in details, especially in the area of statistics. As for dealing with inferences from scientific data, it is not just outsiders who can get confused. This is hard stuff, bordering on philosophy, with many different schools of thought existing simultaneously within the scientific enterprise (Beyea and Berger 2001).

It is hard to tell as an outsider how much of this dynamic of controversy is playing out here at Altamont. With the possible exception of the Altamont controversy, I have found much less cutthroat politics in the renewable energy industry than in other industries. Even at Altamont, it doesn’t seem like WEST, Inc. (consultant to Florida Power & Light) is as far apart from Smallwood/Thelander on the science as some of the rhetoric suggests. My experience with the National Wind Coordinating Committee on site monitoring guidelines and in similar efforts to developing consensus guidelines on biomass energy to protect forests (Cook and Beyea 2000) has been positive, giving me hope that negotiated conflict resolution will be the rule in renewable energy. The hopes I have expressed for other industries in this regard (Beyea 1993) have not been borne out, but I am keeping fingers crossed for renewables.

How is a poor regulator to navigate this type of maze? In recent years, I have been impressed by the potential of "adaptive resource management" as practiced by the Fish and Wildlife Service (FWS) in its waterfowl management efforts, which have involved competing parties (Johnson et al. 2002), (Kendall 2001), (Johnson 1999), (Johnson et al. 1997), (Johnson et al. 1993). It is a model that should be more widely adopted in contested and polarized situations. For this report, I label such an approach, "contested adaptive resource management," or CARM. Recently, I facilitated and contributed to a report on managing deer from an ecosystem perspective in Pennsylvania that based its recommendations on CARM, although without using that precise acronym (Latham et al. 2005). (I suspect that deer management in Pennsylvania is more polarized than anything at Altamont and that CARM has more of a chance of being adopted in California than most places.) I recommend this report, with its treatment of CARM, as a useful supplement to the references from the FWS authors that I have cited.

I should note that the kind of ARM program I recommend has little to do with the ARM-concept defined by WEST, Inc. The WEST approach, which puts the wind companies in control, is not appropriate in my view in a contested and polarized climate.

Issues associated with the report by Smallwood and Thelander.

The report by Smallwood and Thelander represents a monumental effort in terms of data collection and an excellent start at data analysis. The report strengthens the evidence that the turbines at Altamont Pass are killing uncomfortable numbers of Golden Eagles and Burrowing Owls, which are species of special concern in California (Smallwood and Thelander 2005). The information presented in Chapter 4 provides evidence that the number of kills of some species have increased over time, raising new concerns about population viability.

These findings of the Smallwood/Thelander report are alone sufficient to justify mitigative action in my view, without going any further into the report. If the data in Smallwood/Thelander is deemed insufficient by decision makers to pick out management actions tied to individual variables, then offset mitigation is the logical choice, presumably through the purchase of easements. What reduction of kills per year should be the target? Sufficient offset would be needed so that the Hunt population analysis would comfortably indicate population viability. If purchase of easements is more expensive than shutdowns of turbines, clearly shutdowns should be pursued to obtain the necessary reductions in kills. Determination of the appropriate reduction targets would best be made through a workshop format, if this exercise has not already been undertaken. (I am unfamiliar with the reasoning that was used by Alameda County in its requirement of turbine shutdowns.)

If, on the other hand, it is deemed that the Smallwood/Thelander report is sufficiently strong to test other ways to reduce kills, e.g. at lower costs, such tests should be implemented within a framework of adaptive resource management that is modeled after the successful efforts of the Fish and Wildlife Service in waterfowl management, which for this review, as stated earlier, I have labeled, "contested ARM," or CARM. CARM allows management action to proceed in the midst of strong scientific controversy, using data from management actions to update weights assigned initially to each competing theory put forth by conflicting stakeholders. CARM is completely different from the program that WEST, Inc. calls adaptive management. Under CARM, regulators, not the wind energy companies are in control of the management proposals that get adopted and tested, along with the method of test. Regulators also serve as referees, letting all stakeholders test their proposed predictive models.

CARM would allow consultant, WEST, Inc., to use its recommended statistical technique, logistic regression, along with WEST's biological ideas to make its own predictive models. At the same time, BioResource Consultants could use their predictive models based on summing univariate Chi-Sq results, informed by consistency of behavioral observations, as identified in Chapter 8. After making an initial decision as to how much weight to assign to the theories of the contesting parties (a decision that would be self-correcting in contested ARM), policy makers could sit back and let successive field data "prove" which theory was better, adjusting the choice of successive mitigation measures to favor the most successful theories. Note that CARM requires anyone making a prediction to also present a rate of error. This rate of error (e.g., standard error) is used, as new field data comes in, to update the confidence regulators place in individual models.

But what management actions do the Smallwood/Thelander report justify at this time, prior to further data analysis? There is already sufficiently strong evidence in the reported univariate analyses for causal inference for those variables with very low p-values (< 0.005), which justifies the corresponding management recommendations being implemented and tested in a CARM framework.[2] Such recommendations might be best illuminated and fleshed out in a workshop setting. Limitations in the data due to discontinuities, gaps and measurement uncertainties are not likely to have accidentally created such strong associations.

On the other hand, at this time, basing recommendations on variables with univariate p-values that might be as high as 0.05 should not be done in my view without further data analysis, for reasons to be discussed later. I would make exceptions for variables for which consistency was found with behavioral patterns as presented in Chapter 8, because an underlying biologic basis has been added to the inference.

What should be done with those variables whose p-values are not extremely low in univariate analysis? If this were my data set, I would first attempt to tackle some of the discontinuities, gaps, and measurement uncertainties in the data using the statistical models and model combinations available within the WINBUGS Bayesian software http://www.mrc-bsu.cam.ac.uk/bugs/welcome.shtml, (Woodward 2005), (Congdon 2005). The WINBUGS software is now readily available and supported by a community of researchers. Basically, WINBUGS allows an analyst to replace uncertain parameters with distributions that are fitted to his or her data without much restriction on the functional form of the model. "Scavenging rate" would be an example of a relevant parameter, as would the "percentage of missed data." These unknowns might be fit to binomial distributions with unknown rate parameters. Seed values in WINBUGS would be the values found by other researchers. However, there are many more

[2] WEST, Inc. has stated in reviewing earlier work by Smallwood and Thelander, "It should also be clearly stated that there is confounding and correlation among variables and that can affect these apparent univariate associations. This is discussed in several sections, but should also be discussed in the Executive Summary, and Conclusions Section." [as quoted in, (Dorin 2005).] Confounding and correlation are unlikely to change inferences made about variables with very low p-values.

options in WINBUGS to generate mathematical models that are consistent with biologic understanding, including models that would deal with concerns about survey timing and turbine access. With this software, it is no longer necessary to force a biologic model into a tractable mathematical equation. WINBUGS can handle an incredibly large set of equations with unknown parameters that can be fit to data. Most importantly, WINBUGS generates corresponding confidence limits.

There is little to lose with such a Bayesian approach, because, if the data is not adequate the resulting distribution will be very broad and of little use, telling the analyst to ignore the parameter and the model component in which it is embedded. Although WINBUGS is only now being introduced into wildlife studies, it is widely used in other fields and is truly powerful, allowing parameters in complex models to be routinely extracted from data. Of course, no amount of software wizardly can overcome sample size limitations set by subdividing and stratifying data too far and looking for too many interactions.

I am not suggesting that efforts at CARM should be delayed by further data analysis. CARM can begin with those variables having very low p-values in univariate, Chi-sq analysis.

The only data analysis I would recommend immediately, if it has not already been carried out, is to rerun the main univariate analyses, with and without, the Seawest turbine strings. Such analysis would be helpful, given the controversy over the inclusion of these turbines, for which access was only recently obtained. The results might focus better attention on which areas of the Altamont complex should bear most of the mitigation expense.

As for predictive models that combine univariate terms, at this time I would only include variables with very low p-values and I would perform bootstrap analysis to get some idea of the variance around the sums.[3]

The research in this report is just the kind that I hoped would be carried out to counteract the PR-type of research that industry was carrying out, when I first got involved in this issue. The

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[3] WEST, Inc. has stated in a review of earlier work, "Conducting univariate tests is a reasonable start to developing a list of candidate variables for a 'predictive model'. The approach to combine results from univariate tests into a scoring system, that does not account for confounding of variables, correlation of variables and interaction of variables is fairly uncommon and is often criticized as data dredging." [As cited in (Dorin 2005).] I have never heard this kind of scoring system called data dredging. Furthermore, data dredging is not always a pejorative term, since one should always dredge the data at the end of a study in hopes of generating new hypotheses. As for the usefulness of the scoring system in Smallwood and Thelander: 1) For CARM, it doesn't really matter how a predictive model is generated, 2) The scoring system is checked in Smallwood/Thelander in Chapter 8 using consistency with observed bird behavior, 3) the method used by Smallwood/Thelander is easy to understand, which is important for a contested situation. Still, as I have said in the text, I would only include variables with very low p-values. Possibly, a cutoff of $p = 0.005$ is too restrictive, but that is the only low cutoff value presented by the authors in their report.

many years of data collection that make up this study is unusual in the field of wildlife management. If this report cannot form the basis for mitigation measures at the Altamont Pass, there is no real hope of any avian research ever doing so. Relegation of this report to the shelf, because industry consultants have raised questions about parts of it, will mean that all avian research at Altamont, even the impressive efforts funded and guided by the National Renewable Energy Laboratory, will have amounted to no more than a stalling tactic. What good is research that must be so pure that it can never be completed?

Every scientific study has limitations, which can be used by unhappy stakeholders in an attempt to smear the entire study in the eyes of policy makers. There is always more research that can be done to reduce management uncertainty in an attempt to reduce the fears of concerned stakeholders. Although no report should form the sole basis for mitigation policy, a decision to ignore the report completely will be to ensure that more and more years will go by without anything being done in a meaningful, adaptive-resource-management framework. Of course, the report is experimental. That was the point. At a minimum, the report establishes predictive models that can be tested in a CARM framework. The default option being recommended by the energy commissioners in their draft report is to wait for repowering over the next 13 years to replace the existing turbines with taller turbines. Although the Smallwood/Thelander report provides evidence that these turbines will kill less of certain species, nature can be very contrary to researchers. Counting on repowering to solve the problem and waiting 13 or more years to find out does not sound like a wise policy. Furthermore, the Commissioners are asking someone else, the Federal government, to pass national legislation to make repowering possible at one local region, namely Altamont. If a do-nothing decision is made by policy makers, so be it, but the shoulders of those responsible for the decision should be clearly identified, without trying to use limitations in part of a report as an excuse for making a policy tradeoff, namely more dead birds of special concern in exchange for more kilowatts from otherwise clean power.

Statistical methods:

The Chi-Sq method of looking for effects, using the Chi-Sq statistic as a measure of statistical significance, is not controversial at all.[4] It is one of the most basic techniques in

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[4] WEST, Inc. states in a review of an earlier report: “There is no discussion of the fact that the chi-square analyses conducted are based on assumptions of statistical independence of the experimental units.” [As cited in (Dorin 2005).] Putting aside the issue of experimental units used by Smallwood and Thelander, every statistical test makes certain untestable assumptions about the underlying data. Also, many statistical tests work fine outside their original domain of definition.

statistics. In fact, it is the place many analysts would start in data exploration. (I am confused, however, about the handling by the authors of cells with less than 5 counts and will need clarification on that score.)

WEST, Inc has recommended use of logistic regression. Using logistic regression for univariate analysis should not change any conclusions, except possibly at the margins of statistical significance. I happen to have been a regular user of logistic regression as part of my work over the last nine years, not as part of my work on wildlife, but in the field of human epidemiology in connection with environmental causes of breast cancer. Logistic regression is the standard statistical technique in human epidemiology. It can be very useful, but it is no panacea. So, I think concern about logistic regression is irrelevant.

Even with multivariate analysis, use of logistic regression does not in my experience change the inferences that are strong (same is true when interactions are added). In fact, if WEST is correct in saying that this situation is suited for logistic regression, the p-values should move to stronger inferences, at least, that has been the case with the data sets with which I have worked or carefully reviewed. Thus, the use of the logistic regression technique will only change conclusions, most likely, at the margins of inferential decision making, where policy makers should be nervous anyway.

One advantage of using logistic regression, however, might be to aid in the dropping of insignificant variables. At least, in the field of human epidemiology, there are simple conventions about when to drop variables that do not require any confusing PCA-type reduction schemes.[5] Whether or not WEST, Inc. would accept these conventions for wildlife studies is another matter, since the conventions in epidemiology are based on Monte Carlo simulations taken from epidemiology problems. The same simulation approach could be taken for wildlife analysis (and without restriction to logistic regression), but has not yet been done to my knowledge.

The larger question of whether or not any multivariate analysis should be used is trickier. The authors have chosen not to enter the minefield of multivariate analysis. Had they done so, the polarization around their report would likely have been greater, because of the many more options there are for judgment in multivariate analysis. (Same holds for including interaction terms.) Certainly, when univariate statistics of a decision variable are at, or around, the p=0.05

[5] The most common method is to drop any variable that, when introduced into the regressions, produces less than a 10% change in the slope of the coefficient of main interest. Another method is to drop all variables with a p-value above a certain value, say 0.2 or 0.25. Both methods have been shown to be roughly equivalent in simulated epidemiology data sets. (Mickey and Greenland 1989)

value, I would want to move to multivariate analysis, assuming p=0.05 is used as a decision trigger point. This is based on my experience with data sets of a wide range of sample sizes. If the data set is too small to justify multivariate analysis, as the authors of the report have concluded, I would not recommend policy decisions be based on findings with univariate p-values of 0.05, except in an ARM framework of reversible mitigation. (Same recommendation for considering interactions.) I should repeat that I am not a great fan of variable reduction techniques (e.g., PCA) that work with linear combinations of variables that have no biologic or physical interpretation, but that is a matter of taste. As mentioned above, I prefer the method used in human epidemiology, where "non-influential" variables are dropped based on rules of thumb derived from Monte Carlo simulations of data (Mickey and Greenland 1989).

At this stage of my review, I am not actually sure if the authors have recommended mitigative measures based on a variable with a Chi-Sq, p-value close to the p=0.05 value. Perhaps, my comments are moot, but I press on, in case my remarks are indeed relevant.

A shift to multivariate analysis might raise a p-value above p=0.05, when that variable has a univariate p-value of around p=0.05. However, a shift to multivariate analysis, whether it be linear, logistic, Poisson, or probit, is rarely going to change an inference of significance for a variable with a univariate p-value of < 0.005[6]. This fortunate situation (very low p-values) holds for almost all of the variables associated with Burrowing Owls that are identified as statistically significant in the report. Less so with Golden Eagles.

The question of interactions raised by WEST, Inc., as cited in Dorin (Dorin 2005), is a separate issue, since there are many ways to consider interaction effects. For instance, if necessary, interactions can be considered in univariate analyses by constructing new variables, followed by examination of the resulting univariate p-value to see if it is lower than the p-values of the component terms. And there are many more ways to do interaction analysis in multivariate frameworks than to restrict oneself to logistic regression. Consideration of interactions is just as

[6] WEST, Inc. states in a review of an earlier report: "Reiterating, we believe you should acknowledge the limits of the study in the executive summary and conclusions including the pseudoreplication issue, the fact the associations do not imply causation, the multiple testing issue, and the confounding of variables." [As cited in *(Dorin 2005)*.]

All these concerns largely disappear, if focus is put upon the variables with very low p-values. As for the remark about causation, experimental studies do not imply causation any more than observational studies do, once one moves outside the domain of the experiment. There are many different definitions of causation *(Beyea and Greenland 1999)*. In my view, causation in the current context is best thought of as a property of biologic and physical theories. We assume causation depending on the confidence we have in the underlying theory. In the absence of a strongly supported underlying theory, we infer a causal connection based on strength of association and the degree that the variables seem connected to a plausible underlying theory. Lots of room for disagreement among scientists, which is why science is "contested territory" at the frontiers *(Beyea and Berger 2001)*. Welcome to the avian-wind debate.

easy a task using modern software in these other methods (and the results just as difficult to interpret).

Because of the complexity of interpreting interactions, I only recommend their inclusion at this stage in the debate, when interaction effects are strong enough to be demonstrated graphically. Relying on interaction effects that can only be identified with sophisticated software raises new opportunity for controversy, unless a consensus on the approach is reached ahead of time.

How far one goes beyond univariate techniques towards multivariate analysis and interaction effects is a matter of judgment, depending on assessment of sample size, an analyst's familiarity with the data, and an analyst's statistical upbringing and preferences. Routinely, different analysts reach different conclusions about the same data and can get quite hot under the collar about the merits and demerits of different approaches. The fact is that almost every statistical technique makes implicit and/or explicit assumptions about the underlying data that cannot be verified based on the collected data or cannot be checked with reasonable use of resources. As a result, professional judgment cannot be completely removed from statistical analysis. This fact of scientific life makes it inevitable that different vested interests will end up on different sides of the statistical landscape. Policy makers need to navigate around these local fistfights and focus on the larger picture. Here the big picture is: large numbers of threatened species are being killed by turbines at Altamont, and there is evidence that mitigative measures are possible, with different strengths of evidence for each measure.

Attributable risk. At one point, the authors comment that the "number of wind turbines that the model predicted to be more dangerous to each species was many more than the number where we actually found carcasses of each species." This finding may be an artifact of assuming, as the authors implicitly do, that the various underlying causal factors are independent and not jointly interacting. It is well-known that summing attributable risk for multiple factors often sums to more than unity (Rothman and Greenland 1998). It is not possible to relate measures of attributable risk to real probabilities without assuming an underlying biologic model (Beyea and Greenland 1999), which the authors do not have (nor does anyone else). That gap does not negate use of such measures in a CARM framework as a measure of direction of effect.

Behavioral studies. I found the behavioral studies discussed in the report to be very important, because they help in building a physical/biologic model of avian-turbine interactions, which is the best way in my view to make sense out of complex data. Furthermore, the authors have supplemented their analysis in Chapter 8 with a correlation to behavior of birds (at least during visible daytime conditions.) When there is a match to the mortality results, this greatly strengthens the causal inferences that can be made. From a Bayesian perspective, it would change the probability distribution for an association, in effect, making p-values stronger. When there is not a match, however, it is not so easy to draw a negative inference, because the cause of mortality might be associated with bird behavior during conditions of limited observation, e.g., during night, fog, or rain conditions.

The p-values are so uniformly low in this chapter that it seems worthwhile, at some (hypothetical) calm point in the future, to consider interactions in the behavioral data. Unlike the mortality data, the behavioral observations apparently have lots of statistical power available for such an analysis, which means the results will not be as contestable as they would be, if the authors tried interactions with the mortality data.

Limited access to certain turbine strings. Questions have also been raised in the CEC draft energy policy report about inadequate access to certain turbine strings. Researchers were denied access until recently. From a Bayesian perspective, this implies that those who refused access to the area in question knew it to be an unusually high killer of birds. It increases the likelihood that the data is not abnormal; it increases the likelihood that kills in earlier years were greater and had recently been reduced. To assign this data zero weight would be problematic.

To start pulling data sets out of the entire data set is a problem. If it is to be done, it should be done in bootstrap fashion, where removal of all turbine strings is considered in turn. The net effect of a bootstrap analysis may be to increase the uncertainty around the inferences, but it will likely not change the implication of the inferences that are currently strong, nor even the magnitude of the key effects.

On the other hand, if even the strongest associations are all tied to the SeaWest data, then the conclusions apply to SeaWest, and very strongly so. In any case, Sea West cannot object to including the data, so that at a minimum the recommendations derived from the full data set would apply to SeaWest.

Survey timing and duration. The CEC 2005 draft energy policy report raises questions about survey duration and timing. Although unlikely to affect the association found for variables showing very low p-values, there could conceivably be a problem for associations found for other variables. I have not had time to think about this problem in detail, other than in the modeling context using WINBUGS, as alluded to earlier.

Inference from observational studies. It is not correct to say, as critics of the S/T report have said, that manipulative studies are always preferable in establishing causation, when compared to inferential observational studies (Rothman and Greenland 1998). Rarely can one manipulate all the variables one wants. Extrapolation of manipulated studies to real-world situations not specifically studied becomes problematic.

Distance of carcasses from turbines. I expect that considerable information could be extracted from this data, if collaboration were sought with NREL engineering types who would model collisions of birds with turbines. To a first approximation, the speed of the birds could be taken as zero, with the turbine providing a combined linear and rotational “impulse” to the bird, depending on the relative position with respect to the striking blade. Assuming a coefficient of restitution and a wind resistance term, both of which could be fit from the distance data, the trajectory of the carcass (or carcass parts) could be modeled in terms of simple rotating projectile motion. For instance, bird carcasses struck upwards would travel in an arc before hitting the ground. Given wind data history and an assumed height distribution of flying birds, the measured distribution of carcasses could be fit as a function of the height distribution, thereby extracting information, hopefully, about the distribution of flight patterns during collisions. It is particularly helpful for such calculations that the researchers have information on the condition of the carcasses, e.g., headless, wingless. Analysis like this may already have been considered by NREL. If rejected for reasons that I have not considered, please ignore this section.

Accelerometers. In addition to considering installation of accelerometers as discussed in the Smallwood/Thelander report, I recommend considering installation of infrared cameras on blades looking outward from the turbine center. With the field of view wide enough, relatively close encounters could be captured in numbers that would be significant, giving information on flight distributions near the turbines at night. Hopefully, bird signals would be strong enough to allow automatic searching of videotape.

Miscellaneous. I have a number of additional miscellaneous comments that are not important from a policy perspective. Many of them are editorial. I will make them directly to the authors at a later date. I note in passing that a meta-analysis of the Altamont studies might be very interesting. There is software around that takes the pain out of the computational analyses, and the authors have already done the literature review necessary for meta-analysis, and they have done related analysis in Chapter 4.

Citations.

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EDUCATION:

Ph.D., Columbia University, 1970.
B.A., Amherst College, 1962.

PROFESSIONAL EXPERIENCE:

1968 to 1970	Research Associate, Columbia University Physics Department.
1970 to 1976	Assistant Professor of Physics, Holy Cross College.
1976 to 1980	Research Staff, Ctr. for Energy & Env. Studies, Princeton Univ.
1980 to 1991	Senior Scientist, National Audubon Society, NY. NY.
1992 to 1995	Chief Scientist & Vice President, National Audubon Society, NY, NY
1996 to date	Chief Scientist, Consulting in the Public Interest, Lambertville, NJ 08530

ADVISORY ACTIVITIES & APPOINTMENTS:

Current:

-Nat. Academies of Science, Division Advisor (Division on Engineering and Physical Sciences)
-Consultant to the National Audubon Society on forestry biodiversity research
-Member, Committee on Alternatives to Indian Point, National Research Council

Past:

-Peer reviewer for Bioscience, Environmental Management, American Journal of Public Health, Environmental Health Perspectives, Environmental Toxicology and Chemistry, Atmospheric Chemistry and Physics, and various Boards of the National Research Council of the National Academy of Sciences
-Member, Technical Advisory Committee on Forest Health Monitoring, Assessment and Evaluation, New York State Department of Environmental Conservation, 2001-2002
-Nat. Research Council of the National Academy of Sciences, Committee on Alternatives for the Release of Solid Materials from Nuclear Regulatory Commission-Licensed Facilities, 2001-2002
- Nat. Research Council, Comm. on DOE'S Fine Particulate Research Program, 1999
- Member, Board on Energy and Environmental Systems, Nat. Research Council,1992-98
- Nat. Research Council, Comm. on "Linking Sci. & Tech. to Society's Environ. Goals."
- Board Member, U.S. Green Building Council, 1995-1997
- Board Member, Recycling Advisory Council, sponsored by the EPA, 1994-1996
- Composting Committee, Coalition of Northeastern Governors (co-chair) 1994-1996
- Member, Source Reduction Task Force, Coalition of Northeastern Governors 1991-1995
- Secretary of Energy's Advisory Board, Task Force on Economic Modeling, 1991
- National Research Council, Comm. on Alternative Energy R&D Strategies, 1990-1991
- Office of Technology Assessment, Advisor to various studies, 1984-1988

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"Response by the Authors to the NRC Review of 'Reducing the Hazards from Stored Spent Power-Reactor Fuel in the United States,' (Alvarez, Beyea, Janberg, Kang, Lyman, Macfarlane, Thompson, and von Hippel), Science and Global Security, 2003: 11:213-223."

"Recent developments in the scientific literature concerning radiation and disease." Report to the Public Advocate of the Nuclear Claims Tribunal of the Marshall Islands, September, 2003

"The Disposition Dilemma: Controlling the Release of Solid Materials from USNRC-Licensed Facilities," (With Richard McGee et al.), National Research Council, National Academy Press, 2002.

"The Association Between Radiation and Non-Neoplastic Thyroid Disease: a Brief Review of the Literature," Presentation at the Workshop on Targeted Screening for Thyroid and Parathyroid Disease in a Higher-Risk Population Exposed to Iodine-131, Department of Health and Human Services, Advisory Committee on Energy-Related Epidemiologic Research (ACERER), Columbia, Maryland, June 8, 2000.

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